Do Concealed Gun Permits Deter Crime? New Results from a Dynamic Model

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Abstract

Setting off an ongoing controversy, Lott and Mustard (1997) famously contended that so-called shall-issue laws (SILs) deterred violent crime. In this controversy the weapon of choice has been the differences-in-differences (DD) estimator applied to state and county panel data spanning various intervals of time. By treating violent crime as a career choice, this paper brings to bear a more general method, a cohort panel data model (CPDM) that incorporates the fundamental dynamic insights regarding entering and exiting a career. Our model distinguishes among three key parameters that jointly determine the effect of SILs on crime, (i) a direct effect on entry decisions, (ii) a surprise effect on exit decisions by individuals who entered criminal careers prior to the passage of SILs, and (iii) a selection effect on exit decisions by those who entered in the presence of SILs. We find significant and time-invariant results that reject the deterrence hypothesis as well as the DD model specification. Our results suggest that passages of SILs contribute to about one third of total violent crimes in 2011, particularly through higher turnover of violent criminals.

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1 Introduction

Shall-issue laws are state laws providing for the liberal issue of concealed gun permits analogous to getting a drivers license. Setting off a long controversy, Lott and Mustard (1997) (henceforth LM) reasoned that SILs increase the probability that a given would-be perpetrator's crime will fail because he can no longer tell which prospective victim may carry a gun and respond with threats or gun shots. In this controversy the weapon of choice has been the differences-in-differences (DD) estimator applied to state and county panel data spanning various intervals of time. Researchers have come to divergent conclusions spanning "more guns, less crime" to "more guns, more crime."

Elementary dynamic analysis highlights the possibility of three different effects of the introduction of SILs - one effect on those already vested in a life of violent crime, another effect on those teetering between entering such a life and the alternatives and, thereafter, a selection effect on the exit of those who chose to enter in the presence of SILs. With panel data on individual potential and actual violent criminals, an empirical specification to measure these effects would be straight forward. Unfortunately state (not individual) panels of crime rates for various types of violent crimes constitute the best available data.

To date the research on the impact of SILs has ignored any forward-looking behaviors and insights from analysis of the dynamics - insights such as the contemporaneous responses of existing violent criminals may differ between those who were hit with SILs after they became violent criminals and those who selected into a life of violent crime despite the presence of SILs. Rather, variations on a static DD approach have been employed, typically estimating one effect of SILs for each type of violent crime. We argue that DD estimators can be viewed as weighted sums of three effects where the weights depend on the shares of three corresponding sub-populations (potential entrants, those who were hit with SILs after they became violent criminals, and those who selected into a life of violent crime despite the presence of SILs). As the sub-populations change systematically as more time elapses since the passages of SILs, so will the DD estimates. Thus suppose because the time series lengthens as the years roll by, an early investigator applies DD to a sample period including the immediate aftermath of SILs but not a longer run and a later investigator includes many time periods long after SILs passed. Then the DD estimate of the first will tend to estimate a surprise effect (muddied by a bit of a selection effect mixed in) and the DD estimate of the second investigator will weigh the selection effect more heavily. And since these effects bear different magnitudes, the DD estimate produced by the second investigator will tend to be different from the first investigator. This sensitivity of the DD estimate to the time span of the sample period provides a setup for a long controversy!

This situation likely arose because there seemed to be no way to incorporate the basic insights into panel data on crime aggregated to state (county, city) averages. In contrast, the CPDM proposed here, while using data aggregated to the state level can, nonetheless, tease out the three separate effects dictated by almost any dynamic model. We attack the problem indirectly - first by building a model of entry and exits from careers in violent crime and wrapping up all three effects in a net entry (= entry minus exits) equation. Under appropriate assumptions we link this to the observed changes in the number of crimes at the state level, a well-measured dependent variable. In addition, we develop appropriate proxies for the relevant sub-populations of violent criminals. With these in hand, we specify a Cohort Panel Data Model and provide maximum likelihood estimators of the three different effects of SILs on violent crime rates for all violent crimes as well as the four components.

Assuming that violent crime is a career, we provide a staightforward dynamic interpretation of what we term LM's deterence hypothesis. Namely, SILs reduce the prospective value of a criminal career and also the continuation value for existing criminals. This is sufficient to sign the three effects and we strongly reject this hypothesis. We show how the CPDM nests the standard DD model thereby revealing exactly how the DD scuttles the basic implications from dynamics. Tests resoundingly reject the restrictions that reduce the CPDM to a DD model.

Our paper is related to recent work that closely examine the empirical specifications of DD. Bertrand, Duflo and Mullainathan (2004) (henceforth BDM) reviews a large set of DD papers and points out the underestimated standard errors due to serially correlated outcomes. In this paper, similar to Iyvarakul, McElroy and Staub (2011), we recognize that the point estimates are even biased in the DD specification in a large subset of the papers reviewed in BDM due to heterogeneous agents' dynamic decision making. By applying the more general CPDM to the crime setting in this paper, we show the wide application and robustness of CPDM in any setting that involves decision making of forward-looking agents.

This paper also sheds light on the controversial literature on concealed carry weapons, where almost all papers have employed variations of DD as their main statistical specification. LM was the first to use a large panel data set and essentially a DD specification, exploiting the different timing of state SIL passages, to rigorously study the effects of SILs on violent crimes. Since then, several papers have found the opposite, or facilitating effects of guns on crimes (Ayres and Donohue, 2003*b*,*a*) (henceforth AD); some have found no effects (Black and Nagin, 1998; Dezhbakhsh and Rubin, 1998); while some others have confirmed LM's findings (Plassmann and Tideman, 2001)[1]. While most of these studies make use of the same crime and law passage data set and a DD specification, they mainly differ in the lengths of their samples and various controls (time trends and demographics) used. We show that after accounting for serially correlated error terms as suggested by BDM, most of the results (those of both LM and AD) are rendered insignificant. Furthermore, the estimates vary with the size of the sample, suggesting that the DD model is a misspeciffcation. In contrast, the CPDM yields significant results that are invariant to the lengths of different sample periods (see Section 5.2).

This paper also fits in the broader literature on the economics of crimes. We construct a novel proxy for age-specific violent crime rates to study entry and exit behaviors of individual violent criminal cohorts. Similar to the economics of crimes and sociology literature (Hirschi and Gottfredson, 1983), we find consistent distributions of violent crimes across ages and further parameterized an exit function of violent criminals by age. Our results suggest that the recent liberalizations

[1] Moody and Marvell (2008) presents a more thorough literature review of the debate on SILs.

of gun laws, in addition to increasing overall violent crimes, also increased the turnover - both entry and exit - of violent criminals, effectively increasing the number of people with violent crime records, while reducing the duration of their violent criminal careers on average. Higher turnover of violent criminals has large social implications for criminal records, poverty, labor market outcomes, and etc. These results are consistent with and complement the recent work on the reasons and effects of the prison boom in the U.S. (Neal and Rick, 2014; Johnson and Raphael, 2012)[2].

Finally, our CPDM embeds a structural model of criminal discrete choices, extending Gary Becker's rational criminal framework (Becker, 1968) to the dynamic setting. Similar to the structural labor and crime literature (Wolpin, 1984; Imai and Krishna, 2004), we model individual criminals as forward-looking agents with heterogeneous propensity to commit crimes who dynamically optimize utility. However, while these papers estimate criminal behaviors with very special samples of micro data (e.g. the Philadelphia Birth Cohort Study), we believe that state panel data are more widely accessible to researchers and representative of general population and criminal population to study the overall crime patterns. Instead of solving individual-level Bellman equations, we are also able to aggregate to the cohort, state and year level for the simple estimation procedure that still captures average costs and benefits of entry and exit decisions.

The rest of the paper is organized as follows: Section 2 sets up the model, Section 3 introduces data and descriptive evidence, Section 4 describes the empirical specification in detail, Section 5 presents results and Section 6 concludes.

2 Model

This section presents a spare model that captures the essential consequences of forward looking behaviors on the part of potential and actual violent criminals in order to identify the differing effects of SILs across three sub-populations as well as the total effect. Treating violent crime as an occupation lets us capture the effects of SILs on entry into and exit from a career in violent crime in a familiar way. Potential entrants are all those who are capable but not yet criminals; potential exitors are all those who are currently violent criminals. To simplify the language, in this paper, we refer to careers in violent crimes as "careers" and use violent criminals and criminals interchangeably. We also refer to the potential entrants and exitors as the "entry cohort" and the "exit cohort" even though it is not, strictly speaking, a cohort but a stage of life.

Assume the choice governing entry is captured by a value function and those governing exit by a continuation function. The passage and presence of SILs affect both. Begin with the entry cohort. Let (s, t) denote state s in period t and let N^{En} = the number of potential entrants in (s, t).Then a familiar, straightforward reduced-form representation of decisions to enter careers in violent crime would be

[2] Johnson and Raphael (2012) also exploits the dynamics as an instrument to identify the effects of changes in incarceration rates on changes in crime rates with state panel data. We explicitly address the dynamic adjustments of criminals as well as the heterogeneity among criminals with our CPDM.

$$Entry_{st} = (\alpha_0 + \alpha_1 I_{st}^{SIL} + \epsilon_{st}^{En}) N_{st}^{En} \tag{1}$$

where $I_{st}^{SIL} = 1$ if SILs are in effect, and ϵ_{st}^{En} is a well-behaved random error to be discussed. Parameters to be estimated are the base entry rate, α_0, and the impact of SILs on entry, α_1. Note that the dependent variable $Entry_{st}$ is unobserved.

With forward looking behaviors, the contemporaneous effects of SILs on exits from careers in violent crime depend on whether this career was chosen before or after the passage of SILs. For those whose entry was prior, the passage of a SIL induces a surprise change in the continuation value of this career and consequently exit rates change by the surprise effect, denoted by β_2. In the case that the advent of SILs causes continuation values to fall, the exit rates increase and $\beta_2 > 0$, and vice versa. Use $N_{st}^{Surprised}$ to denote the size of the surprised cohort.

In contrast to the surprised cohort, those who chose their careers in violent crime after the passage of SILs presumably capitalized the effect of SILs on the value of a career in violent crime when they selected into careers of violent crime. Use $N_{st}^{Selected}$ to denote the size of this selected cohort. For if the pool of potential entrants is heterogeneous in their "quality" (proclivity for violent crime) the change in the value of the violent career path induced by SILs will affect not just the quantity of entrants as in Equation 1 but also their quality and, in turn, change their exit rate down the road. This is captured by the selection effect β_1. In the case that the advent of SILs decreases continuation values, the marginal and average violent criminal will have a higher quality, be more buffered from negative career shocks, and thus have a lower probability of exiting or $\beta_1 < 0$, and vice versa. These effects are captured in the reduced form exit equations,

$$Exit_{st}^{Selected} = (\beta_0 + \beta_1 I_{st}^{SIL} + \epsilon_{st}^{Ex}) N_{st}^{Selected} \tag{2}$$

$$Exit_{st}^{Surprised} = (\beta_0 + \beta_2 I_{st}^{SIL} + \epsilon_{st}^{Ex}) N_{st}^{Surprised} \tag{3}$$

Thus, as shown below, in contrast to diff-in-diff specifications, this enables the CPDM to explain turning points in criminal activity and not just either upswings or downturns. Finally, subtracting exits from entrances gives the net increase in criminals,

$$\begin{aligned} NetEntry_{st} =& (\alpha_0 + \alpha_1 I_{st}^{SIL} + \epsilon_{st}^{En}) N_{st}^{En} \\ & - (\beta_0 + \beta_1 I_{st}^{SIL} + \epsilon_{st}^{Ex}) N_{st}^{Selected} \\ & - (\beta_0 + \beta_2 I_{st}^{SIL} + \epsilon_{st}^{Ex}) N_{st}^{Surprised} \\ =& (\alpha_0 + \alpha_1 I_{st}^{SIL}) N_{st}^{En} - (\beta_0 + \beta_1 I_{st}^{SIL}) N_{st}^{Selected} - (\beta_0 + \beta_2 I_{st}^{SIL}) N_{st}^{Surprised} + \epsilon_{st} \end{aligned} \tag{4}$$

where the error $\epsilon_{st} = \epsilon_{st}^{En} N_{st}^{En} - \epsilon_{st}^{Ex} N_{st}^{Selected} - \epsilon_{st}^{Ex} N_{st}^{Surprised}$ is mean zero, heteroskedastic, and can be written as $\sigma^2 = \left[(N_{st}^{En})^2 \pi + (N_{st}^{Selected})^2 + (N_{st}^{Surprised})^2 \right] \sigma_{Ex}^2$, where $\pi = \frac{\sigma_{En}^2}{\sigma_{Ex}^2}$ is a parameter to be estimated. Should $Var(\epsilon_{st}^{En}) = Var(\epsilon_{st}^{Ex})$, then $\sigma^2 = Var(\epsilon_{st}^{En})$ and the variance is homoskedastic.

Equation 4 is the basic model for the CPDM. Later in the empirical work, we investigate the effect of floodgate and aging effects to this model. Our approach highlights the importance of three separate effects of SILs: α_1 a direct effect on entry of youths into violent criminal careers and β_1 the subsequent selection effect on their exits; and β_2 the surprise effect on cohorts of older criminals who began their careers prior to SILs. Further, these three parameters capture the two fundamental implications of dynamic analysis. These are (i) the impact of SILs on behaviors are not symmetric between potential entrants and exitors (youths in their entry windows and violent criminals) - roughly, the α's are not equal to the corresponding β's; and (ii) the impact of SILs on exits from violent criminal careers differs between those who began their careers before the advent of SILs and those who began after - $\beta_1 \neq \beta_2$.

Given ideal panel data on individuals, we could observe entries and exits of potential and actual criminals and form subsamples of criminals according to whether their entry preceeded or post-dated the advent of SILs. Then the strategy would be to estimate each of these three separate effects - using something like diff-in-diff - on the corresponding three sub-samples. In reality such data are not on the visible horizon. Unlike other occupations, the pool of criminals as well as their entries and exits go unobserved. The panel data we do have are aggregated to the state (or county or city) level and, of course, do not parse out the criminal population, much less record entry dates. Thus a three-separate-regression estimation strategy for state panel data that parallels that for micro panel data is precluded. In particular, this strategy is precluded because the crime rates (dependent variables) available are for the entire state population, not for the three key sub-populations. The point of using the cohort panel data model is that, despite observing only the impact of SILs on violent crimes aggregated to the state level, nonetheless the CPDM provides a way to identify the three fundamental dynamic effects of SILs - α_1, β_1, and β_2.

2.1 Implications

It is worth pausing to create a sketch of the model as contained in Table 1. The first two blocks in Table 1 show the contribution of each cohort (entry, selected and surprised) to the aggregate net entry rate with the second and last columns giving these contributions before and after SILs, respectively. In the third block of rows, weighting each row by its share and then subracting exits from entries gives the net entry rate before and after SILs. N_{st}^{Ex} is the number of all potential exitors. Finally weighting the second and last share-weighted column total net entries by $(1 - I_{st}^{SIL})$ and I_{st}^{SIL} gives the desired net entry rate for each (s, t) in the last block. Note that the expression in the last block is the same with Equation 4.

We use this table to lay out, in turn, the evolution of the crime rate over time, the implications of the deterence hypothesis, the nesting and testing diff-in-diff specifications as special cases of the CPDM.

Table 1: Effects of SILs on Criminal Careers

Cohorts	Before SIL $I_{st}^{SIL}=0$	After SIL $I_{st}^{SIL}=1$
Entry N_{st}^{En}	α_0	$\alpha_0+\alpha_1$
Exit		
$N_{st}^{Selected}$	β_0	$\beta_0+\beta_1$
$N_{st}^{Surprised}$	β_0	$\beta_0+\beta_2$
Net Entry $N_{st}^{Ex}=N_{st}^{Selected}+N_{st}^{Surprised}$	$\alpha_0 N_{st}^{En}-\beta_0 N_{st}^{Ex}$	$(\alpha_0+\alpha_1)N_{st}^{En}-\beta_0 N_{st}^{Ex}$ $-\beta_1 N_{st}^{Selected}-\beta_2 N_{st}^{Surprised}$

$$\alpha_0 N_{st}^{En}+\alpha_1 I_{st}^{SIL}N_{st}^{En}-\beta_0 N_{st}^{Ex}-\beta_1 I_{st}^{SIL}N_{st}^{Selected}-\beta_2 I_{st}^{SIL}N_{st}^{Surprised}$$

Notes: breakdown of the CPDM into entry and exit, before and after SIL. Multiplying cohort sizes in column 1 with average effects in columns 2 & 3 yields the respective contributions of each cohort to the total effect of SILs on criminal careers. Summing across rows then gives the total effect, or equivalently, our CPDM.

2.1.1 Evolutions of Criminal Cohorts

Under the CPDM, how would passages of SILs affect crime rates? As Equation 4 and Table 1 show, the obvious effects are captured by α_1, $\beta_1$and β_2 that affect entry and exit of the corresponding sub-populations. We turn to how the size and share of each sub-population evolve over time.

First set aside the entry cohort and presume it is exogenous (i.e., fertility is independent of SILs). Divide the selected ($N_{st}^{Selected}$) and surprised ($N_{st}^{Surprised}$) cohorts by the total exit cohort (N_{st}^{Ex}) so they sum to one, $s_{st}^{*Selected}+s_{st}^{*Surprised}=1$. Prior to SILs, crime evolves according the pre-SIL entry and exit rates as they hit the associated entry and exit cohorts. Further, note that as of the period when SILs become effective (t^*), essentially all criminals would have entered before this. Thus in t^* none of the stock of criminals were selected into crime under SILs so that $s_{st^*}^{*Selected}=0$ and thus $s_{st^*}^{*Surprised}=1$. This contrasts with the long run here defined as beginning when the last survivor in the surprised cohort retires or exits (t^{**}) . By then the cohort shares have reversed: $s_{st^*}^{*Selected}=1$ and $s_{st^*}^{*Surprised}=0$ and they remain there going forward. Most importantly, for t in between t^* and t^{**}, the shares evolve systematically with $s_{st^*}^{*Selected}$ growing (approaching 1) at the expense of $s_{st^*}^{*Surprised}$ (approaching 0). These shares are the weights on the selection and surprise effects. Hence, the impact of these effects on crime rates go from the surprise effect (β_2) dominating in the immediate aftermath of the passage of SILs, then fading as these older criminals exit and the fraction selected into crime grows until, in the long run, only the selection effect of SILs remains. These trends are summarized in Table 2.

Table 2: Evolutions of Criminal Cohorts

Impacts on Criminal Cohorts	Before Passage Old Equilibrium	At Passage	After Passage Transition Years	New Equilibrium
	$t < t^*$	$t = t^*$	$t^* < t < t^{**}$	$t \geq t^{**}$
$s_{st^*}^{*Surprised}\beta_2$	0	β_2	$0 < s_{st}^{*Surprised}\beta_2 < \beta_2$	0
$s_{st^*}^{*Selected}\beta_1$	0	0	$0 < s_{st}^{*Selected}\beta_1 < \beta_1$	β_1

Notes: exit cohort sizes and contributions to the total effect over time. Cohorts are normalized by the total exit cohort size N_{st}^{Ex}.

2.1.2 The Deterrence Hypothesis

LM's deterrence hypothesis has a natural interpretation in terms of the CPDM. Recall the channel they envisioned was that in the presence of concealed guns born by law-abiding citizens, violent criminals faced lower payoffs in the form of increased risk from their intended victim because they can no longer tell which victims are unarmed and which not. This translated into our CPDM model as lowering the value of entering a career in violent crime and also lowering the continuation value for those who are already criminals. Consequently, we interpret the deterrence hypothesis as implying that SILs reduce entry via lowering the career value, i.e., $\alpha_1 < 0$. Also, thereafter, those who select into crime are fewer in number but more hardcore than otherwise, i.e., $\beta_1 < 0$. Finally, and this likely gets closest to what LM had in mind: the advent of SILs is a negative surprise for the continuation value for current criminlas and they exit at higher rates than otherwise, i.e., $\beta_2 > 0$.

2.1.3 Nesting DD in CPDM

To show that the CPDM nests the basic DD we rerturn to the two basic insights from a dynamic model of entry and exit into crime. These are (i) differential impacts of SILs between potential entrants and exitors (youths in their entry windows and violent criminals) - roughly, the α's are not equal to the corresponding β's; and (ii) the impact of SILs on criminals' exits by those who began their careers before and after the advent of SIL are not equal, i.e., $\beta_1 \neq \beta_2$. It is exactly the denial of these insights that reduces the CPDM to the DD estimators.

Let us impose these in turn on the specification of the CPDM in Equation 4. First deny insight (ii) by imposing the restriction that those who became criminals before and after the advent of SIL exhibit the same contemporaneous responses to the presence of SILs, or $\beta_1 = \beta_2 = \beta_*$, a common value. In that case Equation 4 becomes

$$NetEntry_{st} = (\alpha_0 + \alpha_1 I_{st}^{SIL})N_{st}^{En} - (\beta_0 + \beta_* I_{st}^{SIL})N_{st}^{Ex} + \epsilon_{st} \tag{5}$$

Then further deny insight (i) by imposing that the contemporaneous impact of SILs on the crime rate is the same for potential entrants as for criminals, or $\alpha_0 = -\beta_0$ and $\alpha_1 = -\beta_*$. Equation

5 is reduced to

$$NetEntry_{st} = \alpha_0 N_{st} + \alpha_1 I_{st}^{SIL} N_{st} + \epsilon_{st} \tag{6}$$

where $N_{st} = N_{st}^{En} + N_{st}^{Ex}$ is the total relevant population at risk to contribute to the net change in the number of criminals. Equation 6 is then the familiar DD form and is, as everyone knows, completely static.

3 Data and Descriptive Evidence

We draw from several sources of data in this paper in order to build up the cohorts in the CPDM and to overcome data difficulties in traditional studies of crimes.

To construct the basic dependent variables (violent crimes), we follow the literature and obtain data from the Uniform Crime Report (UCR) maintained by the Federal Bureau of Investigation (FBI). The UCR data starts from 1977, as used in LM, but we focus on the period 1980-2011 due to other data constraints (BJS, see below). UCR reports violent crime and arrest rates at the state-year level in five categories: (1) murder and nonnegligent manslaughter, (2) forcible rape, (3) robbery, (4) aggravated assault, and (5) total violent crimes. Crime rates are used to construct dependent variables in our empirical specification, while state-level arrest rates are proxies for state police enforcement intensities, as is often used in the literature. Demographic control variables are obtained through the Regional Economic Information System (REIS) of the Bureau of Economic Analysis (BEA). These variables include real per capita personal income, income maintenance, unemployment insurance, and retirement payment for people older than 65 on the state-year level and are again broadly used in this literature to control for state-level income and welfare conditions over time. Table 3 summarizes these crime and control variables.

We obtain single-age population estimates from the Census on the state-year level to construct age-specific entry cohorts in our model. For more homogeneous effects, we focus only on the male population in this paper[3].

There has also been controversy over the exact years of passage of SILs in several states in the literature. We conduct our independent research in the SIL passage years in all states and show them in Appendix B.1. Our coding of the passage years is aligned with AD and extends it 2011. We plot in Figure 1 these SIL passages over time. The upward trended line over the three decades suggests explosive increases in the number of SIL states from 5 to 41. By 2011, 41 states have SILs in place and 36 of these were passed during our sample period 1980-2011. Many states have been persuaded to adopt SILs by political lobbyists as well as strong academic influence (e.g. LM), corroborating the importance to understand effects of SILs. We also identify the causal effects of SILs by exploiting the variations in the timing of state adoptions.

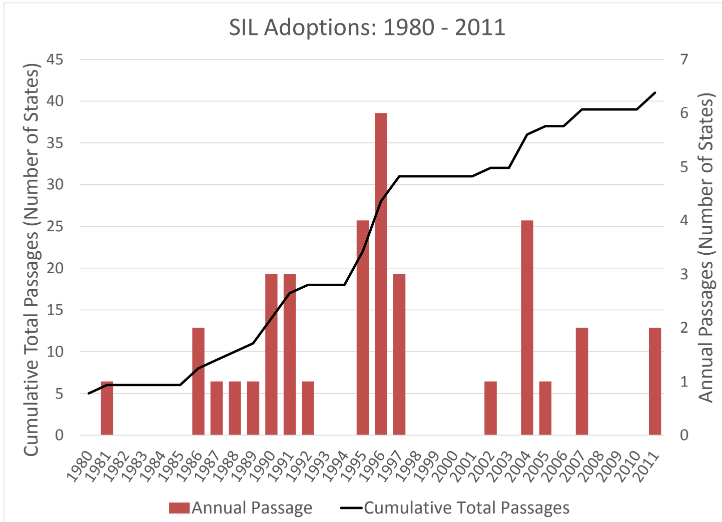
[3]Violent crimes reported to be committed by females are far less than those by males and are likely to be different in nature.

Table 3: Main Sample Summary Statistics: 51 States, 1980-2011

	Mean	SD	Min	Max	N
Crime Rates					
(Crimes/100,000 pop.)					
Violent	480.21	308.17	47.01	2921.80	1632
Murder	6.69	6.95	0.16	80.60	1632
Rape	35.08	13.42	7.30	102.18	1632
Robbery	145.30	151.26	6.40	1635.06	1632
Agg. Assault	293.13	171.62	31.32	1557.61	1632
Arrest Rates					
(Arrests/100,000 pop.)					
Violent	167.40	109.95	3.13	1313.82	1600
Murder	5.14	4.97	0	52.00	1599
Rape	10.41	6.48	0	92.49	1598
Robbery	35.98	44.75	0.16	1251.85	1597
Agg. Assault	116.06	74.70	2.78	656.23	1600
Control Variables					
State Pop. (M)	5.24	5.83	0.41	37.69	1632
Pop. Density ($pp/mile^2$)	313.25	1191.56	0.62	9306.41	1632
Inc. Mainten. ($)	404.46	179.72	104.26	1282.19	1632
Income ($000s)	28.87	7.01	15.01	64.88	1632
Unemploy. Insur. ($)	142.92	103.09	18.86	780.47	1632
Retire. Pay. ($000s)	3.53	1.13	1.18	7.00	1632

Notes: Crime type definitions - murder and nonnegligent manslaughter is defined as the willful (nonnegligent) killing of one human being by another; rape is defined as the carnal knowledge of a female forcibly and against her will; robbery is defined as the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear; aggravated assault is defined as an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury.

Figure 1: SIL Adoptions Trend



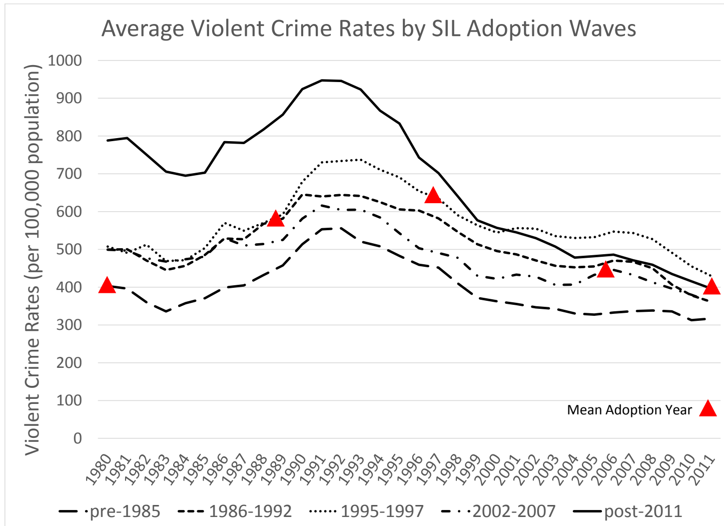
Notes: Bars indicate the number of SIL passages in each year (right axis) and the line shows the total number of SIL states so far (left axis).

It is well known that U.S. crime rates peaked shortly after 1990 and have been falling rather smoothly ever since. Also, our CPDM with $\beta_2 < 0$ and $\beta_1 > 0$ can explain an upswing followed by a downturn in the crime rate. This does not, nonetheless, make the CPDM a good candidate for explaining the national peak in crimes in the early 1990's. This can be seen in Figure 2. There states are partitioned into five groups with the states within a group all adopting SILs about the same time[4]. The first group of states adopted SILs prior to 1985 or have always had equivalent laws as SIL and the last group includes states that adopted SILs in 2011 or never adopted SIL by 2011. If the swings were all explained by the CPDM model, the peak crime rates for each group would all occur some years after that group adopted SILs and Figure 2 would have a series of humps whose max moves to the right as adoption years become more recent. But that is not the case. Instead, Figure 2 shows that for all groups, crime rates peak around 1990. Thus the CPDM for SILs could explain deviations from the overwhelming national peak in the early 1990's. But it is an unlikely candidate for explaing the huge national swing. On the other hand, it is important to control for non-linear time trends in the empirical specification.

Importantly, the patterns in Figure 2 argue against the endogeneity of SILs. For example, the group of states with the second lowest crime rate was the last group to pass SILs while the group with the lowest crime rate was the earliest. In short Figure 2 gives no reason to suspect that high (or low) crime rates cause states to pass SILs.

[4]See Figure 1 of Ayres and Donohue (2003*b*) for comparison. We follow them for this categorization but extend it into a longer panel and finer groupings.

Figure 2: Violent Crime Rates by SIL Passage Years



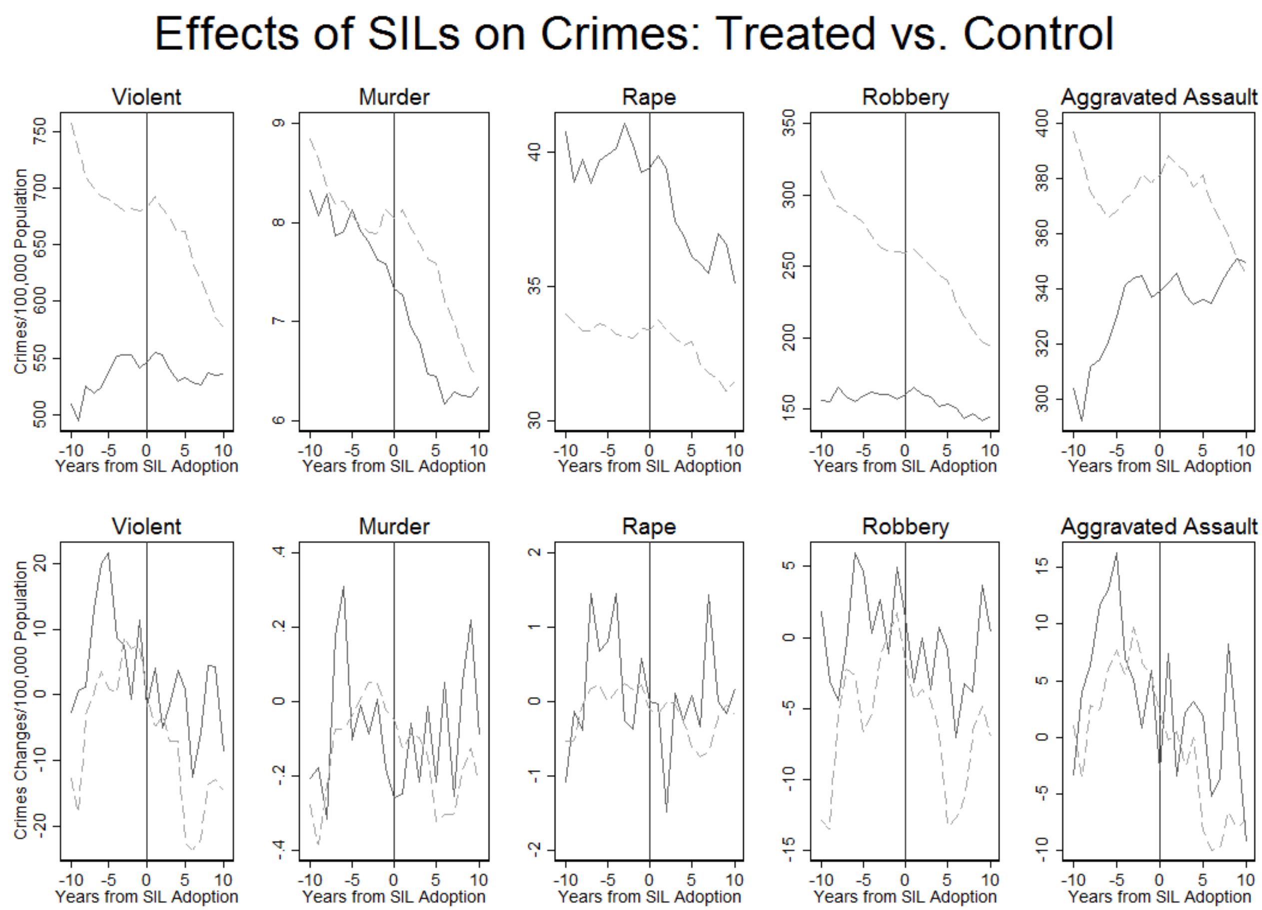
Notes: Total violent crime rates are averaged across states (weighted by state population) within same waves and plotted over time. The solid triangles indicate the average SIL passage year of the group.

To visualize the effects of SILs on violent crimes estimated from a typical DD specification, we compare average crime rates of the treated states vs. the non-treated states. The multiple treatment dates (16 unique years for the 36 states that adopted SILs within our sample) make it difficult to present the treatment and control groups graphically using the standard multiple-event DD as in Equation 6. We follow Gormley and Matsa (2011) here[5] - define a 20-year window around each treatment date t_* (normalizing t_* to zero), use all states who never adopted SILs within the window as the control group and states that exactly adopted SIL in t_* as the treated group, and call the two groups together a cohort. Then we average across cohorts for the overall treated and control groups. The results are shown in Figure 3. In the top row, we plot the levels of crime rates for each crime category, where solid lines are for the treated group and dashed lines for the control group. We find only ambiguous evidence of the effect of SILs - already showing evidence against LM and AD. In particular, the declining crime rates (or in some cases, the "inverted-V" shape) of the treated group cannot be used as evidence for the deterrence hypothesis in comparison to the control group. In the second row, we plot the same for the changes (or net entry) in each crime category. Visually, a small positive effect of SILs can be detected in the treated group compared with the control - the DD is able to capture the more nuanced effect when specified on the changes, while still leaving much dynamics to be explained.

The final data set we use is the national arrests by age groups data from the Bureau of Justice

[5] In the rest of the paper, we use the standard multiple-event DD as our DD specification for estimations but only use the Gormley and Matsa (2011) procedure here for graphically comparing the treated and the control.

Figure 3: Treatment Effects of SILs



Notes: solid lines represent crime rates for the treated group. Dash lines represent crime rates for the control group. Treated and control groups are constructed similar to Gormley and Matsa (2011). X-axis indicates years from SIL adoption, centered at the adoption year. Y-axis indicates the number of (changes of) crimes per 100,000 population.

Statistics (BJS). The BJS arrests data differs from the UCR arrests data in that it reports arrest rates on the age group-year level for each crime category. It covers the period 1980-2011 and reports in 17 age groups: 9 or younger, 10-12, 13-14, 15, 16, 17, 18-20, 21-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, and 65 or older. Together with the UCR data, we then impute age-specific arrest and crime rates, the lack of which is a traditional data problem in studies of crimes, due to the nature of crime reporting (see Appendix B.2 for the imputation procedure).

4 Empirical Specification

In this section we turn back to the CPDM and bring it to the data. We lay out an empirical strategy here to construct the relevant cohorts and to estimate the CPDM parameters.

Recall our CPDM in Equation 4 and Table 1 - unfortunatley we do not observe the dependent variable in Equation 4. The link between the unobserved number of new criminals and the observed net increase in crimes is $\kappa_{ast} = \frac{crimes}{criminals}$. Multiplying Equation 4 through by κ_{ast} converts the criminals dependent variable to the change in crime rate. Ideally, we would know both components of the change in crime rates, κ_{ast} and $\Delta criminals_{st}$. But given the impracticality of a large representative panel on the number of criminals, this seems, at best, beyond the visible horizon. The simplest practical assumption is that κ is constant across all criminals. In that case, multiplying through Equation 4 by κ converts the dependent variable to the observe change in the crime rate and changes the interpretation of the coefficients. Thus, the parameters to be estimated become $\alpha_i' = \kappa\alpha_i$ in place of α_i and $\beta_i' = \kappa\beta_i$ in place of β_i. Thus, α_0' the baseline new crimes/year attributed to the entry cohort, α_1' the change in these crimes due to SILs, and so forth. Note that the percent increase in entry rate due to SILs is identified as $\frac{\alpha_1-\alpha_0}{\alpha_0} = \frac{\alpha_1'-\alpha_0'}{\alpha_0'}$ because the κ's cancel and the analogous result holds for β_1 and β_2.

In an abuse of notation we re-use the α's and β's and write the basic CPDM for crime rates as

$$NetEntry_{st} = \alpha_0 N_{st}^{En} + \alpha_1 I_{st}^{SIL} N_{st}^{En} - \beta_0 N_{st}^{Ex} - \beta_1 I_{st}^{SIL} N_{st}^{Selected} - \beta_2 I_{st}^{SIL} N_{st}^{Surprised} \quad (7)$$

The model above assumes that $\kappa_{ast} = \frac{crimes}{criminals}$ is constant for all criminals at all ages and in every (s, t). We have nothing to add to the usual discussions of holding such parameters constant every s, but need to deal with the obvious fact that intensity κ varies across ages and in response to SILs. Our dependent variable is the time-differenced rate, $\frac{NumberOfCrimes}{Population}$. In turn the number of crimes is the number of criminals times the average $\frac{crimes}{criminals}$. Data limitations preclude parsing out changes in this intensity between changes in the components. If data permitted, we could pursue a more complex model that distinguished, for example, the effects of SILs on the numbers of entrants and their average intensity κ. But, it is not hard to see that such a dynamic model would predict that either both effects are positive or both effects are negative and our entry parameter α_1 measures the combination of these two. Hence, although we refer to "entries and exits of criminals," a more accurate descriptor would be "increases and decreases in the crime level." We prefer, however, "entries and exits" because it constantly reminds us of the dynamic decision making underpining our model.

In constructing the cohorts, the entry cohorts are ideally composed of all capable (reasonable ages, discussed below) males that are not violent criminals[6] already. Since the number of violent criminals at a time in a state is unobservable to us and is relatively small compared to the total population (violent crimes / total population are 0.48% on average), we simply use the male populations as the entry cohorts. Exit cohorts, on the other hand, are even harder to construct. Criminal populations are obviously unobservable. Much of the crime literature suffer from this unavoidable data difficulty and in this paper we try to remedy it using proxies. Older males' population is a potential candidate to proxy for the exit cohorts but it lacks correlation with the actual criminal cohorts and variation from the entry cohorts (perfectly colinear when weighted by total male population). Crime rates are better proxies for the exit cohorts if we believe that criminals across different states, years, and ages commit similar number of crimes. The only remaining issue is that the UCR crimes data only vary at the state-year level and we need age-specific exit cohorts to identify the selection and surprise effects. We thus supplement the UCR crime rates data with the BJS age-specific arrests data to impute the age-specific crimes[7] (Appendix B.2).

Now before we can specify the entry and exit cohorts, we need one more piece of information (assumption in this case). Remember that we needed the age-specific crimes data to construct the variables $N_{st}^{Selected}$ and $N_{st}^{Surprised}$ for the identification of the selection and surprise effects. The reason is that we need to know which age groups entered when to categorize them into young and old cohorts (see below for specific procedures). To do so, we opt for a parsimonious specification[8] in which we define entry and exit windows. Figure 10 (Appendix B.2) suggests that violent crimes peak around age 20, across types of crime and time. Classic sociology theory, discussed in Hirschi and Gottfredson (1983), also confirms that the age distribution of criminals does not vary across times, places, or types of crime[9]. We thus define our entry-only window to be age 13-21, and exit-only window 22-64. The cutoffs of these windows are also empirically informed, beyond what the theory suggests. The age range 13-64 covers, on average, 98% of the crimes committed in a given state-year and allows for easier parametrization (constant entry rate and quadratic exit rates, see below)[10]. The age 21 that divides our entry and exit windows is picked out by maximizing the log-likelihood of the estimated baseline CPDM (see below).

[6]We loosely define violent criminals as anyone who has comitted at least one of the four types of violent crimes in a year.

[7]The imputation procedure and the use of proxy variables will likely introduce measurement errors, which we assume to be uncorrelated with the regressors, as typically done in this literature.

[8]An alternative is to specify a nonlinear probability model to figure out the proportions of people of different entry dates within age groups.

[9]This suggests that any legislation differences and changes would impact the whole distribution of ages similarly. In Figure 10, we observe that the far tails (beyond age 40) of the distributions become fatter over time (from left to right), suggesting an aging criminal population. However, the distributions still peak around age 20 and thus do not affect our choice of entry and exit windows.

[10]This is also the reason why we do not allow overlapping entry and exit windows. The relatively narrow entry window of 13-21 allows for a plausibly constant entry rate but the variations in young male population do not pick up all entry variations. Thus allowing exit in the same region would severely bias the exit parametrization.

Part of the main contribution of this paper is to capture the heterogeneous treatment effects of SILs due to the dynamically optimizing behaviors of different cohorts. The selected and surprised cohorts in the model thus tease these effects (selection and surprise) apart from the base exit rate. We define the selected and surprised cohorts as follows. With age-specific crimes (or criminals, as proxied for), an age cohort belongs to the selected cohort if the entirety of its entry window (13-21) is spent after the SIL passage in that state. Similarly, an age cohort is part of the surprised cohort if the entirety of its entry window is spent before the SIL passage in that state. For age cohorts that experience SIL passage during their entry windows, we divide the cohort by weights corresponding to the number of years within their entry windows before and after SIL passage[11].

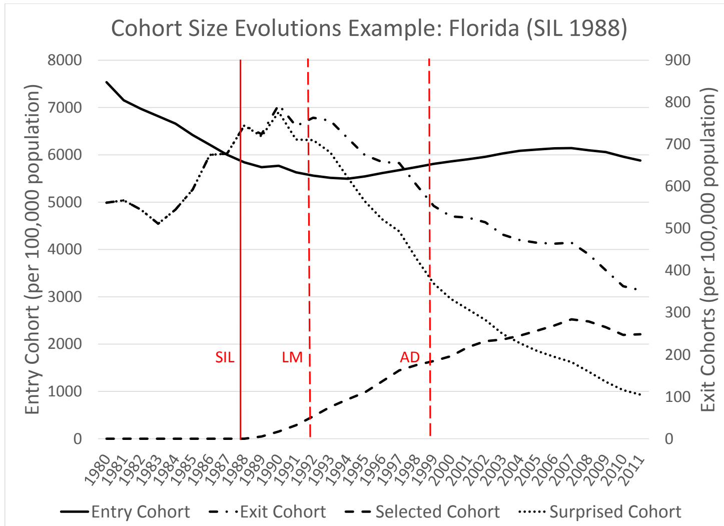
Key to our identification of CPDM is the difference in the evolution of different cohorts over time after the passage of SILs. Expanding on AD's case studies on the populous state Florida, where SIL went into effect in 1988, we illustrate these evolutions in Figure 4. The entry cohort measures male population between 13 and 21 and is relatively stable and exogenous to the SIL passage. The total exit cohort (of violent criminals) measures the current stock of violent criminals and thus fluctuates with violent crime rates and exhibits the "inverted-V" shape following the national pattern. The exit cohort is further divided into the surprised and the selected cohorts after the adoption of SIL. As time goes by, the selected cohort converges again to the total exit cohort while the surprised cohort disappears as the violent criminal stock is replaced with entrants from the post-SIL era. A new equilibrium establishes as the selected cohort coincides with the total exit cohort. In Figure 4 we also show the lengths of samples used in LM and AD. In examples like Florida, where SIL is adopted before 1992, LM's sample weighs more on the surprise effect in a DD model while AD's sample weighs more on the selection effect. We show in Section 5.2 that in the full national sample, given gradual passages of SILs among different states, DD is biased by the changing weights of surprise and selection effects while CPDM tease them apart consistently.

Figure 5, on the other hand, shows the evolutions of the average ages of the different cohorts. While the overall entry and exit cohorts stay relatively constant in age, the surprised cohort on average grows in age over time due to the lack of replenishment of new entries and will eventually all reach retirement age. The selected cohort also on average grows in age due to the initial aging of its constituents but will be balanced out by new entries and converge to the total exit cohort around age 34 when the surprised cohort dies out. The differences and changes in average ages across cohorts and time pose an challenge to the identification of the selection and surprise effects in our model, which we now turn to address.

Much of this paper is concerned with capturing the heterogeneity in cohorts as defined by the timing of their entries into crimes and the passage of SILs. However, there is another dimension of heterogeneity, intertwined with our cohorts definition, which we have so far ignored - the heterogeneity in ages. People of different ages have different physical conditions (important for committing violent crimes), have accumulated different levels of human capital (either human cap-

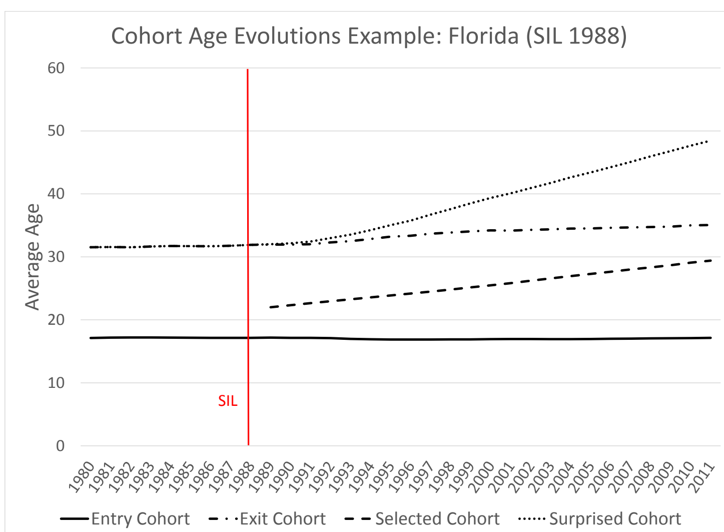
[11] This is internally consistent in the model when we estimate a constant entry rate. See more discussion below on aging effects and non-constant entry rates.

Figure 4: Illustration of Cohort Size Evolutions



Notes: the entry cohort is measured in 100,000 population on the left axis. Exit cohorts are measured in 100,000 population on the right axis. The solid vertical line indicates SIL passage in Florida in 1988. The vertical dashed lines indicate where LM and AD's samples end, respectively.

Figure 5: Illustration of Average Cohort Age Evolutions



Notes: evolutions of the average age in each cohort. The solid vertical line indicates SIL passage in Florida in 1988.

ital in the crime career that results in different skills or human capital outside crimes that results in different values of outside options), have different lengths of potential career left until retirement in crimes (important if we think that people dynamically optimize in choosing their careers), and etc.

In theory, these competing forces over the life cycle likely result in a non-linear base exit probability (irrelevant to the passage of SILs) that bottoms out in male criminals' 30's or 40's. Ignoring this crucial fact (and only estimating a constant exit rate) will bias estimates for selection and surprise effects in our model due to their differences and evolutions in ages. To fit the exit rate over the life cycle empirically, in Figure 6, we plot an empirical distribution of exit rates derived from the imputed age-specific crime rates. Specifically, we compute the fraction changes from crime age cohort a in year t to crime age cohort $a+1$ in year $t+1$ in total violent crimes averaged over all state-years and plot them against age. The positive region in the left panel indicates net entry and confirms our choice of entry window again[12]. The right panel suggests that the exit rate for total violent crimes averages about 8% (without controlling for anything), bottoms out in the early 30's, and increases until retirement. Therefore, Figure 6 presents empirical evidence for not only our choice of the entry window but also the functional form we use to parametrize the aging effects on base exit rates.

We thus parametrize the average base exit rate β_0 as a quadratic function in age as follows[13],

$$\beta_0 N_{st}^{Ex} = \gamma_0 \sum_{a=22}^{64} N_{ast}^{Ex} + \gamma_1 \sum_{a=22}^{64} a N_{ast}^{Ex} + \gamma_2 \sum_{a=22}^{64} a^2 N_{ast}^{Ex} \tag{8}$$

We then estimate (γ_0, γ_1, γ_2) in place of β_0 in the CPDM with aging effects.

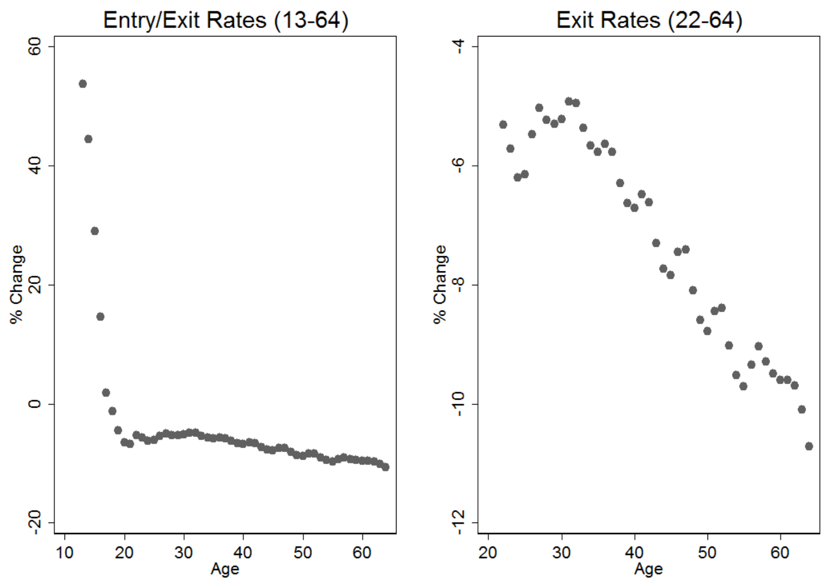
In a dynamic model, the surprise effect only measures the average change in exit rates among the surprised cohort. However, with heterogeneity in proclivity for crime, remaining careers till retirement, etc., the marginal criminals are to be surprised first, with less incumbent criminals to be surprised as time passes after the SIL passage. We therefore expect the surprise effect to be most salient in the immediate years following passages of SILs and to gradually taper off over time. We thus non-parametrically decompose the surprise effects into several floodgate effects over the years succeeding the passage of SILs. Specifically, we let $\beta_2 = \sum_{j=0}^{9+} \lambda_j I_{st}^j$, where I_{st}^j are dummies indicating the j^{th} year after SIL passage. λ_j's then represent the evolution of the surprise effects after the initial passages of SILs.

Combining everything discussed above and building upon the baseline CPDM equation, we arrive at the following estimating equation for CPDM with both aging and floodgate effects.

[12]These fraction changes do not reflect entry probability since the denominators are current criminals but not potential entrants. The graph, however, does suggest aging effects on entry as well but specifying a non-constant entry rate will result in non-lineariry of the model in differentiating selected from surprised cohorts. Since the aging effects on entry do not interfere with the identification of other coe¢cients in the model, we only estimate the average entry rate using a constant term.

[13]Derivation of Equation 8: $\beta_0 N_{st}^{Ex} = \beta_0 \sum_{a=22}^{64} N_{ast}^{Ex} = \sum_{a=22}^{64} \gamma^a N_{ast}^{Ex} = \sum_{a=22}^{64} (\gamma_0 + \gamma_1 a + \gamma_2 a^2) N_{ast}^{Ex} = \gamma_0 \sum_{a=22}^{64} N_{ast}^{Ex} + \gamma_1 \sum_{a=22}^{64} a N_{ast}^{Ex} + \gamma_2 \sum_{a=22}^{64} a^2 N_{ast}^{Ex}$

Figure 6: Aging Effects on Violent Crime Entry and Exit



Notes: fraction changes of the imputed crime rates against ages, averaged across all state-year observations. The left panel shows the entire criminal career span defined in this paper (13-64, entry and exit). The right panel zooms in on the 22-64 age range.

$$\Delta_{st}^{C}(NetEntry) = \alpha_0 \sum_{a=13}^{21} N_{ast}^{En} + \alpha_1 \sum_{a=13}^{21} N_{ast}^{En} I_{st}^{SIL} - \gamma_0 \sum_{a=22}^{64} N_{ast}^{Ex} - \gamma_1 \sum_{a=22}^{64} a N_{ast}^{Ex} - \gamma_2 \sum_{a=22}^{64} a^2 N_{ast}^{Ex}$$
$$- \beta_1 I_{st}^{SIL} \sum_{a=22}^{64} N_{ast}^{Selected} - \sum_{j=0}^{9+} \lambda_j I_{st}^{j} \sum_{a=22}^{64} N_{ast}^{Surprised} + \Gamma X_{st} + \epsilon_{st} \quad (9)$$

We estimate this equation separately for each crime type as well as the total violent crimes. The dependent variable, net entry, is constructed as the difference between the number of crimes in state s in year $t+1$ and year t weighted by state population in year t, i.e. $\Delta_{st}^{C} = (C_s^{t+1} - C_s^{t})/Pop_s^{t}$. All cohort variables on the right-hand side are also weighted by state population in year t for consistency. X_{st} include all control variables (state population, population density, real per capita personal income, income maintenance, unemployment insurance, and retirement payment for people older than 65), state and year fixed effects, and state-specific linear and quadratic time trends. ϵ_{st} is assumed to be autocorrelated over time within each state.

We also follow the dynamic panel data literature (e.g. Anderson and Hsiao (1982)) and use the lagged variables $N_{as,t+1}^{Ex}$, $N_{as,t+1}^{Selected}$ and $N_{as,t+1}^{Surprised}$ as instruments for all exit cohorts in the model[14]. The additional identifying assumption being made is that crime rates C_{st} follow an $AR(1)$ process over time within each state. We then use two stage least squares to estimate the CPDM.

[14] This is because the exit cohorts are imputed partially with the crime rates.

5 Results

In this section we present the estimates of our CPDM, test for the deterrence hypothesis as well as the model specification, further compare DD to the CPDM, and finally decompose the three effects from CPDM in a counterfactual example.

5.1 CPDM Estimates

Table 4 presents estimates of 4 different specifications of the CPDM, with and without the aging and the floodgate effects, on the total violent crimes only.

The baseline model estimates only the three basic effects (direct, selection, and surprise) on top of the base entry and exit rates. Only the CPDM parameters are reported and signed under the deterrence hypothesis in parentheses. The signs of the precisely estimated direct effect α_1 and selection effect β_1 contradict those predicted under the deterrence hypothesis, which we thus strongly reject. The two signs are, however, internally consistent within the model - more entry into violent crimes after SIL passages will lead to higher rate out of the criminal force when it comes to exit - a labor force shakeout. The surprise effect, on the other hand, is estimated to increase exit rates post-SIL for cohorts who became criminals before SIL passages. The older incumbent cohort is still shocked negatively despite the positive reactions of the potential entrants. We thus only find evidence on partial detterence of SILs on the incumbent criminals.

To interpret the magnitudes of our estimates, we note again that the model is estimated with crime rates as proxies instead of actual criminal populations. The dependent variable as well as all the exit cohorts are measured in the number of crimes (all variables are then weighted by every 100,000 state population), while the entry cohorts are measured in the number of potential entrants. Therefore, we have actually estimated the following equation,

$$\begin{aligned}\Delta^C_{st}(NetEntry) =& \hat{\alpha}_0\kappa \sum_{a=13}^{21} N^{En}_{ast} + \hat{\alpha}_1\kappa \sum_{a=13}^{21} N^{En}_{ast} I^{SIL}_{st} - \beta_0 \sum_{a=22}^{64} N^{Ex}_{ast} \\ & -\beta_1 I^{SIL}_{st} \sum_{a=22}^{64} N^{Selected}_{ast} - \beta_2 I^{SIL}_{st} \sum_{a=22}^{64} N^{Surprised}_{ast} + \Gamma X_{st} + \epsilon_{st}\end{aligned}$$

where κ is the number of crimes committed by a career violent criminal in a year and assumed to be constant across age, state, and time. Now the $\hat{\alpha}$'s and β's measure the corresponding entry and exit probabilities into and out of the criminal force (since we can divide the equation through by κ). Since we can not separately identify the $\hat{\alpha}$'s from κ due to data limitations, we only roughly interpret the magnitudes of the α's. The estimated α_1 suggests that, if a criminal commits 10 violent crimes a year, we estimate a 0.19% entry probability into violent criminals from the pool of all males between 13-21 in the absence of SIL. On the other hand, without knowing κ, we estimate a 22.3% (= 0.0042/0.0188) increase in this entry probability due to the direct effect of SIL. For exits, in the absence of SIL, violent criminals are estimated to exit with 53.1% probability annually.

Table 4: CPDM Specification Comparisons

	Baseline	w/ Aging	w/ Floodgate	w/ Both
Entry α_0	0.0188***	0.0111†	0.0210***	0.0148*
	(0.0145)	(0.2742)	(0.0049)	(0.1418)
SIL Entry α_1 (-)	0.0042***	0.0055***	0.0037***	0.0046**
	(0.0049)	(0.0023)	(0.0054)	(0.0208)
Exit β_0	0.5310***		0.5269***	
	(0.0000)		(0.0000)	
Exit γ_0		51.7108***		51.8624***
		(0.0032)		(0.0027)
Exit γ_1		-3.2453***		-3.2551***
		(0.0024)		(0.0020)
Exit γ_2		0.0483***		0.0484***
		(0.0016)		(0.0013)
Selection β_1 (-)	0.2628**	-0.1023	0.3759***	0.1161*
	(0.0429)	(0.3787)	(0.0179)	(0.1697)
Surprise β_2 (+)	0.1129***	0.1086***		
	(0.0006)	(0.0195)		
Floodgate λ_0			0.1071***	0.0890**
			(0.0002)	(0.0537)
Floodgate λ_1			0.0962***	0.0822**
			(0.0120)	(0.0669)
Floodgate λ_2			0.1091***	0.0886*
			(0.0007)	(0.1531)
Floodgate λ_3			0.0960***	0.0706†
			(0.0020)	(0.2672)
Floodgate λ_4			0.0770***	0.0555
			(0.0132)	(0.4294)
Floodgate λ_5			0.0558*	0.0301
			(0.1262)	(0.7143)
Floodgate λ_6			0.0630*	0.0355
			(0.1309)	(0.7152)
Floodgate λ_7			0.0351	-0.0199
			(0.3907)	(0.8304)
Floodgate λ_8			0.0008	-0.0609
			(0.9880)	(0.5588)
Floodgate λ_{9+}			-0.0050	-0.1172
			(0.9487)	(0.3825)
Log-likelihood	-7694	-7696	-7688	-7682
F-statistics	175.5	138.0	111.6	95.4
Nb. Obs.	1549	1549	1549	1549

Notes: all regressions are run on the total violent crimes. Arrest rates of violent crimes, demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). λ_j's measure the surprise effect in the j^{th} year after SIL passage. Key coefficients relevant for testing the deterence hypothesis are signed in parentheses. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

The estimated selection and surprise effects suggest that the criminal cohort that entered after SIL passages experience an additional 26.3 percentage points in exit probability with SIL due to the dilution in criminal quality from the higher entry rate, while the cohort that entered before SIL passages is surprised and exits with a probability increase of 11.3 percentage points.

Building upon the baseline model, we first introduce the aging effects that parametrize the base exit rate. We find very strong empirical evidence supporting the aging effects on base exit rates for violent criminals. All aging parameters are strongly significant. The resulting parabola of exit rates constructed from these estimates suggests the lowest exit rate around age 34, evidence for the peak of violent criminals' careers as a consequence of aging and huamn capital accumulations. All CPDM coefficients stay unchanged from the baseline model except for the selection effect. Aging effects take away the significance of the selection effect coefficient due to the differences in average ages across these different cohorts. The previously estimated selection effect is thus an artifact of the fact that the selected cohort is on average much younger, which is now absorbed away by the aging effects.

On the other hand, if we just relax the surprise effect to be flexible over time with the floodgate effects, the estimated CPDM parameters (except the surprise effect) stay almost unchanged from the baseline specification, while the surprise effect gets less precisely estimated over time as cohorts drop out of our sample. We refuse the temptation of re-running the regressions with ex-post cutoffs but only report them in Table 11 for robustness. The estimated magnitudes of the surprise effect also confirm the theory and taper off over time, capturing the reactions of the older cohort. Combining all of above, we arrive at our preferred specifiation with both aging and floodgate effects, as stated in Equation 9 and shown in the last column of Table 4.

We maximize the log-likelihood of the total violent crime regression to arrive at the entry window cutoff at age 21. F-statistics of the full model strongly reject null hypotheses that all coefficients of the model (except state and year fixed effects) are zeros and provide measures of the fit of the model.

We conduct hypothesis and specification tests in Table 5. Here we first formally test that the parabola of exit rates bottom out around age 33.6, statistically significant from zero. We also show that the aging effects and floodgate effects are both jointly significant where applicable. Although it is obvious from the point estimates in Table 4 that the deterrence hypothesis ($\alpha_1 > 0$, $\beta_1 < 0$, and $\beta_2 > 0$) will be rejected, we present the formal one-sided hypothesis tests in Table 5. Finally, we turn to the specification tests of DD. Specifically, the null hypotheses are the two restrictions in Section 2.1.3 that reduce the CPDM to Equation 5 and Equation 6. Namely, (1) $\beta_1 = \beta_2 = \beta_*$ and ((2) $\alpha_0 = -\beta_0$, (3) $\alpha_1 = -\beta_*$). Note that when we specify the non-parametric floodgate effects, (1) and (3) require all the floodgate effects to be the same with the selection effect to reduce to DD. Bottom of Table 5 then shows results that strongly reject the DD specification across all four specifications of the CPDM.

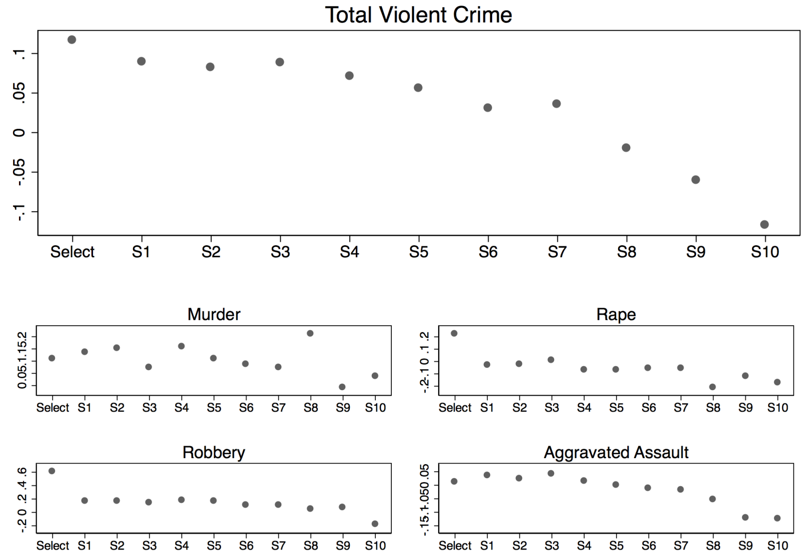
We further estimate our preferred specification on the four sub-categories of violent crimes. Table 6 shows the results. We first note that most of the estimated CPDM parameters (with

Table 5: Hypothesis and Specification Tests: CPDM Specifications (Table 4)

	Baseline	w/ Aging	w/ Floodgate	w/ Both
Turning point		33.6***		33.6***
		(0.0000)		(0.0000)
Joint significance tests				
Aging (γ's)		17.21***		16.90***
		(0.0002)		(0.0002)
Floodgates (λ's)			32.27***	34.72***
			(0.0004)	(0.0001)
Deterrence hypothesis tests				
$\alpha_1 < 0$	0.0042***	0.0055***	0.0037***	0.0046**
(one-sided)	(0.0025)	(0.0012)	(0.0027)	(0.0104)
$\beta_1 < 0$	0.2628**	-0.1023	0.3759***	0.1161*
(one-sided)	(0.0215)	(0.8107)	(0.0090)	(0.0849)
$\beta_2 > 0$	0.1129	0.1086	0.0972	0.0773
(one-sided)	(0.9997)	(0.9903)	(0.9997)	(0.9185)
Diff-in-diff nested specification tests				
(1) $\beta_1 = \lambda_j, \forall j$	1.25	3.03*	15.22†	23.96***
	(0.2628)	(0.0815)	(0.1241)	(0.0077)
(2) $\alpha_0 = -\beta_0$	162.31***	296.04***	175.48***	323.69***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(3) $-\alpha_1 = \beta_1 = \lambda_j, \forall j$	16.02***	6.80**	32.18***	44.15***
	(0.0003)	(0.0333)	(0.0000)	(0.0000)
(2) & (3)	171.75***	313.08***	475.39***	588.90***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)

Notes: age of the turning point, F-statistics for the joint significance tests, point estimates of the CPDM parameters, and F-statistics for the DD tests are shown. For specifications with floodgate effects, we replace β_2 with the weighted cumulative surprise effect of the first five floodgate effects, i.e. $\sum_{j=0}^{4} \lambda_j \frac{\sum_i I_{st}^{ij}}{\sum_{i,j} I_{st}^{ij}} > 0$. One-sided p values are in parentheses for the deterrence hypothesis tests. Two-sided p values are in parentheses for the rest. †, *, **, and *** indicate one-sided (deterrence hypothesis tests) and two-sided (rest) statistical significance at the 15, 10, 5, and 1 percent level.

Figure 7: Estimated Selection and Floodgate Surprise Effects



Notes: point estimates of selection (leftmost on each plot) and floodgate effects from CPDM on violent crimes and sub-categories (Table 6).

exception of surprise effects in later years) are significant and very consistent across crime types, suggesting much stronger results compared to the existing literature on SILs.

The floodgate surprise effects are again higher and more precisely estimated at the beginning and taper off nicely in later years. The pattern persists across all crime types as well. Figure 7 plots the floodgate surprise effects against the estimated selection effect (leftmost). The selection effects are generally higher than or equal to the surprise effects, suggesting again against the deterrence hypothesis. The differences between the two effects (particularly in rape and robbery) also imply the misspecification of a DD.

In the same vein of Table 5, we show results of formal hypothesis and specification tests on Table 4 in Table 7. We find the turning points to be statistically significant and consistent across crime types, with murder being slightly higer (39) and rape and aggravated assault lower (30), reflecting the peak of the combination of male physical conditions and criminal skill accumulations. All other tests show similar results across crime types as the total violent crime as shown in Table 5.

5.2 Comparing DD to CPDM

We further compare DD to our CPDM in this section, in relation to the evolutions of cohorts. Expanding on the Florida example depicted in Figure 4, Figure 8 shows the evolutions of average cohort sizes (across states) over time. Again, the total entry cohort measures male population between 13-21 and is stable over time (exogenous to SIL passages). Interacting the entry cohort

Table 6: CPDM with Aging and Non-Parametric Floodgate Effects: Crime Types

	Violent	Murder	Rape	Robbery	Agg. Ast.
Entry α_0	0.0148*	0.0003**	0.0009†	0.0127**	0.0087*
	(0.1418)	(0.0906)	(0.2303)	(0.0379)	(0.1455)
SIL Entry α_1 (-)	0.0046**	0.0001**	-0.0001	0.0012**	0.0015*
	(0.0208)	(0.0931)	(0.5455)	(0.0571)	(0.1625)
Exit γ_0	51.8624***	27.6572***	15.8851**	127.8388***	15.3134***
	(0.0027)	(0.0000)	(0.0626)	(0.0001)	(0.0017)
Exit γ_1	-3.2551***	-1.5197***	-1.1057**	-7.9613***	-1.0533***
	(0.0020)	(0.0000)	(0.0296)	(0.0001)	(0.0002)
Exit γ_2	0.0484***	0.0193***	0.0183***	0.1182***	0.0173***
	(0.0013)	(0.0000)	(0.0122)	(0.0000)	(0.0000)
Selection β_1 (-)	0.1161*	0.1101	0.2250	0.6073**	0.0133
	(0.1697)	(0.8271)	(0.3451)	(0.0978)	(0.8487)
Floodgate λ_0	0.0890**	0.1354*	-0.0322	0.1698**	0.0348
	(0.0537)	(0.1987)	(0.5159)	(0.0880)	(0.3547)
Floodgate λ_1	0.0822**	0.1521*	-0.0224	0.1725***	0.0242
	(0.0669)	(0.1805)	(0.7011)	(0.0163)	(0.5000)
Floodgate λ_2	0.0886*	0.0749	0.0075	0.1423*	0.0407
	(0.1531)	(0.4367)	(0.9043)	(0.1748)	(0.3396)
Floodgate λ_3	0.0706†	0.1601†	-0.0659	0.1748†	0.0158
	(0.2672)	(0.2101)	(0.4377)	(0.2149)	(0.6921)
Floodgate λ_4	0.0555	0.1107*	-0.0671	0.1667†	0.0010
	(0.4294)	(0.1630)	(0.3416)	(0.2820)	(0.9826)
Floodgate λ_5	0.0301	0.0862	-0.0550	0.1114	-0.0115
	(0.7143)	(0.4756)	(0.4754)	(0.5664)	(0.8261)
Floodgate λ_6	0.0355	0.0735	-0.0552	0.1097	-0.0187
	(0.7152)	(0.4470)	(0.4965)	(0.6520)	(0.7663)
Floodgate λ_7	-0.0199	0.2120**	-0.2131**	0.0434	-0.0535
	(0.8304)	(0.1474)	(0.0782)	(0.8584)	(0.3525)
Floodgate λ_8	-0.0609	-0.0091	-0.1217	0.0761	-0.1211**
	(0.5588)	(0.9305)	(0.3146)	(0.7633)	(0.0908)
Floodgate λ_{9+}	-0.1172	0.0378	-0.1722*	-0.1743	-0.1241*
	(0.3825)	(0.7961)	(0.1564)	(0.6609)	(0.1631)
Log-likelihood	-7682	-2286	-4012	-6827	-7088
F-statistics	95.4	672.5	62.3	171.8	122.2
Nb. Obs.	1549	1548	1547	1546	1549

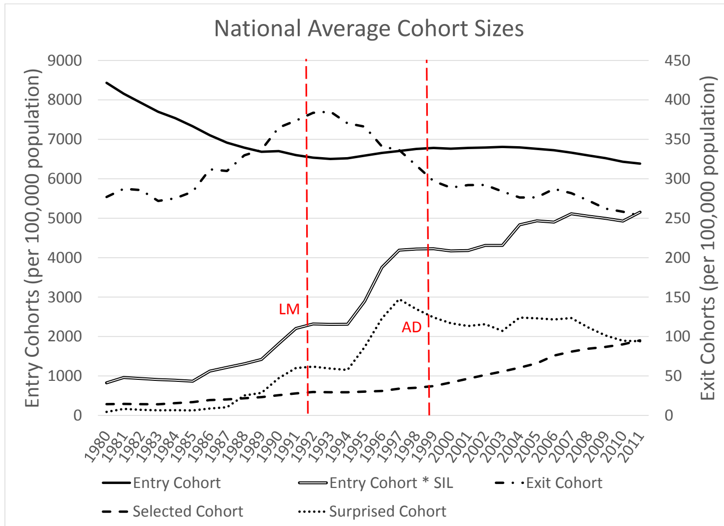
Notes: arrest rates (of corresponding crime categories), demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). λ_j's measure the surprise effect in the j^{th} year after SIL passage. The F-statistics test for the joint significance of all estimated coefficients and reject the null (all coefficients are equal to zero) in all specifications. Key coefficients relevant for testing the deterence hypothesis are signed in parentheses. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 7: Hypothesis and Specification Tests: Crime Types (Table 6)

	Violent	Murder	Rape	Robbery	Agg. Ast.
Turning point	33.6***	39.3***	30.2***	33.7***	30.5***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Joint significance tests					
Aging (γ's)	16.90***	51.10***	8.24**	58.91***	25.52***
	(0.0002)	(0.0000)	(0.0162)	(0.0000)	(0.0000)
Floodgates (λ's)	34.72***	7.79	32.43***	32.60***	18.30*
	(0.0001)	(0.6498)	(0.0003)	(0.0003)	(0.0501)
Deterrence hypothesis tests					
$\alpha_1 < 0$	0.0046**	0.0001**	-0.0001	0.0012**	0.0015*
(one-sided)	(0.0104)	(0.0466)	(0.7273)	(0.0286)	(0.0813)
$\beta_1 < 0$	0.1161*	0.1101	0.2250	0.6073**	0.0133
(one-sided)	(0.0849)	(0.4136)	(0.1726)	(0.0489)	(0.4244)
$\beta_2 > 0$	0.0773	0.1268	-0.0360	0.1653	0.0234
(one-sided)	(0.9185)	(0.9052)	(0.2782)	(0.9346)	(0.7363)
Diff-in-diff nested specification tests					
(1) $\beta_1 = \lambda_j, \forall j$	23.96***	15.59†	32.60***	21.51**	17.52*
	(0.0077)	(0.1120)	(0.0003)	(0.0178)	(0.0636)
(2) $\alpha_0 = -\beta_0$	323.69***	216.66***	38.21***	430.67***	74.06***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(3) $-\alpha_1 = \beta_1 = \lambda_j, \forall j$	44.15***	25.25***	32.65***	35.45***	18.47*
	(0.0000)	(0.0084)	(0.0006)	(0.0002)	(0.0712)
(2) & (3)	588.90***	364.62***	82.23***	742.73***	154.53***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)

Notes: age of the turning point, F-statistics for the joint significance tests, point estimates of the CPDM parameters, and F-statistics for the DD tests are shown. We replace β_2 with the weighted cumulative surprise effect of the first five floodgate effects, i.e. $\sum_{j=0}^{4} \lambda_j \frac{\sum_i I_{st}^{ij}}{\sum_{i,j} I_{st}^{ij}} > 0$. One-sided p values are in parentheses for the deterrence hypothesis tests. Two-sided p values are in parentheses for the rest. †, *, **, and *** indicate one-sided (deterrence hypothesis tests) and two-sided (rest) statistical significance at the 15, 10, 5, and 1 percent level.

Figure 8: Evolutions of National Average Cohort Sizes



Notes: entry cohorts (solid lines) are measured in 100,000 population on the left axis. Exit cohorts (dashed/dotted lines) are measured in 100,000 population on the right axis. Cohorts are averaged across all states. The vertical dashed lines indicate where LM and AD's samples end, respectively.

with SIL passages, the double-solid line exhibits the growth of SIL states as shown in Figure 1. The total exit cohort again follows the national trend of violent crimes. However, note that the total exit cohort is not the sum of the selected and surprised cohorts nationally as states adopt SILs at different times and some states never do so. The surprised cohort first increases as more states adopt SILs and then starts decreasing in late 1990's as the old criminal cohorts exit without being replenished. Finally, the selected cohort keeps gradually increasing as more states adopt SILs and more new criminals having entered under SILs.

Top of Table 8 presents the evolutions of the shares of these cohorts for different sample lengths (LM, AD, and this paper). s^{En} is the share of the entry cohort as a fraction of the total population at risk (the sum of entry and exit cohorts). $s^{*Selected}$ and $s^{*Surprised}$ are defined similarly as in Section 2.1.1, as a fraction of the total exit cohort. Note that the share of the surprise cohort is highest in the middle sample due to the dynamics.

Given these evolutions, we then compare the corresponding DD estimates in these different samples with our CPDM. We estimate two standard DD models as follows,

$$C_{st} = \alpha + \beta I_{st}^{SIL} + \Gamma X_{st} + \varepsilon_{st} \tag{10}$$

$$\Delta_{st}^{C} = \alpha' + \beta' I_{st}^{SIL} + \Gamma' X_{st} + \varepsilon_{st} \tag{11}$$

where Equation 10 is estimated with levels of violent crimes, while Equation 11 uses year-to-year changes of violent crimes. I_{st}^{SIL} is the standard multiple-event DD dummy that equals one if state s has SIL in place at time t. X_{st} includes the same set of controls as in our CPDM in Equation 9. Note that Equation 11 is the same with Equation 6 after weighting by the total population. The first two samples highly resemble the data used in LM and AD[15]. The DD specifications in Equations 10 and 11 are more general and robust than the "dummy variable model" of LM and the "hybrid model" of AD[16]. We also account for auto-correlated errors by clustering at the state level.

The estimates are shown in the middle panel of Table 8. Similar to BDM, we find that most of the effects are essentially zero (with no consistency in signs) after controlling for trends and auto-correlations of errors. We only find significant effects (about 7% reduction in crimes following passages of SILs) with the 1980-1999 sample on the levels of crime rates[17]. The DD estimates reflect the evolutions and offsetting effects of the different cohorts. We have found that the surprise effect increases exit rates and thus decreases net entry rates and levels of crimes - the effect of SIL on crimes is thus dominantly negative when the surprised cohort dominates in the 1980-1999 sample. The reversed trends of the entry and exit cohorts, together with the positive entry and selection effects, also contribute to the negative DD estimate in the 1990's sample. In the full sample, as the exit cohort shrinks with the national trend, the surprised cohort decreases, and the tapering off of the surprise effect over time, we see very weak evidence of positive effects estimated by DD. The DD estimates are also largely insignificant as the entry effects offset the surprise and selection effects.

We then turn to the CPDM estimates of the varying sample lengths in the bottom panel. For comparison, we only show estimates from the baseline CPDM using ordinary least squares[18]. We find strongly significant results with consistency in the estimated signs across different sample lengths. In the shorter samples, the CPDM also struggles to precisely estimate base exit rates (column 1) and base entry rates (column 2), which may bias the dynamic selection and surprise effects slightly upwards due to the aging effects of exit. Despite of this, the CPDM also consistently estimates the direct entry effect across all samples, which, together with the consistently estimated signs of other parameters, yields the most important policy implications.

[15] We differ with them in data in two ways. Both of their data begin with 1977 while ours is cut off at 1980 due to the availability of the cohort population data. We also estimate a DD from 1977 but only report results from 1980 (which are similar) for comparison with the CPDM. While AD also use state-level panel data, LM uses county-level crime data. We also use state-level data for comparison with CPDM but the DD estimates are similar on the county level as well.

[16] We defer further discussions on the literature to Appendix A.2. See Table 15 and Table 16 for details.

[17] These results largely contradict with findings of LM and AD. See Appendix A.2 for replications of LM and AD, and comparisons of different DD specifications.

[18] For robustness, see Appendix A.1.4 for OLS estimates of the full model.

Table 8: Comparison of DD with CPDM for Different Sample Lengths

	1980-1992	1980-1999	1980-2011
Average Cohort Sizes			
s^{En}	0.9585	0.9559	0.9574
$s^{En}I^{SIL}$	0.1707	0.2762	0.4230
$s^{Ex} = 1 - s^{En}$	0.0415	0.0441	0.0426
$s^{*Selected}$	0.4727	0.3235	0.3585
$s^{*Surprised}$	0.5273	0.6765	0.6415
Diff-in-Diff in Levels			
SIL Dummy	-0.8789	-38.0306*	0.8612
	(0.9550)	(0.0582)	(0.9580)
Diff-in-Diff in Changes			
SIL Dummy	-1.6291	-0.2657	2.6447
	(0.8881)	(0.9758)	(0.6256)
Baseline CPDM			
Entry	0.1626*	0.0091	0.0190**
	(0.0949)	(0.7368)	(0.0107)
SIL Entry	0.0017	0.0028*	0.0041***
	(0.3214)	(0.0538)	(0.0015)
Exit	1.0768***	0.3946***	0.3232***
	(0.0000)	(0.0003)	(0.0000)
Selection	0.2604	0.3174*	0.1943†
	(0.6146)	(0.0595)	(0.1342)
Surprise	0.0381	0.1054***	0.1067***
	(0.5829)	(0.0000)	(0.0001)
Nb. Obs.	657	994	1549

Notes: all regressions are run on the total violent crimes. All regressions are run using OLS. Arrest rates of violent crimes, demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate two-sided statistical significance at the 15, 10, 5, and 1 percent level.

5.3 Counterfactual Example

In order to make direct policy evaluations with the CPDM, accouting for the entry, selection and surprise effects, we consider the following counterfactual example. In this example, we eliminate SILs from all states and compute the counterfactual crime levels in the U.S. had we never adopted SILs. To do so, we start with crime rates in 1980 at the beginning of our sample, let the CPDM predict changes in crimes from year to year for all states while shutting down all post-SIL effects (entry, selection, and surprise), and then simulate crime levels for all states in all following years. The result is shown in Figure 9. The actual data (solid line) shows that violent crimes totaled at 1.3 million in the U.S. in 1980, peaked at 1.9 million in 1992, and settled at 1.2 million in 2011. When we take away the effects of SILs (dotted line), we find a drop in violent crimes that shows the dynamic properties that the CPDM captures. After eliminating SILs, the counterfactually predicted crime rates track the actual crime rates very closely for 2/3 of the sample and only diverge in the last 1/3, although by year 2000, 3/4 of the states have already adopted SIL. For example, in 1995, the counterfactual prediction only shows a 1.4% (about 26000 crimes annually) reduction in crime levels. By 2011, there is a large reduction of 34.8% (or about 419000 crimes) in total violent crimes[19].

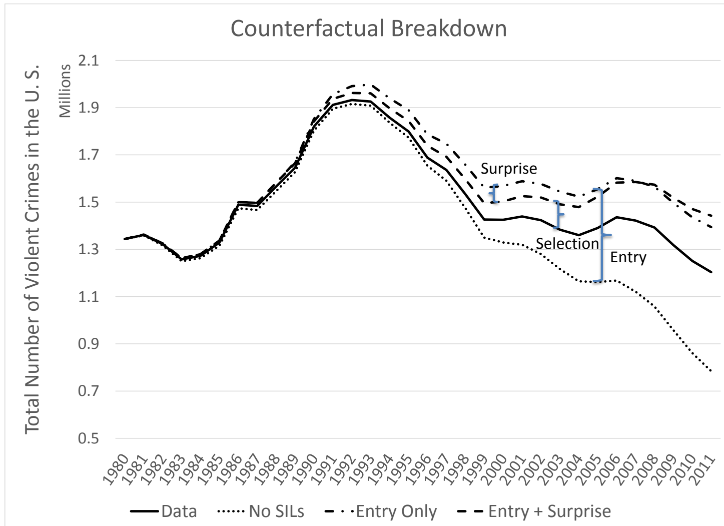
We then further decompose this gap between the levels of crimes into the three effects captured by CPDM. From the dotted line where there are no post-SIL effects, we first add back only the direct entry effect (dash-dotted line). Graphically, the entry effect is positive and significant, driving up the total violent crime level to about 1.4 million in 2011. Adding on top of that the surprise effects (dashed line), which increase exit rates in the first few years following SIL passages and taper off after, shifts down the overall curve but dissipates at the end of the sample. Finally, the remaining gap between the dashed line and the solid line represents the selection effect, which captures the increased exit rates from the lesser criminals who entered post-SIL. As expected, this gap keeps widening over time as the younger cohorts replace their older counterparts.

6 Conclusion

In this paper, we use a more general cohort panel data model to bring a consistent and unified answer to the debate of the effects of shall-issue laws on violent crimes. The CPDM incorporates dynamic decision-making by forward-looking agents through the estimation of (i) a direct effect of SIL passages on entry (into violent crime careers), (ii) a selection effect on exit for those who entered the violent crime under SIL, and (iii) a surprise effect on exit for those who entered prior to the advent of SIL. We find all three effects to be positive - suggesting that in addition to the deterrence effect on existing criminals (who entered before SIL), the passages of SIL also substantially lower

[19] We interpret the large drop as an upper bound for the amount of crime reductions if SILs were eliminated. The reason is that, although we have eliminated all post-SIL effects in the counterfactual simulation, we keep the stock of criminals (i.e. base exit cohorts) constant. With lower entry rate absent SILs, we should see a smaller stock of criminals and consequently less exits as well, which would shift up the dotted line. We ignore this second-order effect in this exercise.

Figure 9: Decomposition of Entry, Selection, and Surprise Effects



Notes: counterfactual national violent crime levels if no SILs were ever adopted. Brackets indicate magnitudes of indicidual entry, selection, and surprise effects, decomposed in contribution to the total effect of SILs on crimes. Estimates obtained from OLS regressions.

the barrier of entry for new potential criminals. The combined effect is large - eliminating all passed SILs from the beginning would reduce total violent crimes by about one third by 2011.

We further show that in contexts where heterogeneous agents make forward-looking decisions the standard DD is a model misspecification due to the lack of dynamic considerations. Our CPDM reduces to the standard DD with restrictions that shut down the three effects. The estimated coefficients strongly reject such restrictions and thus rule the DD as misspecified. We then compare the CPDM and DD estimates on samples with varying lengths corresponding to the literature (LM and AD). We find that the DD estimates fluctuate systematically based on the evolutions of cohort shares - leading to the heated debate in the literature. The CPDM, on the other hand, yields consistent and highly significant results across different sample lengths.

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A Appendix

A.1 Robustness

A.1.1 Entry Windows and Retirement Ages

In our main specification, we choose the starting age of the entry window and the retirement age based on the empirical distribution of arrests over ages. We then choose the cutoff between the entry window and the exit window by maximizing the log-likelihood of the baseline CPDM estimation[20]. In this section, we arbitrarily vary these three cutoffs and show that our results are robust. Table 9 presents the results estimated on our preferred specification.

A.1.2 Aging Effects

In this section, we explore different functional forms of the aging effects on base exit rates and the robustness of the CPDM to the different parametrizations. Table 10 presents the results. We note that, although in the last column the cubic term is statistically significant, we believe that the more parsimonious quadratic polunomial is sufficiently flexible. On the other hand, we have robust estimates across all specifications except the selection effect in the last column, which is imprecisely estimated.

A.1.3 Floodgate Effects

In our preferred specification, we adopt a non-parametric specification of the floodgate effects. In this section, we show that our estimates for all crime types are robust to more parametric specifications. Table 11 presents the results when we group individual year fixed effects and Table 12 shows the linear trend estimates.

A.1.4 OLS Estimates

Table 13 presents estimates from OLS without the dynamic panel instruments. We find similar results compared with Table 6 using IVs.

A.1.5 CPDM on Levels

A.2 Literature Replications

In this section, we review and test the robustness of model specifications in LM and AD. We use state-level panel data from 1980 onwards and only present results on the total violent crimes. LM adopts a simple "dummy variable model," where they only control for state and year fixed effects (but not trends). We first try to replicate their results with our data and then test its robustness with variations of the specification, controls, and sample lengths. Table 15 shows the results. Column (1) resembles the most of their main specification. Specifically, the dependent

[20] The reported standard errors do not take into account the uncertainty of the cutoffs.

Table 9: Entry Window and Retirement Cutoffs (Entry Start-Entry End-Retirement)

	(11-21-64)	(15-21-64)	(13-19-64)	(13-23-64)	(13-21-54)	(13-21-74)
Entry α_0	0.0168**	0.0119†	0.0137†	0.0106†	0.0160**	0.0128*
	(0.0899)	(0.2806)	(0.2570)	(0.2469)	(0.0855)	(0.1795)
SIL Entry α_1	0.0040***	0.0055**	0.0051**	0.0040**	0.0034**	0.0050***
	(0.0136)	(0.0342)	(0.0258)	(0.0229)	(0.0320)	(0.0130)
Exit γ_0	52.5361***	51.1861***	34.6856***	70.3408***	47.2253***	54.1816***
	(0.0028)	(0.0030)	(0.0000)	(0.0128)	(0.0013)	(0.0015)
Exit γ_1	-3.2961***	-3.2166***	-2.2753***	-4.2844***	-2.9579***	-3.3876***
	(0.0021)	(0.0022)	(0.0000)	(0.0103)	(0.0008)	(0.0011)
Exit γ_2	0.0490***	0.0479***	0.0350***	0.0623***	0.0445***	0.0498***
	(0.0014)	(0.0014)	(0.0000)	(0.0076)	(0.0004)	(0.0007)
Selection β_1	0.1158*	0.1141*	0.1352**	0.1024	0.2259**	0.1029†
	(0.1702)	(0.1871)	(0.0960)	(0.3266)	(0.0454)	(0.2056)
Floodgate λ_0	0.0927**	0.0842**	0.0666**	0.1056**	0.0738**	0.0994**
	(0.0447)	(0.0689)	(0.0691)	(0.0568)	(0.0601)	(0.0303)
Floodgate λ_1	0.0851**	0.0779**	0.0598**	0.0979**	0.0649**	0.0943**
	(0.0568)	(0.0860)	(0.0919)	(0.0709)	(0.0903)	(0.0372)
Floodgate λ_2	0.0911*	0.0844*	0.0644*	0.1070*	0.0715*	0.1020*
	(0.1390)	(0.1806)	(0.1912)	(0.1453)	(0.1716)	(0.1008)
Floodgate λ_3	0.0734†	0.0660	0.0477	0.0888†	0.0505	0.0817†
	(0.2431)	(0.3115)	(0.3411)	(0.2447)	(0.3337)	(0.2004)
Floodgate λ_4	0.0583	0.0506	0.0344	0.0747	0.0304	0.0664
	(0.4004)	(0.4838)	(0.5436)	(0.3674)	(0.6134)	(0.3233)
Floodgate λ_5	0.0329	0.0246	0.0131	0.0474	0.0041	0.0441
	(0.6843)	(0.7731)	(0.8452)	(0.6196)	(0.9544)	(0.5765)
Floodgate λ_6	0.0386	0.0295	0.0168	0.0555	0.0062	0.0554
	(0.6852)	(0.7724)	(0.8342)	(0.6184)	(0.9427)	(0.5559)
Floodgate λ_7	-0.0145	-0.0303	-0.0323	-0.0098	-0.0461	0.0053
	(0.8729)	(0.7607)	(0.6692)	(0.9273)	(0.5650)	(0.9526)
Floodgate λ_8	-0.0521	-0.0752	-0.0706	-0.0544	-0.0913	-0.0341
	(0.6030)	(0.5033)	(0.4057)	(0.6452)	(0.3309)	(0.7285)
Floodgate λ_{9+}	-0.1025	-0.1346	-0.1171†	-0.1216	-0.1396†	-0.0837
	(0.4200)	(0.3496)	(0.2866)	(0.4208)	(0.2589)	(0.5118)
Nb. Obs.	1549	1549	1549	1549	1549	

Notes: all regressions are run on the total violent crimes. Arrest rates of violent crimes, demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1 and γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). λ_j's measure the surprise effect in the j^{th} year after SIL passage. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 10: Parametrizations of Aging Effects

	Constant	Linear	Quadratic	Cubic	Translog	Quad. Exp.
Entry α_0	0.0210***	0.0082†	0.0148*	0.0108†	0.0143*	0.0148*
	(0.0049)	(0.2139)	(0.1418)	(0.2354)	(0.1319)	(0.1418)
SIL Entry α_1	0.0037***	0.0025**	0.0046**	0.0063***	0.0039**	0.0046**
	(0.0054)	(0.0311)	(0.0208)	(0.0049)	(0.0320)	(0.0208)
Exit γ_0	0.5269***	-5.6213***	51.8624***	-153.8857***	626.4235***	4.8548***
	(0.0000)	(0.0000)	(0.0027)	(0.0001)	(0.0004)	(0.0077)
Exit γ_1		0.1913***	-3.2551***	14.5884***	-363.0427***	-1.2217***
		(0.0000)	(0.0020)	(0.0002)	(0.0003)	(0.0038)
Exit γ_2			0.0484***	-0.4523***	52.3943***	0.0484***
			(0.0013)	(0.0004)	(0.0003)	(0.0013)
Exit γ_3				0.0045***		
				(0.0006)		
Selection β_1	0.3759***	0.2822**	0.1161*	0.0081	0.1701**	0.1161*
	(0.0179)	(0.0221)	(0.1697)	(0.9323)	(0.0816)	(0.1696)
Floodgate λ_0	0.1071***	0.0529**	0.0890**	0.1186***	0.0781**	0.0890**
	(0.0002)	(0.0970)	(0.0537)	(0.0157)	(0.0692)	(0.0537)
Floodgate λ_1	0.0962***	0.0454*	0.0822**	0.1137**	0.0711**	0.0822**
	(0.0120)	(0.1841)	(0.0669)	(0.0209)	(0.0875)	(0.0669)
Floodgate λ_2	0.1091***	0.0596*	0.0886*	0.1284**	0.0769*	0.0886*
	(0.0007)	(0.1572)	(0.1531)	(0.0473)	(0.1838)	(0.1531)
Floodgate λ_3	0.0960***	0.0476†	0.0706†	0.1213**	0.0577	0.0706†
	(0.0020)	(0.2518)	(0.2672)	(0.0768)	(0.3244)	(0.2672)
Floodgate λ_4	0.0770***	0.0296	0.0555	0.1171**	0.0397	0.0555
	(0.0132)	(0.5070)	(0.4294)	(0.0996)	(0.5456)	(0.4294)
Floodgate λ_5	0.0558*	0.0089	0.0301	0.0957†	0.0145	0.0301
	(0.1262)	(0.8647)	(0.7143)	(0.2320)	(0.8517)	(0.7143)
Floodgate λ_6	0.0630*	0.0113	0.0355	0.1098†	0.0178	0.0355
	(0.1309)	(0.8438)	(0.7152)	(0.2345)	(0.8463)	(0.7152)
Floodgate λ_7	0.0351	-0.0282	-0.0199	0.0527	-0.0334	-0.0199
	(0.3907)	(0.5733)	(0.8304)	(0.5417)	(0.6999)	(0.8304)
Floodgate λ_8	0.0008	-0.0607	-0.0609	0.0210	-0.0744	-0.0609
	(0.9880)	(0.3248)	(0.5588)	(0.8298)	(0.4480)	(0.5587)
Floodgate λ_{9+}	-0.0050	-0.0835	-0.1172	-0.0466	-0.1206	-0.1172
	(0.9487)	(0.3108)	(0.3825)	0.7168	(0.3368)	(0.3825)
Nb. Obs.	1549	1549	1549	1549	1549	1549

Notes: all regressions are run on the total violent crimes. Arrest rates of violent crimes, demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 and γ_3 are coefficients of the constant, linear, quadratic and cubic terms of the exit function (of age). For the translog function, we replace age with $log(age)$; for the quadratic experience column, we replace age with $age - 21$. λ_j's measure the surprise effect in the j^{th} year after SIL passage. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 11: CPDM with Grouped Floodgate Effects

	Violent	Murder	Rape	Robbery	Agg. Ast.
Entry α_0	0.0130*	0.0003*	0.0010*	0.0121**	0.0077*
	(0.1989)	(0.1041)	(0.1839)	(0.0506)	(0.1946)
SIL Entry α_1 (-)	0.0051***	0.0001**	-0.0001	0.0013**	0.0017*
	(0.0082)	(0.0877)	(0.6576)	(0.0234)	(0.1082)
Exit γ_0	51.5256***	27.6088***	15.5613**	127.8716***	13.9920***
	(0.0034)	(0.0000)	(0.0554)	(0.0001)	(0.0038)
Exit γ_1	-3.2329***	-1.5160***	-1.0864**	-7.9605***	-0.9719***
	(0.0025)	(0.0000)	(0.0236)	(0.0001)	(0.0006)
Exit γ_2	0.0481***	0.0193***	0.0180***	0.1182***	0.0161***
	(0.0017)	(0.0000)	(0.0085)	(0.0000)	(0.0001)
Selection β_1 (-)	-0.0228	-0.2386	0.0909	0.4953*	-0.0708†
	(0.7713)	(0.6279)	(0.6114)	(0.1217)	(0.2219)
Floodgate λ_{0-1}	0.1036***	0.1471*	-0.0122	0.1914***	0.0438*
	(0.0122)	(0.1707)	(0.8044)	(0.0084)	(0.1923)
Floodgate λ_{2-4}	0.1012**	0.1330*	-0.0149	0.1904**	0.0441†
	(0.0879)	(0.1807)	(0.8109)	(0.0875)	(0.2535)
Floodgate λ_{5-9}	0.0445	0.1518†	-0.0625	0.1266	-0.0077
	(0.6051)	(0.2261)	(0.4402)	(0.5348)	(0.8893)
Floodgate λ_{10+}	0.0228	0.3195*	-0.0594	-0.0052	-0.0059
	(0.8338)	(0.1476)	(0.5055)	(0.9868)	(0.9378)
Log-likelihood	-7689	-2297	-4025	-6830	-7092
F-statistics	123.6	778.0	61.7	175.0	105.6
Nb. Obs.	1549	1548	1547	1546	1549

Notes: arrest rates (of corresponding crime categories), demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). The F-statistics test for the joint significance of all estimated coefficients and reject the null (all coefficients are equal to zero) in all specifications. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 12: CPDM with Linear Floodgate Trend

	Violent	Murder	Rape	Robbery	Agg. Ast.
Entry α_0	0.0138*	0.0003**	0.0009†	0.0120**	0.0083*
	(0.1721)	(0.0912)	(0.2096)	(0.0516)	(0.1629)
SIL Entry α_1	0.0050***	0.0001**	-0.0001	0.0014***	0.0016*
	(0.0164)	(0.0962)	(0.6136)	(0.0168)	(0.1433)
Exit γ_0	51.6829***	27.7278***	16.3377**	127.9731***	14.8706***
	(0.0030)	(0.0000)	(0.0453)	(0.0001)	(0.0015)
Exit γ_1	-3.2418***	-1.5233***	-1.1334***	-7.9673***	-1.0244***
	(0.0022)	(0.0000)	(0.0198)	(0.0001)	(0.0002)
Exit γ_2	0.0482***	0.0194***	0.0187***	0.1183***	0.0168***
	(0.0015)	(0.0000)	(0.0076)	(0.0000)	(0.0000)
Selection β_1	0.0070	0.0271	0.1633	0.4449*	-0.0349
	(0.9364)	(0.9473)	(0.4631)	(0.1393)	(0.5816)
Floodgate cons.	0.1363***	0.1445*	0.0153	0.2343***	0.0735**
	(0.0005)	(0.1867)	(0.7482)	(0.0000)	(0.0220)
Floodgate slope	-0.0154**	-0.0058	-0.0150*	-0.0112	-0.0147**
	(0.0932)	(0.5364)	(0.1688)	(0.6826)	(0.0233)
Log-likelihood	-7685	-2291	-4022	-6832	-7090
F-statistics	134.7	834.3	38.5	152.9	98.1
Nb. Obs.	1549	1548	1547	1546	1549

Notes: arrest rates (of corresponding crime categories), demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). The F-statistics test for the joint significance of all estimated coefficients and reject the null (all coefficients are equal to zero) in all specifications. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 13: OLS Estimates: CPDM Preferred Specification

	Violent	Murder	Rape	Robbery	Agg. Ast.
Entry α_0	0.0125*	0.0002	0.0010†	0.0090**	0.0083*
	(0.1258)	(0.3141)	(0.2291)	(0.0951)	(0.1153)
SIL Entry α_1 (-)	0.0032**	0.0002***	-0.0001	0.0010**	0.0011†
	(0.0410)	(0.0001)	(0.5929)	(0.0539)	(0.2377)
Exit γ_0	18.8892**	35.5137***	13.9282†	55.3285***	6.5723**
	(0.0328)	(0.0000)	(0.2331)	(0.0090)	(0.0828)
Exit γ_1	-1.2704**	-1.9969***	-0.9334*	-3.5189***	-0.5185***
	(0.0206)	(0.0000)	(0.1705)	(0.0059)	(0.0183)
Exit γ_2	0.0202***	0.0261***	0.0151*	0.0536***	0.0094***
	(0.0114)	(0.0000)	(0.1053)	(0.0031)	(0.0022)
Selection β_1 (-)	0.0727	0.9867***	0.2705*	0.2807**	-0.0814
	(0.3850)	(0.0039)	(0.1781)	(0.0857)	(0.3092)
Floodgate λ_0	0.0636**	0.3219***	-0.0182	0.1179*	0.0289
	(0.0894)	(0.0003)	(0.7077)	(0.1230)	(0.4013)
Floodgate λ_1	0.0529*	0.3383***	-0.0129	0.1339***	0.0120
	(0.1372)	(0.0001)	(0.8265)	(0.0104)	(0.7102)
Floodgate λ_2	0.0651†	0.2632***	0.0170	0.1002	0.0348
	(0.2309)	(0.0131)	(0.7802)	(0.3097)	(0.3887)
Floodgate λ_3	0.0468	0.3483***	-0.0589	0.1417†	0.0067
	(0.3519)	(0.0001)	(0.5021)	(0.2389)	(0.8494)
Floodgate λ_4	0.0330	0.3017***	-0.0612	0.1021	-0.0023
	(0.5540)	(0.0012)	(0.3384)	(0.3958)	(0.9537)
Floodgate λ_5	0.0121	0.2719***	-0.0455	0.0436	-0.0109
	(0.8558)	(0.0081)	(0.5212)	(0.7758)	(0.8212)
Floodgate λ_6	0.0230	0.2339***	-0.0526	0.0760	-0.0111
	(0.7584)	(0.0082)	(0.4802)	(0.6686)	(0.8440)
Floodgate λ_7	-0.0202	0.3339***	-0.2080**	-0.0059	-0.0369
	(0.7525)	(0.0008)	(0.0705)	(0.9727)	(0.4523)
Floodgate λ_8	-0.0581	0.1372*	-0.1092†	0.0400	-0.1056*
	(0.4782)	(0.1893)	(0.2737)	(0.8225)	(0.1237)
Floodgate λ_{9+}	-0.0752	0.0717	-0.1617*	-0.1756	-0.0710
	(0.4697)	(0.5762)	(0.1341)	(0.5674)	(0.3539)
Nb. Obs.	1549	1548	1547	1546	1549

Notes: arrest rates (of corresponding crime categories), demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). λ_j's measure the surprise effect in the j^{th} year after SIL passage. Key coefficients relevant for testing the deterence hypothesis are signed in parentheses. Standard errors are clustered at the state level. Two-sided p values are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 14: CPDM Dependent Variable: Changes vs. Levels

	Change Δ_{st}^{C}	Level $C_{s,t+1}$
Lag C_{st}		1.1261***
(S.E.)		(0.4573)
Entry α_0	0.0148*	0.0135*
	(0.1418)	(0.1743)
SIL Entry α_1 (-)	0.0046**	0.0037**
	(0.0208)	(0.0394)
Exit γ_0	51.8624***	25.7653***
	(0.0027)	(0.0005)
Exit γ_1	-3.2551***	-1.7063***
	(0.0020)	(0.0001)
Exit γ_2	0.0484***	0.0270***
	(0.0013)	(3E-5)
Selection β_1 (-)	0.1161*	0.1987**
	(0.1697)	(0.0242)
Floodgate λ_0	0.0890**	0.0703*
	(0.0537)	(0.1158)
Floodgate λ_1	0.0822**	0.0640*
	(0.0669)	(0.1428)
Floodgate λ_2	0.0886*	0.0757†
	(0.1531)	(0.2219)
Floodgate λ_3	0.0706†	0.0603
	(0.2672)	(0.3314)
Floodgate λ_4	0.0555	0.0427
	(0.4294)	(0.5287)
Floodgate λ_5	0.0301	0.0169
	(0.7143)	(0.8330)
Floodgate λ_6	0.0355	0.0217
	(0.7152)	(0.8122)
Floodgate λ_7	-0.0199	-0.0288
	(0.8304)	(0.7343)
Floodgate λ_8	-0.0609	-0.0681
	(0.5588)	(0.4868)
Floodgate λ_{9+}	-0.1172	-0.1099
	(0.3825)	(0.3815)
Nb. Obs.	1549	1498

Notes: arrest rates (of corresponding crime categories), demographic and welfare controls, state and year fixed effects and state-specific linear and quadratic time trends are controlled for but not reported. γ_0, γ_1, γ_2 are coefficients of the constant, linear and quadratic terms of the exit function (of age). λ_j's measure the surprise effect in the j^{th} year after SIL passage. Key coefficients relevant for testing the deterence hypothesis are signed in parentheses. Standard errors are clustered at the state level. Two-sided p values (except for the lag variable, which shows the standard error) are in parentheses. †, *, **, and *** indicate one-sided statistical significance at the 15, 10, 5, and 1 percent level.

variable is the log of crime rates and the demographic controls include arrest rates, state population, population density, real per capita personal income, income maintenance, unemployment insurance, and retirement payment for people older than 65. In particular, LM also control for a large set of race and age group variables (18 groups divided into three races - black, white, and others - and six age groups - 10-19, 20-29, 30-39, 40-49, 50-59, and 65+). We include the same controls in column 1 for comparison but later exclude them in our preferred DD specification. Similar to LM, we find a roughly 8.8% (vs. 5-10% in LM) reduction in violent crimes following SIL passages. In columns (2) and (3), we keep the same specification but expand the sample to 1999 and 2011, respectively. Despite having more observations in the sample, we find gradually smaller and less precisely estimated effects. With this specification and the full sample in (3), we find essentially zero effect of SILs on violent crimes. We then compare column (4) with (1) by dropping the controversial race and age controls. We also find small and almost insignificant effects. The last two columns are our preferred specifications[21], where we exclude the race and age controls but instead control for state-specific linear and quadratic time trends and account for serially correlated errors by clustering on the state level. We find no effects on both the log and the level of crimes. Overall, we find that the original LM specification is sensitive to controls, sample lengths, and assumptions on error structures.

On the other end of the debate, AD study the effects of SILs up to 1999 and employ a "hybrid model." In addition to the level shift in a standard DD, they include a trend-break (overall trend interacted with the SIL dummy) term post-SIL to capture the slope change. They find overall positive effects of SILs on violent crimes and positive "long run" effects of SILs suggested by their trend-break term. We argue that, however, in a DD specification, if our state-specific trends are flexible enough, we should not need the trend-break term. Therefore, in our preferred DD specification, we include state-specific quadratic time trends that will capture the "inverted-V" shape argued in this literature. Table 16 presents the results. In column (1), we follow AD and drop the race and age controls. We find an overall increase of about 7.4% in crimes following SIL adoptions. We add the trend-break term in column (2) and find similar results to AD. In (3) and (4), we simply vary the sample length and again find inconsistent results over time. In (5), we add back the race and age controls for comparison. Finally, (6) and (7) are our preferred specifications[22]. We find the opposite effects compared to (1), after controlling for state-specific linear and quadratic time trends and clustering standard errors.

[21]Column (6) corresponds to estimates reported in Table 8.

[22]Column (7) corresponds to estimates reported in Table 8.

Table 15: Replication and Variations of Lott and Mustard (1997)

	(1)	(2)	(3)	(4)	(5)	(6)
SIL Dummy	-0.0881***	-0.0276*	-0.0063	-0.0260†	-0.0015	-0.8789
	(0.0000)	(0.0716)	(0.5885)	(0.1298)	(0.9615)	(0.9550)
Dep. Var.	log	log	log	log	log	level
Sample	1980-1992	1980-1999	1980-2011	1980-1992	1980-1992	1980-1992
Race & age controls	Y	Y	Y	N	N	N
State trends	N	N	N	N	Y	Y
Clustering	N	N	N	N	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y

Notes: all regressions are run on the total violent crimes. Two-sided p values are in parentheses. †, *, **, and *** indicate two-sided statistical significance at the 15, 10, 5, and 1 percent level.

Table 16: Replication and Variations of Ayres and Donohue (2003*b*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SIL Dummy	0.0737***	-0.0832**	-0.0260†	0.1088***	-0.0276*	-0.0385*	-38.0306*
	(0.0000)	(0.0135)	(0.1298)	(0.0000)	(0.0716)	(0.0665)	(0.0582)
SIL Trend		0.0107***					
		(0.0000)					
Dep. Var.	log	log	log	log	log	log	level
Sample	1980-1999	1980-1999	1980-1992	1980-2011	1980-1999	1980-1999	1980-1999
Race & age controls	N	N	N	N	Y	N	N
State trends	N	N	N	N	N	Y	Y
Clustering	N	N	N	N	N	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y

Notes: all regressions are run on the total violent crimes. Two-sided p values are in parentheses. †, *, **, and *** indicate two-sided statistical significance at the 15, 10, 5, and 1 percent level.

B Data Appendix

B.1 State SIL Passage Years

B.2 Age-Specific Arrest and Crime Rates

We first use the BJS national arrests by age groups and the shape-preserving piecewise cubic hermite interpolating polynomials to impute age-specific arrests[23]. Figure 10 presents the fit results for 1980 and 2010 in four crime categories.

To impute age-specific crime rates, let p_{st} be the probability of arrest for criminals in state s and year t, assuming it does not vary across ages. We also assume that every criminal commits κ crimes each year across states, years and ages. Let C be the number of crimes, A the number of arrests, and then we have, by definition, $\frac{C_{ast}}{\kappa} \cdot p_{st} = A_{ast}$, where the subscript a indicates age. Summing over ages and dividing the two equations, we get $\frac{\frac{C_{ast}}{\kappa} p_{st}}{\sum_a \frac{C_{ast}}{\kappa} p_{st}} = \frac{A_{ast}}{\sum_a A_{ast}}$, and after manipulations, $C_{ast} = \frac{A_{ast}}{\sum_a A_{ast}} C_{st}$. We, however, do not observe arrests on the age-state-year level and have to rely on an additional assumption that the arrests for each age group as a fraction of the total arrests do not vary across states, i.e. $\frac{A_{at}}{\sum_a A_{at}} = \frac{A_{ast}}{\sum_a A_{ast}}$. It is plausible that criminals of age 20 in Pennsylvania do no better or worse than those in North Carolina compared to other age groups in the same state. Then we arrive at the desired variable, age-specific crime rates, $C_{ast} = \frac{A_{at}}{\sum_a A_{at}} C_{st}$, where A_{at} are the age-specific national arrests imputed from BJS and C_{st} are the state-year level crime rates data from UCR. We then let the exit cohort $N^{Ex}_{ast} = C_{ast}$.

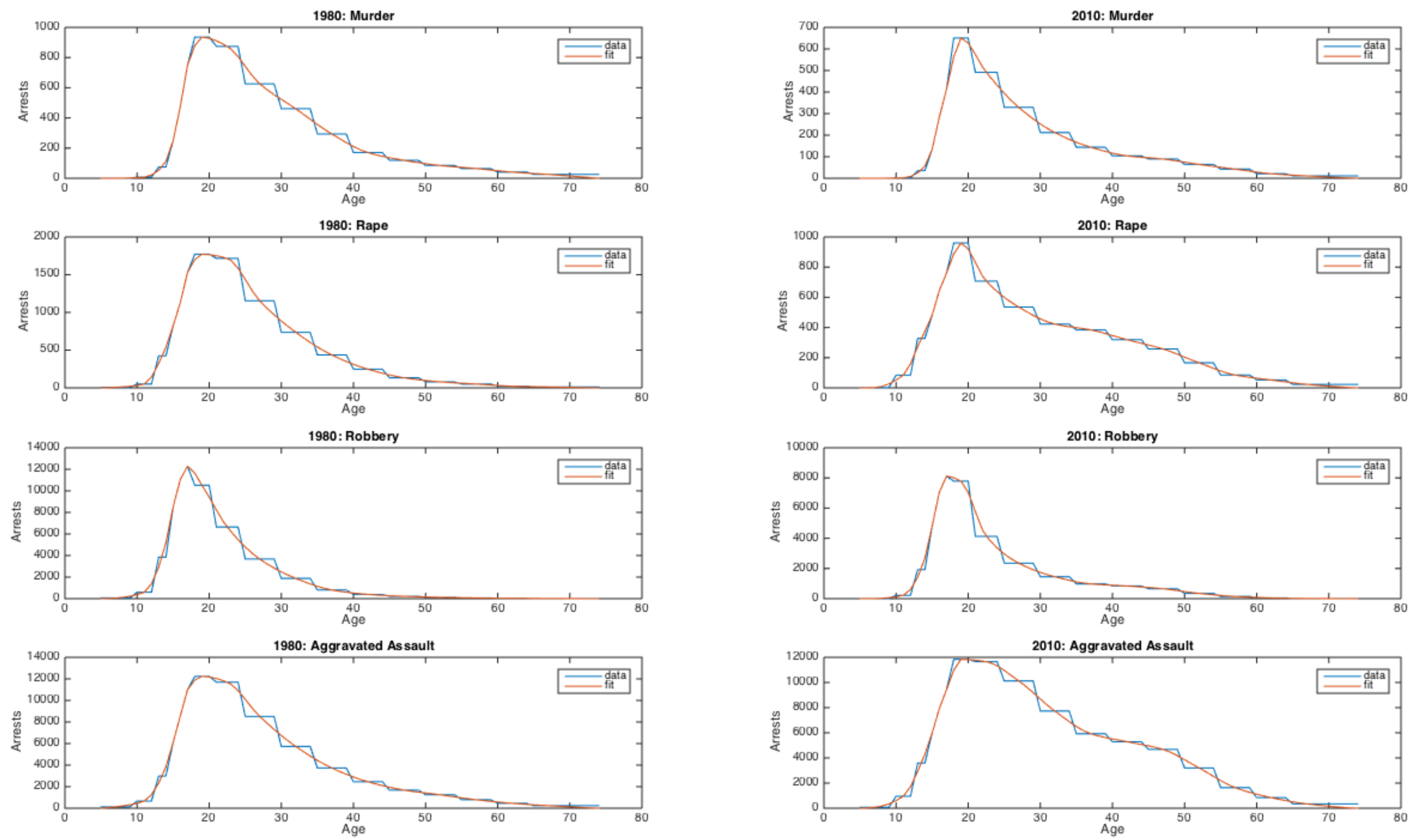
[23]Specifically, we assume there are no violent crimes comitted by people younger than 5 or older than 74. We then assume that the mean age point in an age group has the average arrests in the age group. For example, there are 21 murders for age group 10-12 in 1987 and thus we let the 11-year olds have 7 murders in order to construct our data points. Then we interpolate over these data points using cubic hermite polynomials to impute arrests for each specific age.

Table 17: State SIL Passages

Pre-1985	**1986-1992**	**1995-1997**	**2002-2007**	**Post-2011**
Alabama (*always*)	Maine (*1986*)	Alaska (*1995*)	Michigan (*2002*)	Iowa (*2011*)
Vermont (*always*)	North Dakota (*1986*)	Arizona (*1995*)	Missouri (*2002*)	Wisconsin (*2011*)
New Hampshire (*1960*)	South Dakota (*1987*)	Tennessee (*1995*)	Colorado (*2004*)	California (*never*)
Washington (*1962*)	Florida (*1988*)	Wyoming (*1995*)	Minnesota (*2004*)	Delaware (*never*)
Connecticut (*1970*)	Virginia (*1989*)	Arkansas (*1996*)	New Mexico (*2004*)	Hawaii (*never*)
Indiana (*1981*)	Georgia (*1990*)	Nevada (*1996*)	Ohio (*2005*)	Illinois (*never*)
	Pennsylvania (*1990*)	North Carolina (*1996*)	Kansas (*2007*)	Maryland (*never*)
	West Virginia (*1991*)	Oklahoma (*1996*)	Nebraska (*2007*)	Massachusetts (*never*)
	Idaho (*1991*)	Texas (*1996*)		New Jersey (*never*)
	Mississippi (*1991*)	Utah (*1996*)		New York (*never*)
	Oregon (*1991*)	Kentucky (*1997*)		Rhode Island (*never*)
	Montana (*1992*)	Louisiana (*1997*)		District of Columbia (*never*)
		South Carolina (*1997*)		

Notes: state SIL passage years coding by adoption waves. Years in parentheses indicate the year that state SIL went into effect, typically the following year of law passage. We categorize any concealed carry laws that are equivalent to or more restrictive than may-issue laws (MILs) as non-SILs and laws that are more lenient than SILs as SILs.

Figure 10: Arrests by Age in 1980 and 2010 (# of Arrests)



Notes: rugged blue lines represent the data as we simply divide arrests evenly in an age group. Smooth orange lines represent the well-fitted single-age arrests.