

How do state crime policies affect other states?

The externalities of state DNA database laws

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Abstract

State and local governments control most criminal justice policy in the United States, without regard for potential effects on other states. Lenient state policies might draw in criminals from other states, whereas harsh state policies might drive criminals away to other states. Alternatively, harsh policies might deter or incapacitate criminals who would otherwise migrate to neighboring states and commit crimes there. Thus the effects of a state's policies extend beyond its borders, but these effects could be positive or negative. This paper estimates the externalities of DNA databases. I exploit variation in the timing of database expansions as exogenous shocks to the sizes of state databases, to measure the cross-state effect of database size. The federal government seems to think externalities are positive, since it subsidizes state-level database expansions. I find instead that externalities are negative: increasing the number of other-state profiles increases own-state crime, so that approximately 10 percent of DNA databases' crime-reducing effect is due to states' displacing crime to other states. Effects are larger for nearby states. This result is consistent with the hypothesis that offenders respond rationally to state policies by moving to places where they are less likely to get caught for their crimes.

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1 Introduction

In the United States, a large share of criminal justice policy is made at the state level, though criminal offenders are free to move across state lines. This means that if offenders are mobile, crime-reduction policies in one state could affect crime rates in other states. Since such externalities typically will not be considered by state policy-makers, the equilibrium level of law enforcement will not be socially optimal from a national perspective.

This paper considers the cross-state externalities of state DNA databases. All states have a DNA database that contains DNA profiles of known offenders as well as DNA profiles from crime scenes. Computer algorithms compare these profiles, looking for matches that may identify suspects in unsolved cases. This technology increases the probability that anyone who is added to a DNA database is caught if they reoffend, which deters future crime (Doleac, 2016). DNA databases can also match newly-added offenders to past crimes. Though state databases are linked by the FBI to form a nationally-searchable network, state-level legislation determines which groups are required to provide a DNA sample. Most states started with violent felony convicts and gradually expanded their databases to include additional offenders – property felony convicts, all felony convicts, misdemeanor convicts, felony arrestees, and so on. There has been tremendous variation across states and over time in which individuals are added to state databases (and thus the national network).

This variation provides incentives to individual offenders to move to other states in order to avoid DNA profiling. For instance, suppose Georgia adds felony arrestees to its database, but neighboring Florida does not include that group. Offenders in Georgia who think they could be arrested for a felony now have an incentive to move to Florida to avoid DNA profiling and therefore detection for past or future crimes. Georgia’s database expansion would then displace some of its own crime to Florida. Georgia would probably not include this external cost in its own cost-benefit analysis, and a state database that seems optimal from the state’s perspective will be inefficiently large from the national perspective. (This effect would be compounded if offenders who are newly-added to Georgia’s database move to Florida because they mistakenly believe they cannot be identified there.)

However, expanding state DNA databases could also have positive externalities. If adding an offender to a database in one state stops him from committing crime everywhere – through deterrence or incapacitation – this could reduce crime in other states. In addition, because state databases are searchable by other states, adding an offender in one state can help other states identify him as an offender in local cases. For instance, if Georgia expands its database, Florida could benefit from searching those additional profiles. The expansion of Georgia’s database could help Florida identify and convict more offenders, reducing its own crime rate.

When Georgia is weighing the costs and benefits of expanding its database, it will probably not consider these external benefits, and so its database will be inefficiently small from the national perspective.

The federal government has invested a large amount of money in helping states expand their databases and clear DNA sample backlogs, implying that it expects state database expansions to have positive externalities.¹ However, it is unclear that any positive externalities outweigh any negative externalities between states.

Using an instrumental variable (IV) strategy, I consider the effect of state database expansions on other states' crime rates. The timing of these expansions is independent of other states' crime trends, and so they provide exogenous shocks to the number of offenders in other states who qualify for inclusion in the nationally-searchable database. The larger the shock, in terms of the number of offenders affected, the larger the potential positive and negative externalities on other states.

I find that an additional offender profile from another state increases a state's own violent crime rate and property crime rate. The effect is larger when the offender profile was added by a nearby state than a state that is farther away; this is what we would expect if the increase in crime is due to offenders moving to other states, because nearby states are easier to get to. My preferred specification uses a distance-weighted measure of the treatment provided by other-state profiles. These estimates imply that an additional other-state profile from a state that's 100 miles away increases own-state violent crime by 0.0012 offenses, and property crime by 0.0042 offenses. An additional other-state profile from a state that's 3000 miles away increases own-state crime by 0.00004 offenses and property crime by 0.00014 offenses.

These effects are statistically significant but small relative to the crime-reducing effects of DNA databases. Doleac (2016) used a similar IV strategy to measure the effect of own-state profiles on own-state crime, finding that adding an offender to a state database reduced own-state crime by 0.047 violent offenses and 0.328 property offenses. The results above imply that part (but not all) of this reduction is due to displacement of crime to other states. The net effect of adding an offender to a state database, after accounting for the negative externalities on other states, is a reduction of violent crime by 0.042 offenses and property crime by 0.313 offenses – about 10 percent smaller than the original estimates. This is still a net benefit, but the negative effects on other states imply that the negative externalities of state DNA databases outweigh any positive externalities, and that state databases are therefore inefficiently large from a national perspective.

These results are consistent with a small literature on the externalities of other state-level crime-reduction policies: Iyengar (2008) finds that California's Three-Strikes law, which dramatically increased sentences for individuals with multiple convictions, incentivized some offenders with two strikes to move to (and commit crime in) other states – particularly nearby Arizona and Nevada. Ross (2012) considers the effects of Truth-

¹The National Institute of Justice awarded \$898 million dollars to local agencies for this purpose between 2004 and 2016, as part of the Debbie Smith Act. See <http://www.nij.gov/topics/forensics/lab-operations/evidence-backlogs/Pages/awards-abstracts.aspx> for more information on these awards.

in-Sentencing laws, which require that convicted offenders serve a larger percentage of their original sentence. She finds evidence that the adoption of such policies in one state reduced crime in that state but increased violent crime in adjacent states. Gonzalez-Navarro (2013) finds that the installation of Lojack devices in particular models of cars in some Mexican states displaced motor vehicle thefts to other states.

These studies suggest that a meaningful subset of criminal offenders are willing to relocate to other states when the expected cost of committing crime increases in their current location. This suggests that criminals respond to economic incentives when choosing their state of residence. In this, they resemble non-criminals whose choice of residence is affected by state-varying government benefits or taxes.² However, this finding is at odds with a long criminology literature on situational crime prevention and hot-spot policing, which finds that a diffusion of crime-reduction benefits (positive externalities) is more likely than the displacement of crime (negative externalities), within cities or neighborhoods (see Johnson, Guerette, and Bowers, 2014, and Telep, Weisburd, Gill, Vitter, and Teichman, 2014, for reviews). The conclusion of that literature is that offender mobility is low, at least when deciding which block or neighborhood in their city to target. It could be that individuals who commit crime respond rationally to incentives to relocate, but their crime-committing behavior in any particular location is more influenced by opportunity and state of mind (for instance, inebriation) than cost-benefit analysis.

It could also be that local evaluations miss displacement because they focus on too narrow a geographic area. If offenders aren't sure of the boundaries of a police intervention, and so avoid committing crime nearby, the observed effect would be a diffusion of crime-reduction benefits to nearby neighborhoods (as is often observed), but displacement of crime farther away (out of view to the researcher). Evaluations of state-level interventions may avoid this problem because the state-line boundaries are clearer to offenders.

This paper makes the following contributions: (1) It adds to the literature on DNA databases by incorporating externalities into estimates of DNA database effectiveness. (2) It tests the implicit assumption by the federal government that DNA databases have positive externalities on other states, and finds that they in fact have negative externalities. (3) It adds to a broader literature on the spillover effects of crime policies by showing that offenders may be much more mobile than previously thought. (4) Finally, it adds to a long literature on the externalities of state-level policies in a federalist system.

In the next section, I discuss the existing literature on DNA databases and related policies. Section 3 describes the data used in this analysis. Section 4 explains my empirical strategy. Section 5 presents the results. Section 6 discusses those results and concludes.

²See for example Gelbach, 2004; Bailey, 2005; McKinnish, 2005; and De Giorgi and Pellizzari, 2009 For evidence to the contrary see Goodman, 2016.

2 DNA databases

DNA databases are one of a growing number of crime-reduction policies that aim to increase the probability that an offender gets caught. This is in contrast with policies such as sentence enhancements, sentencing guidelines, Three-Strikes laws, and Truth-in-Sentencing laws, all of which aim to increase the punishment an offender receives if he gets caught. There is increasing evidence that the former set of policies can be more cost-effective than the latter, due to strong deterrent effects. For instance, Kilmer, Nicosia, Heaton, and Midgette (2013) discuss a program called 24/7 Sobriety that involves frequent, random drug tests for probationers; those who fail the test receive short but immediate jail sentences. This program dramatically reduces DUIs and other substance-related offenses. Many similar policies take advantage of advancements in technology: for instance, electronic monitoring (see DiTella and Schargrodsky, 2013, and Marie, 2015), body-worn cameras for police officers (Ariel, Farrar, and Sutherland, 2014), predictive policing, surveillance cameras, NIBIN databases that match bullets across crime scenes, and gunshot sensors (Carr and Doleac, 2016), are increasingly common in U.S. police departments. Where evaluations of such technologies exist, they tend to find large deterrent effects on crime. This is consistent with the hypothesis that criminal offenders heavily discount the future, and so short-term consequences are more effective deterrents than long-term consequences (Mastrobuoni and Rivers, 2016).

Most relevant to the present study, Doleac (2016) uses a regression discontinuity design to test the effect of adding offenders to DNA databases on those offenders' subsequent behavior. That paper shows that DNA databases have a dramatic deterrent effect on crime in the U.S., reducing recidivism by 17% for violent offenders and 6% for property offenders. A recent working paper, Doleac, Landersø, and Tegner Anker (2016), finds similar effects on recidivism in Denmark. The earlier paper also uses an IV strategy similar to that employed in the present study, to measure effects of DNA databases on crime rates. It finds that the growth in the average state database between 2000 and 2010 reduced violent crime by at least 7 percent and property crime by at least 5 percent. That result implies that DNA databases are extremely cost-effective relative to more labor-intensive alternatives such as hiring additional police officers and lengthening prison sentences. It appears the effects are driven more by deterrence of crime than an increased detection (and incapacitation) of offenders.

A common misperception about DNA databases is that they are only useful for solving violent crimes. A field experiment described in Roman, Reid, Reid, Chalfin, Adams, and Knight (2008) demonstrated the effectiveness of DNA databases in helping to solve property crimes such as burglary. They randomly assigned crime scenes to have DNA evidence analyzed and uploaded to the database (treatment group) or not (control group); crimes in the treatment group were much more likely to be solved. Right now (in 2016),

police departments around the world are just beginning to regularly analyze DNA evidence from property crime scenes; there have historically been large backlogs of DNA evidence at crime labs, with non-violent property offenses constantly pushed to the bottom of the triage pile. Backlogs will be less of a problem as DNA technology improves. Indeed, "rapid DNA" technologies, allowing DNA profiling within hours instead of weeks or months, may soon be the norm in police departments around the country. This should increase the deterrent effect of DNA profiling even further. Such reductions in crime benefit the states that enact these policies. The question addressed in the present study is what consequences these policies have for other states.

3 Data

To test the effect of other-state DNA profiles on own-state crime, I need annual data on state database sizes, instruments for database size, and state crime rates.

Historical data on state database sizes are generally unavailable; they are not systematically tracked by states. I collected these data over time from state crime labs, FBI statistics, and media reports, including every state-year that information on database size was available, focusing on the years 2000 to 2014. The resulting data likely include measurement error but instrumenting for them will remove any attenuation bias.

Database size is endogenous: it is simultaneously determined with state crime rates, and depends on states' adeptness and motivation in implementing state database laws. (Many states were slow to analyze the DNA samples taken from offenders, resulting in large backlogs and databases that did not grow as quickly as intended by law.) I follow Doleac (2016) and construct instrumental variables by estimating the number of offenders who qualify for inclusion in a state's database each year (hereafter "qualifying offenders"), based on pre-period (1999) state-level data on arrests, convictions, and prison populations, as well as information on the timing of state database expansions. Arrest estimates are based on the 1999 Uniform Crime Reports (UCR) multiplied by 1999 clearance rates, by crime type. Conviction estimates are based on those arrest estimates, multiplied by the share of arrestees who are ultimately convicted, by crime type, in the 2000 State Court Processing Statistics. State prison populations are from the 1999 National Jail Census. Due to limits on data availability, I am not able to construct instruments for three small states: Rhode Island, Connecticut, and Vermont. These states are therefore excluded from the analysis.

Information on the timing and details of state database expansions comes directly from state legislative histories. The maps in Figure 1 show the evolution of the number of qualifying offenders (the instrument) across states, over time. Databases gradually grow everywhere, but there is a large amount of variation in database size within region at any given time.

I construct three measures of other-state database sizes as the treatment: (1) total other-state profiles, (2) other-state profiles in nearby states only (those within 500 miles), and (3) total other-state profiles divided by the distance in miles to each state (that is, weighted by the inverse of the distance between states, so that nearby states get more weight). For the distance between states, I use Google maps to determine the shortest driving distance, in miles, from the center of each state to the center of each other state. This proxies for the difficulty of relocating from one state to the other. I exclude Alaska and Hawaii because they are removed from the other contiguous 48 states.

For outcome measures, I use Uniform Crime Report (UCR) data on reported crime and National Incident-Based Reporting System (NIBRS) data on arrests in reported crimes. In the latter, I create an indicator variable for whether an arrest was made in a particular case, then (because the effect sizes are very small) multiply that indicator by 100 so that the estimated effects are on a 0-100 scale instead of 0-1.

All crime, database size, and qualifying offender variables are converted to rates, so that the numbers used are per 100,000 own-state residents.

The resulting panel dataset consists of 660 state-year observations. Summary statistics are in Table 2.

4 Empirical Strategy

I follow the IV strategy in Doleac (2016) to exploit exogenous variation in state database size. That study shows that the timing of state database expansions is exogenous with respect to underlying crime trends in those states. However, the implementation of those laws (the rate at which qualifying offenders' profiles are analyzed and uploaded to the national database) might not be, which could introduce omitted variable bias to simple OLS estimates of crime rates on database size. In addition, state database size and crime rates are simultaneously determined; this biases OLS estimates of the effect of database size upwards. I use the timing of state database expansions, along with pre-period convict, arrest, and incarceration rates, to estimate the number of qualifying offenders in each state in each year. I then use that number as an instrument for actual database size in each state. This isolates the "good" (i.e., exogenous) variation in database size from the "bad" variation stemming from differences in implementation, removing the omitted variable bias. Using pre-period rates removes the simultaneity bias.

Table 1 shows the effects of state characteristics – including pre-period crime rates and the timing of a state's own database expansions – on the timing of nearby states' database expansions. Very few of the estimates are statistically significant, and there are no systematic patterns in the signs of the effects. It seems that the timing of nearby states' expansions are largely unrelated to the crime context of any given state. This supports the assumption that other-state database expansions serve as exogenous shocks to own-state

crime.

I first consider the effect of the number of other-state profiles in the national DNA database network (known as CODIS) on own-state crime outcomes. I use each of the three treatment variables described above, in an IV framework.

The baseline 2SLS IV specification is:

$$\text{Own-state crime rate}_{s,t} = \beta_1 + \beta_2 * \text{Other-state profile rate}_{s,t} + \beta_3 * \text{Own-state qualifying offender rate}_{s,t} + \gamma_{State} + \delta_{Year*Region} + e_{s,t}, \quad (1)$$

where *Other-state profile rate* is estimated in the first-stage regression:

$$\text{Other-state profile rate}_{s,t} = \alpha_1 + \alpha_2 * \text{Other-state qualifying offender rate}_{s,t} + \alpha_3 * \text{Own-state qualifying offender rate}_{s,t} + \gamma_{State} + \delta_{Year*Region} + e_{s,t}, \quad (2)$$

In both specifications, s indexes states, and t indexes years. State fixed effects, γ_{State} , control for average differences in state crime rates. Year-by-region fixed effects, $\delta_{Year*Region}$, control for annual variation in average crime rates, at the Census region level. The identification therefore comes from within-region-year variation in other-state profiles. In all specifications, I control for own-state qualifying offenders (the instrument, not actual database size), to account for own-state policy effects on own-state crime. Standard errors are clustered at the state level. The coefficient of interest is β_2 , which measures the marginal effect of an other-state DNA profile on own-state crime.

5 Results

First stage results are presented in Table 3. Each version of the instrument (total other-state qualifying offender rate, nearby-state qualifying offender rate, and distance-weighted qualifying offender rate) is highly correlated with the actual number of profiles from other states. F-statistics range from 1,199 to 426,897, implying that the instruments are extremely strong.

The effects of other-state profiles on own-state crime are show in Table 4, using the IV strategy described above. The first panel shows effects on violent crime (aggregating murder, forcible rape, aggravated assault, and robbery). The second panel shows effects on property crime (aggregating burglary, larceny, and motor vehicle theft).

In all specifications I control for the own-state qualifying offender rate (the instrument for own-state

database size). Note that the effect of own-state qualifying offenders is both negative and statistically significant in all cases. While the magnitude of the coefficient cannot be directly compared with the magnitudes of the other coefficients in the table (because it is the instrument, not actual database size), this implies that adding additional offenders to a state database reduces crime in that state. This echoes the findings of Doleac (2016).

Column 1 shows the effect of a marginal other-state profile, without regard to which other state it was from. The effect is positive and statistically significant. It implies that an other-state profile *increases* own-state violent crime by 0.0001 offenses, and own-state property crime by 0.0003 offenses.

Column 2 restricts attention to the effects of nearby states – defined here as those less than 500 miles away.³ If the mechanism is offenders traveling between states, we would expect the effect of nearby states' databases to be larger than that of more distant states' databases. Indeed, that is what we find. An additional profile in nearby states' databases increases own-state violent crime by 0.0011 offenses, and own-state property crime by 0.0063 offenses – larger effects than we saw in Column 1.

Column 3 considers the same issue slightly differently, using a distance-weighted measure of other states' database sizes. Each other state's database is multiplied by the inverse of the distance to the state of interest, so that states farther away get less weight than states nearby. For instance, an additional profile in a state 3000 miles away will count as 0.0003 profiles, while an additional profile 100 miles away counts as 0.01 profiles. We again see that other-state profiles have a positive and statistically-significant effect on both violent crime and property crime. The coefficients imply that an additional profile 100 miles away increases own-state violent crime by 0.0012 offenses and property crime by 0.0042 offenses, while an additional profile 3000 miles away increases own-state violent crime by 0.00004 offenses and property crime by 0.00014 offenses.

Tables 5 and 6 show the main effects separately by specific crime type. We see that other-state profiles consistently have a positive effect on own-state crime in all cases, though the proximity of the state matters more for the crimes of burglary and larceny. For those crimes, only profiles from nearby states have a statistically-significant effect on own-state crime.

5.1 Other results

Next, I consider the differential effects by type of other-state profile. Of particular interest in the current policy environment is whether arrestee profiles are more or less valuable than convict profiles. Arrestee profiles could be more valuable if arrestees are more likely to be active offenders, profiled early in their criminal careers. They could be less valuable if arrests are a noisy measure of criminal activity, so that the average arrestee is less of a threat to public safety than the average convict. To test this, I interact the

³This result is not sensitive to the precise definition of "nearby."

treatment (profiles) and the IV (qualifying offenders) with an indicator of whether arrestees were included in the relevant other-state database in that year. CODIS did not allow arrestee profiles until 2006, which provides useful variation in addition to state-level policy changes.

Table 7 considers the differential effects of other-state profiles when arrestees are included in other-state databases. Columns 1–3 replicate the main results with an interaction term for whether arrestees are included in each state’s database. Overall, those interaction terms are not statistically significant, which means I cannot reject the null hypothesis that arrestee profiles have the same effect on other-state crime that convict profiles do. However, estimates are non-zero for distance-weighted profiles, suggesting that arrestee profiles have larger negative externalities with respect to violent crime, and smaller negative externalities with respect to property crime. It is important to note here that I do not observe *how many* arrestee profiles are in each state’s database. I only observe whether arrestees were part of the overall total. Having more detailed information on the composition of state databases would allow more precise estimates.

Finally, I test for effects of other-state profiles on the probability of making arrests in own-state cases. If CODIS is helping states solve more crimes, we would expect other-state profiles to have a positive effect on own-state rates of crime solving. In this analysis, standard errors are clustered at the police department level.

Table 8 shows the effect of other-state profiles on the probability of making arrests in own-state crimes. Recall that the outcome is measured on a 0-100 scale (instead of 0-1) to make the table more readable. There are no statistically significant effects in any specification. This suggests that cross-state CODIS searches do not meaningfully affect clearance rates in new offenses.

6 Discussion

The cross-state effects of DNA database policies are consistent with the hypothesis that offenders are mobile and respond rationally to states’ crime-reduction strategies. In particular, it appears that a DNA expansion in one state incentivizes offenders in that state to move elsewhere, which increases crime rates in other states. In other words, DNA databases’ negative effects on own-state crime appear partly due to shifting crime to other states.

Doleac (2016) estimated that a marginal own-state profile reduces own-state violent crime by 0.047 offenses and own-state property crime by 0.328 offenses. Here I estimate that a marginal other-state profile increases own-state crime by 0.0001 violent offenses, and 0.0003 property offenses. To get the net effect of a marginal own-state profile on national crime, we can multiply the other-state effect by 49 and add the own-state effect. This suggests that a marginal DNA profile reduces violent crime by 0.042 offenses, and

property crime by 0.313 offenses. That is still a net benefit, though it is about 10 percent smaller than the benefit perceived by the state uploading the profile.

This has implications for federal DNA database policy. As long as state database laws vary, adding additional offenders in one state will have negative consequences for other states. Individual states weighing the costs and benefits of database expansions will therefore settle on databases that are inefficiently large. Federal programs that decrease the costs by funding state expansions exacerbate this problem. To reduce these inefficiencies, the federal government could stop subsidizing state-level expansions, or standardize DNA database policies nationwide so that offenders have no incentive to move.

These results have broader implications for U.S. criminal justice policy and the evaluation of policy effects. Most crime policy is determined at the state level, but the social efficiency of those policies is partly dependent on their effects on other states. If criminal offenders were not mobile, cross-state externalities would be negligible, and states would reach efficient outcomes independently. This study suggests externalities are important. Research on policy effects should be more careful to consider external effects of state and local policies; in particular, to what degree are local reductions in crime due to shifting crime elsewhere? Addressing this issue can be difficult since detailed state and local data on crime and criminal justice policies are often unavailable nation-wide. These results show that improved data availability is critical for evaluating the full, and sometimes unintended, effects of criminal justice policies.

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7 Figures and Tables

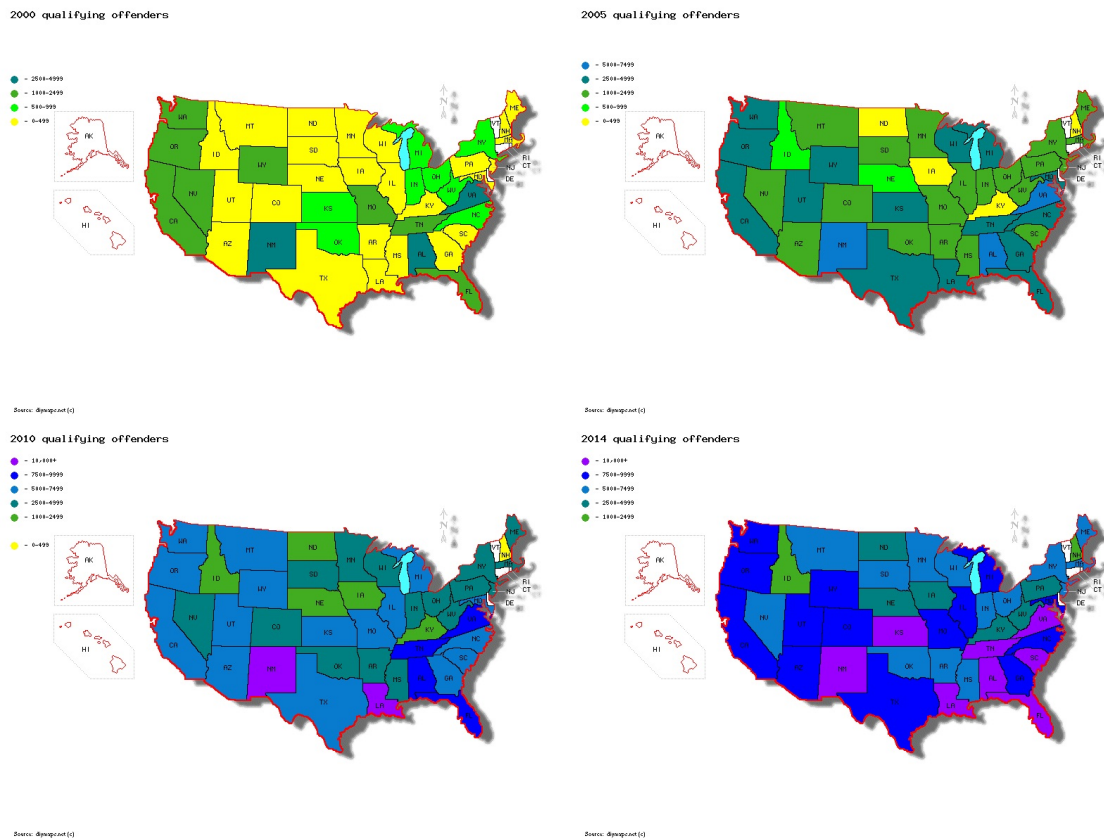


Figure 1: Variation in number of qualifying offenders in each state over time

Number of qualifying offenders is per 100,000 state residents. Numbers are estimates based on state laws and number of arrestees and convicted and released offenders, as described in the text. Database sizes range from less than 500 qualifying offenders per 100,000 residents (yellow) to 2500-4999 qualifying offenders per 100,000 residents (dark green) to 10,000 or more qualifying residents per 100,000 residents (purple).

Table 1: Effect of state characteristics on timing of nearby states' database expansions

	Sex felony convicts		Violent felony convicts		Burglary convicts		All felony convicts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1999 Violent Crime Rate	-0.0009		0.0004		-0.0011		-0.0022*	
	(0.0010)		(0.0015)		(0.0010)		(0.0013)	
Year sex felony convicts added		0.1280		0.3537***		-0.0773		-0.0159
		(0.0886)		(0.1260)		(0.0840)		(0.1183)
Year violent felony convicts added		0.0749		0.0319		-0.0436		-0.1275
		(0.0624)		(0.0889)		(0.0591)		(0.0832)
Year burglary convicts added		-0.1037		-0.0157		-0.1400*		-0.0227
		(0.0822)		(0.1169)		(0.0780)		(0.1097)
Year all felony convicts added		0.0305		-0.1112		0.0544		-0.0775
		(0.0723)		(0.1028)		(0.0686)		(0.0965)
Year sex felony arrestees added		-0.1574		0.0689		0.1007		0.1035
		(0.0959)		(0.1362)		(0.0909)		(0.1279)
Year violent felony arrestees added		0.1215		-0.1790		0.0432		0.0648
		(0.1605)		(0.2281)		(0.1521)		(0.2141)
Year burglary arrestees added		-0.0768		-0.1949		-0.1429		-0.0244
		(0.1480)		(0.2103)		(0.1403)		(0.1975)
Year all felony arrestees added		0.1564		0.2628*		-0.0634		-0.0703
		(0.0936)		(0.1330)		(0.887)		(0.1248)
	Sex felony arrestees		Violent felony arrestees		Burglary arrestees		All felony arrestees	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1999 Violent Crime Rate	-0.0021		-0.0009		-0.0002		-0.0003	
	(0.0017)		(0.0015)		(0.0014)		(0.0012)	
Year sex felony convicts added		-0.0234		0.0436		-0.0290		-0.0161
		(0.1568)		(0.1331)		(0.1210)		(0.1056)
Year violent felony convicts added		-0.1954*		-0.2085**		-0.1780**		-0.0771
		(0.1104)		(0.0937)		(0.0851)		(0.0743)
Year burglary convicts added		-0.0349		-0.0368		-0.1344		-0.0418
		(0.1455)		(0.1235)		(0.1122)		(0.0979)
Year all felony convicts added		0.0823		0.0490		0.0919		-0.0040
		(0.1280)		(0.1086)		(0.0987)		(0.0861)
Year sex felony arrestees added		0.0269		0.0466		0.0856		0.2001*
		(0.1696)		(0.1439)		(0.1308)		(0.1141)
Year violent felony arrestees added		-0.1150		-0.0706		-0.1991		-0.1457
		(0.2839)		(0.2410)		(0.2190)		(0.1911)
Year burglary arrestees added		0.1549		0.0989		0.2378		0.1345
		(0.2618)		(0.2223)		(0.2020)		(0.1762)
Year all felony arrestees added		0.1311		0.0737		0.0000		-0.1240
		(0.1655)		(0.1405)		(0.1277)		(0.1114)
Observations	43							

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome is the mean database expansion year in nearby states (less than 500 miles away). A state's start year set to 2015 if the relevant law had not been adopted by 2014. Violent crime rate data source: UCR. Expansion date data source: state legislative histories.

Table 2: Summary Statistics

	State-year observations	Mean	SD
Year	660	2007	4.3238
Violent crime rate	660	390.53	161.37
Property crime rate	660	3161.2	799.06
Other states' profile rate	660	172498	278713
Nearby states' profile rate	660	13420	19562
Distance-weighted profile rate	660	170.73	242.60
Instruments			
Other states' qualifying offender rate	660	372609	518556
Nearby states' qualifying offender rate	660	29225	35873
Distance-weighted qualifying offender rate	660	371.87	446.66
Own-state qualifying offender rate	660	3760.6	3111.4

Notes: Crime rate data source: FBI UCR, 2000-2014. Profile rate measures DNA database size, per 100,000 state residents. Qualifying offender rate measures the number of people in the state who qualify for inclusion in the database based on state law and pre-period conviction and arrest rates.

Table 3: First Stage for Instrumental Variable Regressions

	Other states' profiles	Nearby states' profiles	Distance-weighted profiles
Other states' qualifying offenders	0.6321*** (0.0010)		
Nearby states' qualifying offenders		0.6087*** (0.0176)	
Distance-weighted qualifying offenders			0.6218*** (0.0067)
State-year observations	660	660	660
F-statistic	426897	1199	8656

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 4: Effect of Other-State Profiles on Own-State Crime

	(1)	(2)	(3)
Violent Crime			
Other states' profiles	0.0001*** (0.0000)		
Nearby states' profiles		0.0011*** (0.0003)	
Distance-weighted profiles			0.1273*** (0.0417)
Own qualifying offenders	-0.0155*** (0.0033)	-0.0172*** (0.0042)	-0.0148*** (0.0032)
2000 violent crime rate	426.83		
State-year observations	660	660	660
Property Crime			
Other states' profiles	0.0003** (0.0002)		
Nearby states' profiles		0.0063*** (0.0021)	
Distance-weighted profiles			0.4241** (0.1998)
Own qualifying offenders	-0.0743** (0.0377)	-0.0711** (0.0349)	-0.0706* (0.0387)
2000 property crime rate	3558.8		
State-year observations	660	660	660

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 5: Effect of Other-State Profiles on Own-State Violent Crime

	(1)	(2)	(3)
Murder			
Other states' profiles	0.0000*** (0.0000)		
Nearby states' profiles		0.0000** (0.0000)	
Distance-weighted profiles			0.0015*** (0.0003)
Own profiles	-0.0001*** (0.0000)	-0.0001*** (0.0000)	-0.0001*** (0.0000)
Rape			
Other states' profiles	0.0000*** (0.0000)		
Nearby states' profiles		0.0001*** (0.0000)	
Distance-weighted profiles			0.0117*** (0.0037)
Own profiles	-0.0004* (0.0002)	-0.0006** (0.0002)	-0.0003* (0.0002)
Assault			
Other states' profiles	0.0001** (0.0000)		
Nearby states' profiles		0.0006** (0.0003)	
Distance-weighted profiles			0.0840** (0.0341)
Own profiles	-0.0108*** (0.0029)	-0.0123*** (0.0036)	-0.0104*** (0.0029)
Robbery			
Other states' profiles	0.0000*** (0.0000)		
Nearby states' profiles		0.0004*** (0.0001)	
Distance-weighted profiles			0.0301*** (0.0066)
Own profiles	-0.0042*** (0.0008)	-0.0043*** (0.0008)	-0.0040*** (0.0007)
State-year observations	660	660	660

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 6: Effect of Other-State Profiles on Own-State Property Crime

	(1)	(2)	(3)
Burglary			
Other states' profiles	0.0000 (0.0000)		
Nearby states' profiles		0.0012** (0.0005)	
Distance-weighted profiles			0.0632* (0.0383)
Own profiles	-0.0129** (0.0065)	-0.0116* (0.0061)	-0.0122* (0.0066)
Larceny			
Other states' profiles	0.0001 (0.0001)		
Nearby states' profiles		0.0035*** (0.0013)	
Distance-weighted profiles			0.1706 (0.1414)
Own profiles	-0.0435* (0.0243)	-0.0387* (0.0229)	-0.0411* (0.0250)
Vehicle Theft			
Other states' profiles	0.0002*** (0.0000)		
Nearby states' profiles		0.0017** (0.0007)	
Distance-weighted profiles			0.1903*** (0.0455)
Own profiles	-0.0179** (0.0087)	-0.0207** (0.0081)	-0.0173* (0.0088)
State-year observations	660	660	660

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 7: Differential Effects of Other-State Profiles on Own-State Crime

	(1)	(2)	(3)
Violent crime			
Other states' profiles	0.0001** (0.0000)		
Other states' profiles * Arrestees included	0.0000 (0.0001)		
Nearby states' profiles		0.0010** (0.0004)	
Nearby states' profiles * Arrestees included		0.0001 (0.0006)	
Distance-weighted profiles			0.0765 (0.0468)
Distance-weighted profiles * Arrestees included			0.0733 (0.0839)
Own profiles	-0.0155*** (0.0033)	-0.0173*** (0.0043)	-0.0149*** (0.0032)
State-year observations	660	660	660
Property crime			
Other states' profiles	0.0002 (0.0004)		
Other states' profiles * Arrestees included	0.0001 (0.0006)		
Nearby states' profiles		0.0061*** (0.0021)	
Nearby states' profiles * Arrestees included		0.0004 (0.0037)	
Distance-weighted profiles			0.6055* (0.3449)
Distance-weighted profiles * Arrestees included			-0.2621 (0.5739)
Own profiles	-0.0742** (0.0377)	-0.0713** (0.0347)	-0.0701* (0.0388)
State-year observations	660	660	660

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses. Coefficients show the effect of an other-state DNA profile on own-state crime. Crime data source: UCR 2000-2014. Database size data source: Collected by the author from a variety of sources. Instrumental variables are constructed as described in the text.

Table 8: Effect of Other-State Profiles on Own-State Arrest Probability

	(1)	(2)	(3)
Violent Crime			
Other states' profiles	-0.0000 (0.0000)		
Nearby states' profiles		-0.0001 (0.0000)	
Distance-weighted profiles			-0.0033 (0.0049)
Own qualifying offenders	-0.0000 (0.0005)	-0.0001 (0.0006)	-0.0001 (0.0005)
2000 violent crime arrest probability	37.877		
State-year observations	2555344	2555344	2555344
Property Crime			
Other states' profiles	0.0000 (0.0000)		
Nearby states' profiles		-0.0000 (0.0000)	
Distance-weighted profiles			0.0021 (0.0027)
Own qualifying offenders	-0.0000 (0.0003)	-0.0001 (0.0003)	-0.0000 (0.0003)
2000 property crime arrest probability	12.099		
State-year observations	20307252	20307252	20307252

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the police department level, are shown in parentheses.

Table 9: Effect of Other-State Profiles on Own-State Crime – Dropping states

	Drop AL	Drop AZ	Drop AR	Drop CA	Drop CO
Violent Crime					
Distance-weighted profiles	0.1265*** (0.0418)	0.1273*** (0.0419)	0.1237*** (0.0414)	0.1207*** (0.0426)	0.1248*** (0.0434)
Property Crime					
Distance-weighted profiles	0.4219** (0.1997)	0.3738* (0.1986)	0.4064** (0.1982)	0.4972** (0.1918)	0.5980*** (0.1834)
State-year observations	645	645	645	645	645
	Drop FL	Drop GA	Drop ID	Drop IL	Drop IN
Violent Crime					
Distance-weighted profiles	0.1212*** (0.0408)	0.1287*** (0.0421)	0.1246*** (0.0424)	0.1187*** (0.0389)	0.1285*** (0.0424)
Property Crime					
Distance-weighted profiles	0.4067** (0.1980)	0.4276** (0.2008)	0.4133** (0.2055)	0.4221** (0.2000)	0.4369** (0.2044)
State-year observations	645	645	645	645	645
	Drop IA	Drop KS	Drop KY	Drop LA	Drop ME
Violent Crime					
Distance-weighted profiles	0.1271*** (0.0416)	0.1269*** (0.0420)	0.1266*** (0.0417)	0.1229*** (0.0416)	0.1240*** (0.0423)
Property Crime					
Distance-weighted profiles	0.4250** (0.2019)	0.4250** (0.2019)	0.4341** (0.2064)	0.4046** (0.2046)	0.4170** (0.2049)
State-year observations	645	645	645	645	645
	Drop MD	Drop MA	Drop MI	Drop MN	Drop MS
Violent Crime					
Distance-weighted profiles	0.1275*** (0.0416)	0.1281*** (0.0421)	0.1255*** (0.0419)	0.1276*** (0.0424)	0.1272*** (0.0420)
Property Crime					
Distance-weighted profiles	0.4245** (0.2001)	0.4249** (0.2003)	0.4210** (0.1999)	0.4300** (0.2026)	0.4327** (0.2001)
State-year observations	645	645	645	645	645

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the state level, are shown in parentheses.

Table 10: Effect of Other-State Profiles on Own-State Crime – Dropping states

	Drop MO	Drop MT	Drop NE	Drop NV	Drop NH
Violent Crime					
Distance-weighted profiles	0.1268*** (0.0417)	0.1275*** (0.0434)	0.1289*** (0.0416)	0.1333*** (0.0397)	0.1322*** (0.0468)
Property Crime					
Distance-weighted profiles	0.4233** (0.1996)	0.4005** (0.1967)	0.4363** (0.1958)	0.4163** (0.2042)	0.4075* (0.2091)
State-year observations	645	645	645	645	645
	Drop NJ	Drop NM	Drop NY	Drop NC	Drop ND
Violent Crime					
Distance-weighted profiles	0.1269*** (0.0421)	0.1276*** (0.0438)	0.1253*** (0.0425)	0.1270*** (0.0419)	0.1108*** (0.0425)
Property Crime					
Distance-weighted profiles	0.4125** (0.1991)	0.3436* (0.1750)	0.4236** (0.2043)	0.4122** (0.1991)	0.3968* (0.2174)
State-year observations	645	645	645	645	645
	Drop OH	Drop OK	Drop OR	Drop PA	Drop SC
Violent Crime					
Distance-weighted profiles	0.1275*** (0.0427)	0.1274*** (0.0417)	0.1294*** (0.0413)	0.1275*** (0.0425)	0.1313*** (0.0419)
Property Crime					
Distance-weighted profiles	0.4357** (0.2060)	0.4209** (0.1991)	0.4054** (0.2027)	0.4286** (0.2038)	0.4159** (0.2005)
State-year observations	645	645	645	645	645
	Drop SD	Drop TN	Drop TX	Drop UT	Drop VA
Violent Crime					
Distance-weighted profiles	0.1133** (0.0447)	0.1272*** (0.0418)	0.1274*** (0.0424)	0.1291*** (0.0406)	0.1281*** (0.0421)
Property Crime					
Distance-weighted profiles	0.3632* (0.2144)	0.4226** (0.1997)	0.4023** (0.1987)	0.4267** (0.1997)	0.4282** (0.2026)
State-year observations	645	645	645	645	645
	Drop WA	Drop WV	Drop WI	Drop WY	
Violent Crime					
Distance-weighted profiles	0.1326*** (0.0411)	0.1231*** (0.0432)	0.1319*** (0.0432)	0.1803*** (0.0255)	
Property Crime					
Distance-weighted profiles	0.4393** (0.2035)	0.4046** (0.2002)	0.4320** (0.2038)	0.5890*** (0.2034)	
State-year observations	645	645	645	645	

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the state level, are shown in parentheses.