The effects of DNA databases on the deterrence and detection of offenders

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Abstract

This paper studies how collecting offender DNA profiles affects offenders' later recidivism and likelihood of getting caught by exploiting a large expansion of Denmark's DNA database. We find that DNA profiling increases detection probability and reduces recidivism within the following year by as much as 43%. We estimate the elasticity of criminal behavior with respect to the probability of detection to be -1.7, implying that a 1% higher detection probability reduces crime by almost 2%. We also find that DNA profiling changes non-criminal behavior: profiled offenders are more likely to engage in a stable relationship, and live with their children.

1 Introduction

Surveillance technologies have the potential to improve public safety by increasing the probability that offenders are caught for their crimes, thereby deterring criminal behavior. It may also take serial offenders who are not deterred off the streets more quickly. While the existence and direction of these effects have much support in the literature, we currently know very little about precisely how much deterrence we achieve for any given increase in the likelihood that an offender is apprehended. Such knowledge is essential in deciding how best to use scarce law enforcement dollars.

Yet, estimating the effects of law enforcement or surveillance tools on criminal behavior is inherently difficult, not only because crime is related to many (to the researcher) unobservable characteristics, but because we only observe that someone offends if he is identified by police. If measures of offenses, arrests, charges, or convictions were unrelated to the likelihood of apprehending the offender, this would only reduce the precision of our estimates. But because we only observe an offender's criminal behavior precisely when the offender is identified as the culprit, improvements in detection lead to an upward bias when we estimate surveillance tools' effects on recidivism. In some cases, detected crime may increase even as actual crime falls.

This paper addresses these issues by studying the causal effects of adding offenders to a DNA database — which is meant to increase the likelihood of detection by matching offender profiles with crime scene evidence — on deterrence from subsequent crime and the likelihood that recidivism is indeed detected. From these results we also provide a first causal estimate of the elasticity of crime with respect to detection probability. The paper is based on Danish register data and uses a 2005 reform that dramatically increased the size of Denmark's DNA database as an instrumental variable for whether an individual was added to the database. The change allowed police to add anyone charged with what is roughly equivalent to a felony in the U.S. (the relevant policy margin for most U.S. states considering database expansions)

to the DNA database, increasing offenders' average probability of being included in the database from 4% to almost 40%.

To analyze the behavioral effects of the database, we exploit the fact that it takes time (at least 3 weeks) to analyze and upload crime scene evidence to the database and find a match, together with the unique rich Danish register data on the timing of all subsequent reported offenses and charges. We distinguish new charges that were filed more than three weeks after an offense – and therefore might have been aided by the DNA database – from charges that came more quickly after the offense, and could not have been aided by a database match. The first set of (slow) charges is affected by both the deterrence and detection effects of DNA databases, but the second set of (fast) charges provide a clean estimate of deterrence, which we use to separate the deterrence and detection effects of the DNA database. Using the database expansion as an exogenous shock to the likelihood of DNA registration, we estimate that being added to the DNA database reduces recidivism by 5.7 percentage points (43%) in the first year ($p < 0.01$). That deterrence effect persists for at least three years after the initial charge, and is strongest for those charged with violent offenses.

We use these estimated effects on 'fast' and 'slow' charges to separate the detection effect of DNA databases, and estimate the elasticity of criminal behavior with respect to detection probability. We estimate a statistically significant detection effect implying that police identify the offender of a crime 3-4% more often due to DNA profiling. Estimates increase over time and indicate statistically significant effects for both property offenses and violent offenses. The magnitude of the detection effect suggests that economically meaningful deterrence effects could be missed if the two effects of surveillance technologies are not separately identified. These separate estimates of the deterrence and detection effects imply an elasticity of crime with respect to detection probability of -1.7 over three years, among those who have been charged at least once before.

From the rich Danish register data and the broad nature of the reform, we are further able to explore heterogeneity in effects of DNA profiling. We consider differences by previous

criminal history, age, and parent status. We find statistically significant deterrence effects for all groups except older offenders. Deterrence effects are largest for offenders initially charged with violent crime, while DNA databases prevent subsequent property, weapon, and violent offenses. This supports the hypothesis that offenders frequently commit multiple types of crime, rather than specializing in only a specific crime type. In addition, we find that, by reducing recidivism, DNA databases have beneficial effects on family life. First-time offenders are statistically significantly more likely to be married after they are added to the database, and recidivists are more likely to be with the same partner and to live with their children, at least during the first year after the initial charge. These findings are consistent with the hypothesis that keeping people out of trouble (and out of prison) can put their lives on a more positive track.

A variety of robustness and placebo tests support our main results. Among others, we run our analysis using other years as placebo reform dates and find near-zero and insignificant estimates, and while our main estimates are based on a sample of offenders charged within 24 months of the reform, our estimates are nearly identical for sample cutoffs ranging from 12 to 30 months.

We foremost contribute to the the literature on detection of and deterrence from crime by showing that DNA profiling of offenders increases detection probability thereby deterring offenders from future crime.[1] To our knowledge, we are the first to estimate an elasticity of criminal behavior with respect to *the probability of detection*, whereas most previous work on this topic focuses on the elasticities of crime with respect to specific inputs such as *police hiring* (these estimates range between -0.1 and -2, see e.g., Chalfin and McCrary, 2017a; Evans and Owens, 2007; Levitt, 1997).[2] The elasticity with respect to *the probability of detection* is a central parameter in the economics of crime and first formalized by Becker (1968).[3] Our estimates are qualitatively consistent with the previous literature's findings,

[1]See Chalfin and McCrary (2017b) for a review of this literature.

[2]Chalfin and McCrary (2017a) provide the most precise estimated elasticities of -0.67 for murder, -0.56 for robbery, and -0.23 for burglary.

[3]Becker (1968) on pp. 11: "an increase in p_j [detection probability], would reduce the expected utility,

but we show that the underlying elasticities of overall detection probability are larger than what is previously reported for the elasticity with respect to number of police officers. This makes sense, as a 1% increase in police force likely results in less than a 1% increase in overall detection probability. In addition, our results are consistent with the findings in Levitt (1997) and Evans and Owens (2007) that that the elasticity of violent crime is higher than the elasticity of property crime.

Furthermore, this is only the second paper to estimate the causal effects of DNA databases on criminal behavior. Doleac (2017) uses U.S. data to estimate the net deterrence effect (i.e., a combination of the deterrence and detection effects) in a regression discontinuity design based on state database expansions, and finds a reduction in subsequent convictions of 17% for violent felony convicts, and 6% for property felony convicts. We analyze the effect of DNA databases for a much broader group at the current policy frontier in the U.S. – those charged with felony crimes – rather than convicts only. We find substantially larger deterrence effects for this set of less-serious offenders, suggesting that the marginal benefits of adding people charged with felonies (rather than waiting for conviction) are large.

Finally, the large public safety benefits found here are also related to the existing evidence of other high-tech surveillance tools' effectiveness, as for instance, electronic monitoring which has been found to reduce recidivism (Di Tella and Schargrodsky, 2013; Marie, 2015; Henneguelle et al., 2016). While electronic monitoring has been used as an alternative to pre-trial detention or incarceration and operates through different mechanisms, the results are consistent with our findings that surveillance can provide a substantial, low-cost deterrent for individuals who might otherwise be prone to commit crime.

The paper proceeds as follows: Section 2 presents the institutional background and the reform of the DNA database, which we use for identification. Section 3 describes our theoretical framework, and Section 4 details the empirical strategy we use to estimate the deterrence and detection effects of DNA profiling. Section 5 describes the data. Section 6 presents the

and thus the number of offenses, more than an equal percentage increase in f_j [sanctions], if j has preference for risk."

results. Section 7 concludes.

2 Institutional background and the reform of the DNA database

The Danish Central DNA Database was introduced on July 1, 2000, in order to i) ease police detection work by identifying offenders and ii) deter offenders by increasing a profiled offender's probability of getting caught for any subsequent crimes (Justitsministeriet, 1999). The database consists of a person-specific section with DNA samples from suspects, and an evidence-specific section with DNA samples collected at crime scenes or from a victim (Lov om oprettelse af et centralt dna-profilregister, 2000). At the time the database was created, however, only suspects of a limited number of the most serious offenses (e.g., murder, robbery, arson, major violence, incest, and rape) could be included in the person-specific section, and only when the DNA profile was essential to a specific criminal investigation. Likewise, police only collected crime scene evidence from other types of cases if they were suspected to be linked to cases of serious crime and could aid in the apprehension of such offenders.

2.1 The 2005 reform

The Danish legislature expanded the DNA database on May 24, 2005.[4] The bill introduced two major changes surrounding DNA profiling in criminal cases. First, the list of crime types that qualify for DNA profiling was vastly expanded to include all offenses where the maximum penalty is a prison sentence of 18 months or more[5]. This is roughly equivalent to adding anyone charged with a felony in the United States. Examples of newly-qualifying offenses include burglary and simple violence/assault. Second, prior to the reform, DNA profiles

[4]The law was proposed on February 22nd 2005, passed on May 24th 2005 and enacted on May 25th 2005 (Lov om ændring af lov om oprettelse af et centralt dna-profilregister og retsplejelove, 2005).

[5]The law also added possession of child pornography as a qualifying offense, even though the maximum penalty for that crime is a prison sentence of 1 year (Justitsministeriet, 2005).

were only collected if they were deemed to be essential to a specific criminal investigation, so charged individuals who confessed were not obliged to be profiled, nor were individuals charged in cases with no DNA evidence (Det Etiske Råd, 2006). The reform eliminated these requirements. Furthermore, the reform also made it easier and cheaper to obtain DNA samples as it authorized the police to collect the DNA sample instead of requiring medical personnel. On these dimensions, the legislation and implementation of Denmark's DNA register are now very similar to those of DNA databases in the U.S. and many other countries around the world.

The changes in 2005 were quite substantial in terms of increasing the likelihood that a charge would result in DNA profiling and registration. Figure 1a shows the likelihood that a charged individual was added to the DNA database (see section 5.1 for more on the sample description). In our sample the likelihood of being registered in the DNA register increased substantially from May 2005 to October 2005, from 4% to almost 40%. In the subsequent years, DNA registration becomes gradually more prevalent and by 2007 almost 60% of charged offenders had their DNA registered. Yet, Figure 1a also suggests that there was a lag in law enforcement's implementation of the new rules for DNA profiling in 2005, which we will discuss in more detail in Section 4.

Figure 1b shows the evolution of the aggregate number of cases with crime scene evidence in the DNA database (dashed line) and the annual number of hits between offenders and crime scene evidence (solid line) from 2003–2007. The x-axis in Figure 1a differs from the x-axis in Figure 1b: the former denotes the timing of DNA profiling of offenders, while the latter denotes both the year of registration of crime scene evidence and the year of the hit. Yet, Figure 1b shows a clear increase in both collected crime scene evidence and in the number of hits between offenders and evidence. This illustrates that police behavior in the collection of evidence changed along with the increased requirement to profile offenders and, as a consequence, detection via the database increased.[6]

[6] Apart from the increased likelihood of DNA profiling and the increased collection of DNA evidence from crime scenes, the reform did not coincide with other changes to the judicial system or policing.

3 Theoretical framework

Standard economic models suggest that the propensity to commit crime is a negative function of the expected punishment for that crime. As initially formulated by Becker (1968), an individual will commit crime when the expected benefits exceed the expected costs:[7]

$$y_i = 1[\alpha_i - c_i > 0] \tag{1}$$

where α_i summarizes the expected benefits from the crime, and may be a function of monetary and non-monetary payoffs, and c_i is expected costs, which is an increasing function, $f(p, s)$, of the probability of being convicted, p, and the sanction if convicted, s.

This paper focuses on how a change to p – by being included in the DNA database – affects crime. As suggested from Figure 1, having one's DNA recorded in the database DNA_i=1 increases the probability of being apprehended from $p = \bar{p}$ to $p_i = \bar{p} + \gamma DNA_i$. From Equation (1) we get that crime in the two states, y_i^0 and y_i^1, respectively, equal:

$$y_i^0 = 1[\alpha_i - f(\bar{p}, s) > 0]$$
$$y_i^1 = 1[\alpha_i - f(\bar{p} + \gamma DNA_i, s) > 0]$$

We label the difference between the potential outcomes 'the deterrence effect of DNA profiling':

$$\Delta = y_i^1 - y_i^0 \tag{2}$$

However, we face two problems. First, as in most applied studies we do not observe y_i^0 and y_i^1 for the same individual and thus, we have to address with the endogenous relationship between unobservable characteristics embedded in α_i and the variable of interest DNA_i. We will introduce this when we describe our empirical strategy in Section 4. Yet we also face

[7] We disregard discounting of benefits and costs here, for simplicity.

a second problem common to studies with crime as an outcome: we cannot link crime to offenders unless they are caught. Hence, we do not observe actual crime y_i but only 'observed crime' $\tilde{y}_i$ with probability, $\bar{p}$ and $\bar{p}+\gamma DNA_i$ without and with a DNA database, respectively:

$$\tilde{y}_i^0 = \bar{p} * y_i^0$$

$$\tilde{y}_i^1 = (\bar{p} + \gamma DNA_i) * y_i^1$$

Hence, even if we observed an individual in both treatment states we would get:

$$\tilde{y}_i^1 - \tilde{y}_i^0 = \bar{p} * \Delta + \gamma DNA_i * y_i^1,$$

instead of the desired quantity, Δ. In consequence, the deterrence effect from DNA profiling is not identified simply by the differences in observed crime rates with and without DNA profiling, because not all crime is observed. There are two channels of bias in addition to the endogenous relationship between offenders' unobservable characteristics and DNA profiling. First, the observed change in crime as a result of the reform is attenuated because only a fraction of crime $\bar{p}$ is observed. The effect is scaled down by the clearance rate of crime, $\bar{p}$. Second, there may be an upward bias because DNA profiling increases the fraction of crime that is observed. Offenders are caught more often when DNA databases are in effect – that is the purpose of the technology. We define this latter source of bias as 'the detection effect':

$$\delta = \gamma DNA_i * y_i^1 \tag{3}$$

In the next subsection we will describe how we overcome both problems. Finally, from the deterrence and detection effects we are able to define a central policy parameter: the elasticity of crime with respect to the likelihood of detection: ϵ. We define this as the percentage change in crime (the deterrence effect, Δ, relative to baseline crime, y_i^0), divided by the percentage change in the probability of detection (the detection effect, δ, divided by

baseline crime, y_i^0, relative to the baseline clearance rate, $\bar{p}$):

$$\epsilon = (\Delta/y_i^0)/(\delta/y_i^0 * 1/\bar{p}) = \bar{p} * \Delta/\delta. \tag{4}$$

4 Empirical strategy

To identify the causal effect of DNA profiling on an individual's (observed) criminal behavior, we need exogenous variation in who is added to the DNA database. We exploit the 2005 expansion of Denmark's DNA database for that purpose. That expansion introduced a large shock to the probability that someone charged with a crime is added to the DNA database. Offenders charged within a period around the reform are effectively randomized into control and treatment groups based on the precise timing of their charges. However, as mentioned above, there was a delay in full policy implementation until October 2005. Police officers' summer vacations were the main reason for this: police departments were short-staffed during the summer, and so the extra work required to stock the necessary DNA collection kits was often delayed.[8] Therefore, while the reform motivates an RD strategy, we will treat the change as an 'normal' instrumental variable excluding months June through September (the summer months immediately after the reform) while conditioning on running variables counting months leading up to May and after October 2005. (This strategy if often referred to as a 'donut RD'.)

We will estimate effects using two-stage least squares (2SLS) treating timing of charge as a binary instrument Z. The first stage is:

$$DNA_i = \gamma Z_i + \mu_1 g(x_i) + X_i \beta_1 \tag{5}$$

where Z is dummy variable indicating whether the offender was charged before or after the

[8] Based on conversations with Danish law enforcement.

reform, x is a flexible running variable counting the months before May and after October 2005, and X a set of observable covariates.[9] The second stage is:

$$\tilde{y}_i = \beta^{IV} \widehat{DNA}_i + \mu_2 g(x_i) + X_i \beta_2 \tag{6}$$

Observations are at the charge level, and we will cluster standard errors by individual offender. We argue that the reform satisfies the standard IV / LATE conditions (Imbens and Angrist, 1994) – that the instrument strongly predicts inclusion in the DNA register[10], the exclusion restriction holds, and that the reform did not reduce the likelihood of apprehension for any offenders. Given this, we will estimate:

$$\begin{aligned} \beta^{IV} &= E(\tilde{y}_i^1 - \tilde{y}_i^0) \\ &= E(\bar{p} * \Delta + \gamma DNA_i * y_i^1) \end{aligned} \tag{7}$$

taking conditioning on covariates in X as implicit. Hence, we will estimate the average change to observed crime for those who are included in the DNA database as a result of the reform. However, as discussed above, this approach captures a composite of two effects; a deterrence effect due to the behavioral response to an increased probability of detection once an offender is added to the database, and a detection effect from an increased probability of apprehending the offender due to a DNA match. Separating deterrence from detection will provide key information about how DNA databases affect criminal behavior.

[9] X includes: age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, offense type, and month fixed effects.

[10] As discussed in Durlauf and Nagin (2011) and Nagin (2013), a prerequisite to deterrence effects is that potential offenders are aware of the variation in detection probability. This is likely satisfied in the present context, as profiled individuals observe and participate in the DNA sampling, though precise probabilities of detection are likely learned through personal experience or word of mouth.

4.1 Identification of deterrence and detection effects

We will identify the two effects by exploiting the unique information available in the Danish register data (described in the next section). Here, we observe not only when an offender is charged for a crime, but also the exact date of the crime allowing us to divide observed crime $\tilde{y}_i$ into two categories: crime that is solved quickly, $\tilde{y}_i^F$, and crime that is solved slowly, $\tilde{y}_i^S$.

The former, $\tilde{y}_i^F$, denotes crime solved within three weeks from the date of the crime, before any DNA evidence from the crime scene could have been processed. We define the latter, $\tilde{y}_i^S$, as crime solved after three weeks from the date of the crime, at which point DNA evidence could have been processed and may have been used in the investigation to identify the offender. Hence, changes to crime solved within three weeks from the date of the crime will only capture the deterrence effect, while effects to crime solved more slowly will be a composite of both the deterrence and detection effects. In our main set of results, which we will present in Section 6.2, we will present estimates of DNA profiling separately for all observed crime $\tilde{y}_i$, cases solved fast $\tilde{y}_i^F$, and cases solved slowly $\tilde{y}_i^S$, thereby making the different impacts of the deterrence and detection effects explicit. All estimation using $\tilde{y}_i$, $\tilde{y}_i^F$, and $\tilde{y}_i^S$ will be attenuated as only a fraction of crime is linked to offenders, whereas estimates from $\tilde{y}_i^F$ will not be upward biased and thus provide a lower bound of the deterrence effect.

However, by imposing structure on the different elements we will be able to identify and estimate both effects on criminal behavior. We first assume that the baseline clearance rate of crime without the DNA register $\bar{p}$ occurs at an unchanged rate and that it is uniform and invariant with offender characteristics that are not captured by the different crime types.[11] Thereby, we may define the fraction of solved crime that occurs within three weeks from the

[11] Underlying this are two assumptions of 'invariance'. First, we assume that procedures in the criminal justice system did not change along with our IV except through the increased probability of detection $\bar{p}+\pi$. In support of this assumption, we find that there were not any changes in characteristics of charged offenders nor to the share of charges that lead to a conviction which coincide with our IV. We discuss this and provide balancing tests in Section 6.1. Second, in order to compute $\pi\bar{p}$ and $(1-\pi)\bar{p}$, we assume that $\bar{p}$ is invariant across crimes that are potentially solved 'fast' and 'slow'. We acknowledge that this latter assumption of invariance in the clearance rate is strong. Yet, we are only able to identify a uniform $\bar{p}$ from the data and not to separate 'fast' and 'slow' charges because unsolved cases cannot be assigned to either of the two categories (an unsolved case is neither solved 'fast' or 'slow').

date of crime as $\pi\bar{p}$ both before and after the expansion of the DNA register. Therefore:

$$\tilde{y}_i^F = \pi\bar{p}y_i,$$

$$\tilde{y}_i^S = ((1-\pi)\bar{p} + \gamma DNA_i)y_i$$

By estimating the effects of DNA profiling on crime solved within three weeks using the reform as an instrumental variable (IV), we will get:

$$\begin{aligned} \beta_F^{IV} &= \pi\bar{p} * E(\Delta) \Rightarrow \\ E(\Delta) &= (\beta_F^{IV})/(\pi\bar{p}), \end{aligned} \tag{8}$$

which is the deterrence effect. As we have information on all reported crimes, and know the fraction of crimes that leads to a charge of the offender (the clearance rate) within three weeks from the crime date, we observe $\pi\bar{p}$ directly. Hence, we may estimate the deterrence effect. If we instead focus on the estimated effect of the reform on crime solved after three weeks from the date of crime, we get:

$$\beta_S^{IV} = E[\gamma DNA_i * y_i^1 + (1-\pi)\bar{p} * \Delta]$$

By subtracting the former estimate β_F^{IV} multiplied by $(1-\pi)/\pi$ from β_S^{IV} we arrive at:

$$\begin{aligned} \beta_S^{IV} - \beta_F^{IV} * (1-\pi)/\pi &= E[\gamma DNA_i * y_i^1 + (1-\pi)\bar{p} * \Delta] - \pi\bar{p} * E(\Delta) * ((1-\pi))/\pi \\ &= E[\gamma DNA_i * y_i^1] \\ &= E(\delta) \end{aligned} \tag{9}$$

which is the detection effect. From Equations (8) and (9), we are also able to estimate the elasticity of crime with respect to detection probability as:

$$
\begin{aligned}
E[\epsilon] &= \bar{p} * [(\beta_F^{IV})/(\pi\bar{p})]/[\beta_S^{IV} - \beta_F^{IV} * (1-\pi)/\pi] \\
&= \beta_F^{IV}/[\pi * (\beta_S^{IV} + \beta_F^{IV}) - \beta_F^{IV}] \qquad (10) \\
&= \beta_F^{IV}/(\pi\beta^{IV} - \beta_F^{IV})
\end{aligned}
$$

In Section 6.2 we will present the overall estimated effects from Equation (7). In Section 6.4 we will show separate estimates of the deterrence and detection effects from Equations (8) and (9) and the elasticity of crime with respect to detection from Equation (10).

5 Data

While Denmark differs from the U.S. in many respects, average crime rates are overall similar across the two countries[12] We focus solely on adult offenders, for which the judicial system bears close resemblance to those in other OECD countries.[13]

We use Danish full population register data with information on all residents from 1980 and onwards. Unique individual identifiers allow us to merge information on involvement with the criminal justice system and demographic characteristics among others, and the identifiers also allow us to link each individual to family members and partners.

5.1 Sample definition

Two main registers are used to construct the sample used in our analysis. The first is the charge register, which contains information on the crime date, charge date, crime type and of course the personal identification number of the charged individual. The second register is a record of all the individuals in the person-specific section of the Central DNA register (what we refer to elsewhere as the DNA database), and contains – besides the personal

[12]See pp. 207 in OECD, 2005 and `http://www.oecdbetterlifeindex.org/topics/safety/`. Substantial differences exist for specific crime-types as for example gun-violence or homicide.

[13]The age of criminal responsibility in Denmark is 15, after which young people are considered fully responsible for their criminal acts and subject to imprisonment.

identification number – a record number that refers to the particular charge in relation to which the DNA sample was collected. These two registers are merged in order to determine which charges led to DNA profiling.

In our main sample we include charges[14] that occurred between June 2003 and September 2007. Due to the lag in police practice in terms of implementing the new rules concerning DNA profiling, we exclude the months of June-September of 2005, and use a 24 months sampling window on either side of that period. We choose the bandwidth of 24 months on the basis of a cross-validation (CV) procedure (as described in Lee and Lemieux, 2010, and Ludwig and Miller, 2005) in order to minimize prediction error close to the reform.[15]

Besides the time frame, the charges included in the sample have to fit the following four criteria: i) the charge has to be for an offense against the Penal Code or Weapons Act; the latter mainly consists of illegal possession of explosives, firearms and other weapons (see table A-1 for Danish crime categorizations). These include the vast majority of criminal offenses, and so we only discard individuals charged with traffic offenses, small-scale drug possession and offenses such as Health Code and Tax Law violations. ii) The charged individual has to be a resident of Denmark and appear in the residential register in order for us to be able to merge information on basic demographics such as age and gender onto the sample of charges.[16] iii) We only include charges against men aged 18-30 at the time of the charge. This group is the most criminally active and is the most relevant for estimating effects on criminal behavior. iv) In order to avoid that the individuals who are charged with several crimes within the time frame are given disproportionately high weights in the analyses, we

[14]Only one charge per person per charge date is included to avoid having crimes that violate several different laws disproportionately represented in the data.

[15]The cross-validation procedure consists of two steps. First, we estimate the reduced form regressions with a dummy variable indicating before/after June-September of 2005 and running variables measuring months before or after (+ covariates), but leave out observations in the 1-3 month preceding June and following September. Second, we use the estimates to predict the outcome for the observations in the excluded window around the reform, and calculate the mean prediction error (or CV functions) for each outcome which we finally aggregate across the outcomes. We have done this for bandwidths from 5—50 months before/after the reform. Figure A-1 shows that a bandwidth of 24 months yields the best prediction.

[16]This implies that we exclude tourists and individuals from other EU countries moving freely within the EU without being registered with a Danish social security number.

only include charges against men who at the time of charge have a maximum of 10 prior charges behind them.[17]

Our sampling unit is 'charges'. To illustrate how we handle multiple charges against the same individual, suppose individual i is charged initially at time t_0. This will enter as one observation with any subsequent recidivism in the following years $t_0 + 1, t_0 + 2, ...$ recorded as outcomes linked to that observation. A subsequent charge to individual i at, for example, time $t_1 = t_0 + 1$, will enter as a new observation (if t_1 falls within the sample window) with recidivism in years $t_1 + 1, t_1 + 2, ...$ as outcomes. While this ensures that we do not select the sample on outcome variables, one might still worry that fluctuations in the sampling coincide with our instrument because we sample some individuals more than once (those who are charged several times within our sample window). We do not consider this a problem for three reasons. First, our results are robust when we focus on first time offenders and thus obviate repeated observations and any potential selection associated with this. Second, while our design is not formally a discontinuity, we estimate effects conditional on the running variable and effectively compare individuals charged within a small window of time. Finally, in Section 6 we implement placebo tests (placebo reforms in other years / use previous charges as outcomes) which all produce near-zero and insignificant estimates. Hence, we are confident that there is nothing mechanical in our sampling that generates spurious effects.

Our sample consists of 38,674 individuals who received a charge that fits the aforementioned criteria with a total of 66,911 observations. As multiple charges against the same person are not independent observations, we cluster standard errors at the individual level.

[17]Different caps on maximum number of charges do not change our results and conclusions substantially (see table A-8).

5.2 Outcome variables

We define the outcome variables as subsequent convictions for crimes committed after the charge in question. Our main outcome is overall crime, but we also split this into different categories based on the type of offense. The main category includes all violations against the Penal Code and the Weapon Act ('any crime'), and we also show results for violence, property crime, sexual offenses, other penal offenses (including drug offenses), and violations of the Weapons Act separately.

As noted previously, our unit of observation is a criminal charge. Hence, individuals may appear several times in the data. We define the outcomes from the time of the charge for which an individual enters the sample. Counting from the day after this charge, we measure subsequent criminal activity as crimes for which the individual is convicted within one, two, and three years. All crime measures are coded in a binary version indicating at least one conviction and in a version that counts the number of convictions within the one-, two- and three-year follow-up periods.[18] The latter measures are top-coded at a maximum of ten convictions per follow-up year, to limit the effect of outlier individuals. (We will show how estimates change when this top-code is adjusted.)

In constructing the outcome variables, we distinguish between convictions for which the charge occurred three weeks or less after the crime date, and convictions for which the charge occurred more than three weeks after the crime date. This is done in an attempt to separate the charges where prior DNA profiling is unlikely and likely, respectively, to have contributed to the identification of the offender. Because the analysis of crime scene evidence takes some time, it is not possible that a match in the DNA database led police to the offender if he was charged very soon after the crime. Any effect of DNA profiling on observed recidivism during that window would come solely from a deterrence effect; afterwards, DNA profiling would have both a deterrence effect and a detection effect. According to the Forensic Institute at

[18] As offenders who are not included in the DNA register for a given crime will be included (and thus be treated) with increasing likelihood when they recidivate in subsequent years, estimates of long run effects may be attenuated.

Copenhagen University, where the DNA analysis is conducted, the police should expect to wait 4 weeks for a DNA sample to be processed (95% of samples are processed within 4 weeks or less), but to strengthen the validity of one of our identifying assumptions we set the limit at three weeks as it is likely that some DNA samples are processed before the 4 week deadline (Retsmedicinsk Institut, 2014).[19]

Although recidivism is our primary concern and subsequent convictions are our primary outcomes, we also examine whether DNA profiling affects non-criminal behavior. Here we focus specifically on marriage and relationship stability – a proxy for having a strong support network. A large criminology literature identifies a stable family environment as one of the chief predictors of crime desistance (see e.g., Sampson et al., 2006). If DNA registration reduces recidivism it is possible that the change in behavior also has a positive impact on engaging in or maintaining other pro-social relations such as romantic relationships. To examine whether this is the case, we use timing of changes in marital status and home addresses to measure whether the individual is married, remains in the same relationship if he had a partner prior to the initial criminal charge, and lives with his child and the child's mother if he had children prior to the initial criminal charge.[20]

5.3 Data Descriptives

Table 1 shows average characteristics of the full sample and divided by whether the charge took place before or after the reform. Overall, individuals charged with crimes have 11 years of education, only slightly above the compulsory level in Denmark (9 years). Their annual incomes are low – about 112,000DKK (equivalent to under $17,500 in the U.S.) – and nearly half are unemployed at the time of the charge. Most (86%) are single but a small share

[19]Results are robust to moving the limit for fast charges to 2 weeks. If some of our 'fast' charges are in fact aided by the DNA database, then we will underestimate the deterrence effect.

[20]We observe the unique individual identifier and home addresses of the full population, which allows us to identify whether a given offender lives with a partner and any children. The measure of the father living with his child and his child's mother is constructed for each of his children (born prior to the initial charge), and for this outcome the father appears in the sample once for each child and charge.

(12%) have children. Immigrants are heavily overrepresented, making up 21% of the sample (relative to less than 10% in the general population). Almost 40% live in one of the four largest cities. Table 1 also shows the sizes of the subgroups that we will use to examine whether DNA profiling has heterogeneous effects across different offender characteristics. For example, 24% enter the sample on their very first charge, whereas the rest have between 1 and 10 charges behind them (the overall mean is 3 previous charges).

Table 2 shows average characteristics of compliers, i.e. offenders whose DNA profiling was induced by the reform, along with full sample means for comparison, and the ratio of compliers' background characteristics relative to the full sample's. Among other things the table shows that a significantly larger share of the compliers belong to the younger age-category compared to the whole sample, fewer have children, and fewer enter the sample on their first charge. The compliers are also less educated and have a lower gross income, but are just as often unemployed. In terms of crime types, we see that the compliers are more often violent and sexual offenders compared to the overall sample. Those differences noted, most categories of offenders are well-represented within the complier group and our instrumental variable provides large and significant increases to the probability of DNA profiling in all subsamples. This will allow us to consider heterogeneity of effects by offender characteristics while also supporting monotonicity of the instrumental variable.

Panel A in Table A-2 shows summary statistics for the conviction outcomes within 1, 2 and 3 years of the initial charge. The tables present statistics divided by timing of the charge relative to the reform. In the full sample 13.4% are convicted for another offense within one year. Two years after the initial charge, 27% have been convicted for another crime and the average number of convictions is roughly 0.4. After three years these numbers are 36% and 0.6 convictions. The most prevalent crime type is property crime, which constitutes approximately 55% of all recidivism. Almost 30% of recidivism is violent crime, while sexual offenses constitute less than 1%, and weapon-related and the residual 'other crime' (mainly drug-related offenses) each constitute around 7% and 8% of recidivism respectively.

Panel B in Table A-2 summarizes marital status outcomes. Only 4.6% of the full sample are married by the time of charge, a share that increases to 5% one year after the initial charge and to 7% after three years. When looking at those who have a partner (married or cohabitating) prior to the initial charge, 46% of them are with the same partner one year after the charge. For the offenders who have at least one child at the time of the initial charge, the probability that the father lives with the child and the child's mother is 30%.

6 Results

6.1 Validity of the reform as an instrument

Below we provide balancing tests showing that the reform provides a clean identification of the effects of DNA profiling. As described above, we exclude June-September 2005 from our main analysis. Offenders charged between June 2003 and May 2005 make up our control group, those charged between October 2005 and September 2007 compose our treatment group, and our identifying assumption is that offenders' propensity to recidivate, conditional on their non-treatment characteristics, does not change between May 2005 and October 2005 (the effective date of the 2005 DNA database expansion).

Table 3 shows results of regressions that test for discontinuities in the covariates by regressing each covariate on a dummy indicating whether the charge occurred after the reform or not conditional on running variables counting the number of months before and after the reform (and month fixed effects in column 2).

According to the table, there are significant differences around the reform for a few covariates. However, what matters for our analysis is whether those differences in individual characteristics are meaningful enough to affect offenders' propensities to reoffend. Figure 2 shows offenders' predicted propensities to reoffend based on the pre-treatment relationship between observable characteristics and recidivism, for individuals charged before and after

the reform.[21] Both the probability of committing any crime and the number of predicted crimes are smooth through the threshold.[22]

This claim is further strengthened by the results in Table 4, which present the regression equivalent of Figure 2 testing for discontinuities in outcomes predicted by the covariates. The distribution of predicted recidivism (based on observable characteristics) is indeed smooth through the threshold as we see no significant differences in this measure just around the reform, which makes it highly unlikely that the small differences in covariates seen in Table 3 will bias our results. Furthermore, our first and second stage results (which we will present below) show that our estimates are virtually unaffected by the inclusion of covariates, thereby strengthening this claim.

To test for a discontinuity in the sample size, which would suggest that the timing of charges was shifted from one side of the threshold to the other, we conduct a McCrary test (McCrary, 2008) on the number of charges in our sample (excluding the summer months of 2005), which is shown in Figure A-2. There are no significant discontinuities in the distribution of charges at the threshold.

Furthermore, one might be concerned that the reform changed police behavior with respect to evidence collection or charges of suspects. For instance, perhaps the database expansion made police more aware of the value of DNA evidence and more careful to only charge defendants when such evidence was present. However, such a change in police behavior would affect all active offenders regardless of whether they are in the database. (In particular, police can always get a warrant for a DNA sample from a suspect to compare with crime scene evidence; the difference for those in the database is that they might be matched to cases in which they would not otherwise have been a suspect.) That said, we find no discontinuity in the likelihood of a charge leading to conviction across the reform, which might suggest

[21] We use pre-reform data to regress recidivism on observable characteristics. We then use those estimated coefficients to predict recidivism based on observable characteristics for the full sample, and test for a discontinuity in this predicted measure.

[22] See e.g., Card et al. (2007) for a similar argumentation in relation to balancing of covariates and predicted outcomes in a discontinuity design.

that charges are becoming more accurate (results are available upon request).

Finally, another concern is if the reform made offenders increasingly aware that if they had their DNA profiled they would be linked to past crimes via already old crime scene evidence. This would imply that the reform not only changed probability of being caught p, but also the sanctions s. This could introduce non-random variation in offenders across the timing of the reform. None of the tests provided above suggest that this is the case as the treatment and control groups are balanced through the threshold defining our instrumental variable. Nonetheless, even in the absence of selection in profiling, changes to the likelihood that one is convicted of previous crimes following the reform and DNA profiling would change our framework and the interpretation of our results substantially. If this is the case, we should see that the reform increased charges and/or convictions for crimes that were committed before but solved after DNA profiling. In Table 5 we test this by showing the estimated changes in charges and convictions for crimes that were committed *before* the specific charge that lead to DNA profiling, but where charges were not pressed until *after* the DNA profiling. All estimates are close to zero and insignificant showing that the increased DNA profiling induced by the reform did not increase the likelihood that offenders were convicted for crimes committed before being profiled.

6.1.1 First stage results

Figure 1a illustrates the first stage effect of the DNA database expansion on the probability that a charge results in DNA profiling. The summer months of 2005 are shown in grey. After excluding those months, there is a clear shock to the probability of DNA profiling after the reform changing the probability of DNA profiling from 4% to almost 40%. Table 6 formally presents the first stage estimates. The reform increased the likelihood of DNA profiling by 35 percentage points, which is a highly statistically significant increase ($p < 0.001$).

6.2 Main results

Figures 3 and 4 show monthly averages relative to the sample mean of the probability of being convicted for a crime and the number of convictions for crimes committed within the first year following the initial charge. As in Figure 1a, data from the excluded summer months of 2005 are shown in grey for transparency; those data are excluded from our regressions to avoid selection bias. The figures provide a first visualization of our main findings, as they illustrate the reduced form relationship between the reform, and all convictions, convictions where charges were filed within three weeks of the crime date ('fast' charges), and convictions where charges were filed at a later stage ('slow' charges). Following the reform, 'fast' charges drop substantially after the reform while 'slow' charges remain largely unaffected.

Table 7 presents the estimated effects of DNA registration on subsequent convictions 1, 2 and 3 years after the initial charge, with standard errors in parentheses.[23] Columns 1–3 show effects on the probability of any subsequent conviction, and columns 4–6 show effects on the number of subsequent convictions. Columns 1 and 4 reveal that DNA registration reduces the probability of any new conviction by 6.4 percentage points in the first year (42%, $p < 0.001$), and the number of convictions by 0.093 (49%, $p < 0.01$). Both effects grow slightly in magnitude by year 2, and then fall in the third year, but all estimated effects are economically meaningful and at least borderline significant.

Columns 2 and 5 focus on new convictions from 'fast' charges. Since the DNA database could not have identified the suspect in these cases, these provide a clean estimate of the deterrence effect. In year 1, we see that DNA registration reduces the likelihood of recidivism by 5.7 percentage points (43%, $p<0.01$) and the number of new offenses by 0.075 (47%, $p<0.01$). Again, both effects grow in magnitude during year 2 and then fall slightly in year 3, but now all effects remain statistically significant.

Columns 3 and 6 focus on convictions following 'slow' charges. As DNA databases may

[23] Table A-6 shows our main results using different conditioning sets. As the inclusion of covariates does not affect point estimates but increase precision, all remaing tables present results conditional on covariates.

have aided in the detection in these cases, the estimates represent a combination of both detection and deterrence effects. A positive detection effect would bias any estimated effect on observed recidivism upward. As expected, the coefficients are closer to zero and insignificant.

6.2.1 Heterogeneity by offender characteristics

A frequent topic of policy debate is which categories of offenders should be included in a DNA database. Should only serious offenders or violent offenders be added, or should a broader set of individuals be included in the hope of catching or deterring would-be serious offenders earlier in their careers?

In order to investigate this, Table 8 presents estimates of the effect of DNA profiling on subsequent crime convictions by the offense category of the initial charge. Focusing on columns 2 and 5 (fast charges), effects on recidivism are most striking for violent offenders. For this group, DNA registration reduces the probability of a subsequent conviction by 5.8 percentage points (48%, $p < 0.01$). That effect persists through year 3. The estimates further suggest that offenders initially charged with property offenses, other penal offenses, and weapons-related crime also reduce recidivism following DNA registration. For property offenders the effects of DNA profiling on convictions based on a fast charge are large, though typically only marginally significant. Effects on offenders charged with weapons-related crime and other penal offenses are also large, albeit imprecisely estimated. The estimates for offenders initially charged with sexual offenses are positive for slow charges, suggesting a detection effect, but none of those estimates are statistically significant, likely due to the small sample size.

Table A-3 presents estimates of DNA registration on subsequent convictions separately for first-time offenders (those who enter our data for their first ever charge) and recidivists (those who have one or more previous charges). Columns 1–3 show results for the probability of any subsequent criminal conviction. Overall, estimates for first-time offenders and recidivists are quite similar in magnitude. However, as pre-reform baseline recidivism rates differ between

the two groups (6% of first-time offenders have reoffended within one year compared to 18% for the rest of the sample) so does the relative decrease in recidivism. First-time offenders' 4.8 percentage point drop in the likelihood of a subsequent conviction constitutes a 79% reduction, while recidivists' 6.7 percentage point reduction constitutes a 36% decline relative to their respective pre-reform baselines. Columns 4–6 of Table A-3 show effects on the number of subsequent convictions. Here the same pattern emerges, though we only see significant effects for recidivists. For this group, DNA registration reduced the number of convictions by 0.105 per person in the first year (46%, $p < 0.01$).

Table A-4 divides offenders by age at the time of DNA registration. Given differences in criminal propensities by age, a policy like DNA registration may have different effects for younger, more crime-prone, individuals than for older individuals who may be on the margin of retiring from crime. The significant crime reductions are driven by the 18-23 year olds, who constitute two thirds of the sample. This is particularly true for the first year following the initial charge (where age-group estimates are significantly different), but from thereon, we lack the precision to separate the effects. As was the case with first offenders vs. recidivists, the two age groups differ in baseline recidivism rate, which for the older offenders is about two-thirds of the younger offenders. This means that although estimates are slightly smaller for older offenders in year two, these estimates still suggest substantial deterrence of older offenders.

Table A-5 shows effects of DNA registration separately for those who have at least one child by the time of the initial charge (12% of the sample) and those who do not. It is possible that offenders are more easily deterred if they are motivated to be an role model for their kids, or less deterrable if they feel financial pressure to commit crime to support their family. Here we see significant reductions in the number of convictions and the likelihood of any conviction for both groups. For offenders with children, all effects are consistently negative and most statistically significant when convictions are based on fast charges, but consistently positive and for the convictions based on the slow charges suggesting both deterrence and

detection effects of DNA profiling. The deterrence effects for fathers are especially strong, when compared to their relatively low baseline recidivism rates, which are about 20% lower than for those without children, at the time of charge.

6.2.2 Robustness checks

We perform a series of robustness test on these results. The estimated effects are robust to different sample definitions (Table A-7) and restrictions on the sample in terms of the cap on the number of prior charges (Table A-8).[24] Also, a series of placebo tests (shown in Table A-9), which artificially impose reforms in years other than 2005, shows that significant reduced form estimates occur only in the year of the actual reform. Another type of placebo test is presented in the bottom of Table 7, where DNA registration is used to predict the number of charges and the likelihood of being charged prior to the sampling charge. If the reform was an exogenous shock, we should not see significant estimates here. In this placebo test we do not find any significant pre-period 'effects' of DNA registration.

In Table A-10 we consider charges instead of convictions as the outcome measure of recidivism. If these results are qualitatively different it could indicate that reform has also lead to changes in policing practices. However, this appears not to have been the case as results in Table A-10 are similar to those in Table 7 (although estimates are smaller and more imprecisely estimated) and point to reduced recidivism due to DNA profiling.

Table A-11 presents results where the number of subsequent convictions have been adjusted for the time spent incarcerated in the follow-up period. More specifically, the number of convictions has been divided by the proportion of the follow-up period where the individual was not incarcerated. This adjusts for the attenuation bias that may occur if higher-risk offenders are incarcerated for longer, thereby lowering their crime rates through incapacitation. The results in Table A-11 are similar to the main results, but estimates are generally

[24] The parameter estimates in Table A-7 are slightly lower and standard errors slightly larger the more we restrict the sample to those charged closer to the policy change, but the results are overall quite similar for all specifications - especially when it comes to the first years after DNA profiling.

numerically larger for the adjusted outcomes.

Finally, we present estimates in Table A-12 based on a sample where we keep the summer months of 2005 in the dataset. Our preferred sample excludes these months, but a potential concern is that an unrepresentative sample of offenders was added to the database during this period which could introduce selection bias. When we include the summer months, we estimate that the reform led to a 21 percentage point increase in the likelihood of DNA registration. This is smaller than before but still highly statistically significant ($p < 0.001$). The table also replicate our main results and results for each offender subgroup. Across the board, the coefficients are less precisely estimated, but they are very similar in magnitude and qualitatively equal to those described above. For instance, the estimated effect of DNA registration on the likelihood of a new conviction stemming from a 'fast' charge – a clean estimate of the deterrence effect – is a reduction of 6.9 percentage points over 3 years ($p < 0.10$) when the summer months of 2005 are included, compared with a reduction of 6.7 percentage points ($p < 0.01$) when those months are excluded.

6.2.3 Heterogeneity in types of crime prevented

To examine which types of crime are prevented by DNA registration, Table A-13 presents estimates separately by type of offense. The table shows that decreases in both violence and property crime along with reductions in violations of the Weapons Act drive the effects of DNA profiling seen above.

For property crime, the effects of DNA profiling are strongest – and most significant – for the probability of a criminal conviction and in the first two years following the initial charge. After two years DNA profiling has reduced the likelihood of a property crime conviction (based on a fast charge) by 4.7 percentage points (32%). We see a similar picture for violent offenses, where the likelihood of conviction (with a fast charge) is reduced 3 percentage points after two years (34%). Also, the likelihood of a subsequent conviction for weapon offenses (based on a fast charge) decreases by 1.9 percentage points, a 73% decline compared to the

pre-reform recidivism rates in Table A-2. Finally, sexual offenses and other penal offenses – which constitute a minority of convictions – appear to be largely unaffected by DNA profiling with estimates near zero and statistically insignificant.

6.3 Non-crime effects of DNA profiling

The consequences of criminal behavior has been linked to a variety of other outcomes that may in turn induce crime again[25], through its effects on one's network, time available for investment in other activities, and because the stigma of a criminal record might limit future opportunities. We have so far shown that DNA profiling deters offenders from committing future crime. This could in turn improve other outcomes, which we will focus on next by estimating DNA profiling's effects on family relationships.

Table 9 shows the estimated effects of DNA profiling on the likelihood of being married, the likelihood of remaining in the same relationship as before the initial charge (given that the offender was in a relationship), and that the offender lives with his children and their mother (if the offender has children). In the table, we present estimates from one to three years after the initial charge, as well as estimates using marital status one year prior to the initial charge (year -1) as a placebo test.

Columns 1–3 show effects for all offenders. Columns 4–6 show effects for first-time offenders only (less-hardened offenders, for whom lower recidivism may have a more substantial effect on other aspects of their lives), and columns 7–9 show effects for recidivists only.

We see no statistically significant effects for the full group of offenders, though the imprecisely-estimated coefficients imply economically meaningful effects. One year after their initial charge, DNA-profiled offenders are 0.7 percentage points (12%) more likely to be married, 11.0 percentage points (24%) more likely to live with the same partner, and 12.4 percentage points (40%) more likely to live with their child and the child's mother.

[25] For example incarceration (e.g., Aizer and Doyle, 2015), labor market outcomes (e.g., Grogger, 1998; Raphael and Winter-Ebmer, 2001; Mueller-Smith, 2015), and family formation (e.g., Laub et al., 2008).

For first-time offenders, the effect on the likelihood of marriage is a statistically significant 3 percentage point (46%, $p < 0.05$) increase after the first year. This estimate grows in magnitude and remains statistically significant through the third year. Estimates of the effect of living with the same partner are initially near-zero, and remain statistically insignificant, though the relevant sample is small. DNA registration appears to decrease the likelihood of living with the child and their mother for first-time offenders, though these effects are statistically insignificant and again based on a relatively small sample. For recidivists, we see no impact of DNA profiling on the likelihood of being married (all coefficients are near-zero). However, there is suggestive evidence that DNA registration increases the likelihood of living with the same partner as before DNA profiling: DNA-profiled offenders are 13.1 percentage points (30%, $p < 0.10$) more likely to live with the same partner one year later, though that estimate falls to 4.4 percentage points by year three. DNA registration increases the likelihood that an offender lives with his child and the child's mother by 15.6 percentage points (58%, $p < 0.05$) after one year, though that effect size again falls, to 6 percentage points after year three.

Overall, these results point to the benefits of considering other aspects of offenders' lives than just crime, as criminal behavior – or desistance therefrom – is often interwoven with family life. Our results illustrate that policies affecting offenders' recidivism can also have implications for their marital status. Moreover, the results touch on the indirect consequences of criminal behavior. A disproportionate number of children with criminal fathers grow up with divorced parents and/or with an incarcerated father (see e.g., Wakefield and Wildeman, 2014) thereby strengthening intergenerational persistence in poverty, risky behavior, and crime. DNA registration could help to break elements of this vicious circle by affecting the behavior of criminal fathers, reducing their recidivism rates and (possibly) the likelihood that children grow up without both parents.

6.4 Deterrence, detection, and elasticities

As suggested by the differences in effects for 'fast' and 'slow' charges, both deterrence and detection effects contribute meaningfully to the overall effects of DNA profiling. While DNA profiling reduces crime with 'fast' charges substantially, we have not found a single significant reduction to crimes with 'slow' charges throughout the entire analysis. As described in Section 3, we now use the distinction between convictions with charges filed within three weeks of the offense and charges filed more than three weeks after the offense, to separately identify the deterrence and detection effects of the DNA database. We also use these estimates to report implied elasticities of crime with respect to detection probability. Table 10 shows the estimated deterrence and detection effects as defined in Equations (8) and (9) and elasticities as defined in Equation (10). The table shows results for the main crime categories: all crime and property, violent, and weapons related crime separately.

The estimated deterrence effects are based on the above estimates for 'fast' charges, but scaled here by the inverse of the clearance rate. These estimates therefore show not only the change in convictions but the change in actual crimes committed. The ratios of the estimates in columns 3 and 4 to the baseline crime rate are the same as in columns 2 and 5 in Table 7.

Table 10 adds to our estimates based on those 'fast' charges by also estimating the detection effect. For all crime, we see that DNA profiling increases the number of new crimes that are detected by approximately 0.032 crimes, and the probability of any subsequent detected crime by 2.1 percentage points in the first year following DNA profiling. Respectively, these represent 4.2% and 3.4% of the pre-reform baselines (also scaled up by the clearance rate to estimate the actual number of crimes). These results show furthermore that the increasing number of hits beween offenders and evidence in the DNA database (Figure 1b) did indeed reflect increased detection and not only that DNA evidence served as substitutes for other detection work by the police. By examining detection effects separately for different types of crime, the table shows that the detection effect mainly operates for property crime.

Finally, the table shows estimated elasticities of crime with respect to detection prob-

ability. The estimated elasticity is -1.7 by year three, implying that a 1% increase in the likelihood of being caught reduces crime by 1.7%. That overall effect is about the same for property crimes. Violent crimes initially respond much more, with an elasticity of -7.5 in year 1 and -16.9 in year 2. That drops to -2 in year 3, about the same as for property crime and crime overall. The change in the violent crime elasticity may reflect a lag in offenders' learning about the now-higher probability of getting caught for this type of crime.

7 Discussion

Governments around the world are taking advantage of improvements in technology to change their approaches to criminal justice and to introduce new policies to deter offenders from crime and to aid police in identifying offenders. One popular policy is the introduction and expansion of DNA databases allowing police to identify repeat offenders by matching previously-charged offenders with DNA samples collected at the scene of crime. So far there has been relatively little analysis of the effects of DNA databases and similar technologies.

In this paper, we estimate the effects of DNA profiling on subsequent convictions, using full population register data from Denmark. To obviate the non-random selection into the DNA database, we exploit a 2005 expansion of the DNA database in Denmark, which increased the likelihood of being profiled from approximately 5% to almost 40% for offenders charged with a wide range of different crimes, to estimate offenders' responses to DNA profiling.

We find that DNA profiling has a deterrence effect on future criminal activity. Reductions in the probability of conviction for violent, property and weapons-related crime drive this overall decline in recidivism, although it appears to be offenders who were initially charged with violent crime who respond the most to DNA profiling. Both offenders who enter the DNA database for their first ever charge and individuals who have been charged before are deterred from committing subsequent crime, but when compared to their baseline recidivism

rates DNA profiling has the largest effect on first-time offenders.

Reducing criminal behavior should have positive effects on other aspects of deterred offenders' lives. Turning to non-crime effects of DNA profiling, we find that DNA profiling increases the likelihood of being married for first-time offenders. And we see indications that DNA profiling also leads to more stable relationships and decreases the risk of children of offenders growing up without their father present.

Finally, we exploit the nature of DNA databases to separate the deterrence and detection effects of this technology, and to provide first causal estimates of a central theoretical and policy parameter: the elasticity of crime with respect to detection probability. We find that DNA profiling leads to significant deterrence of future crime while also increasing the likelihood of offenders being detected. We thus illustrate that in the context of DNA databases, deterrence effects are upwardly biased if detection effects and clearance rates are not taken into account. We further estimate the overall elasticity of crime with respect to detection probability within a three year period to be -1.7. This implies that a 1% increase in the likelihood of being apprehended reduces crime by almost 2%. Our results thereby show that policies that increase the identification of criminal offenders are an effective tool to reduce crime and increase public safety.

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Tables and Figures

Table 1: Mean characteristics and subgroup sizes by timing of initial charge

	Pre reform		**Post reform**		**All**	
	Mean	SD	Mean	SD	Mean	SD
In DNA database	0.043	0.202	0.488	0.500	0.256	0.436
Covariates						
Age	22.276	3.644	22.017	3.578	22.152	3.615
Immigrant background	0.211	0.408	0.216	0.412	0.214	0.410
Has children	0.129	0.335	0.113	0.317	0.121	0.326
Single	0.853	0.354	0.862	0.345	0.858	0.349
Lives in 1 of 4 biggest citites	0.368	0.482	0.380	0.485	0.374	0.484
Years of education	10.914	1.910	10.818	1.887	10.868	1.900
Gross income (10.000s)	11.671	9.477	11.655	12.181	11.663	10.858
In employment	0.523	0.499	0.581	0.493	0.551	0.497
# prior charges	3.122	2.997	3.143	2.981	3.132	2.989
Crime type						
Property	0.595	0.491	0.521	0.500	0.560	0.496
Violence	0.247	0.431	0.296	0.457	0.271	0.444
Sexual	0.023	0.148	0.025	0.155	0.024	0.152
Drugs (penal)	0.021	0.144	0.024	0.153	0.023	0.148
Other penal	0.058	0.233	0.069	0.254	0.063	0.243
Weapon	0.056	0.230	0.065	0.246	0.060	0.238
Observations	34829		32082		66911	
Subgroups	Share	N	Share	N	Share	N
Previous charges						
First-time offenders	0.244	8508	0.241	7718	0.243	16226
Recidivists	0.756	26321	0.759	24364	0.757	50685
Age group						
18-23	0.662	23053	0.693	22244	0.677	45297
24-30	0.338	11776	0.307	9838	0.323	21614

Note: The table shows means and standard deviations for all covariates for the full sample and for those charged before and after the reform separately. The table also shows the number and proportion of the sample belonging to specific subgroups used in the analysis. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 2: Distribution of characteristics in the complier group

	Overall mean	Complier mean	Sig.
Covariates			
Aged 18-23	0.677	0.730	***
Aged 24-30	0.323	0.270	***
Imm. background	0.213	0.215	
Has children	0.121	0.114	***
Single	0.858	0.869	***
Lives in 1 of 4 biggest citites	0.374	0.389	***
Max. 10 years of educ.	0.474	0.495	***
Gross income above sample median	0.500	0.464	***
In employment	0.551	0.551	
First charge	0.243	0.198	***
Crime type			
Property	0.560	0.511	***
Violence	0.271	0.373	***
Sexual	0.023	0.036	***
Other penal	0.085	0.067	***
Weapon	0.060	0.016	***

$^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Note: The table shows the distribution of background characteristics in the complier group (column 2) following Angrist and Pischke (2009) and the overall sample (column 1). The final column indicates whether complier means are statistically significantly different from the overall sample mean (standard errors are calcuated on the basis of 100 bootstrapped samples).

Source: Own calculation based on Data from Statistics Denmark and the National Police.

Table 3: Unconditional balancing tests for each covariate

	(1)	(2)
Age	-0.052	-0.058
	(0.065)	(0.067)
Imm. background	0.008	0.012
	(0.008)	(0.008)
Single	-0.010	-0.013*
	(0.006)	(0.006)
Has children	-0.006	-0.007
	(0.006)	(0.006)
Lives in 1 of 4 biggest citites	0.005	0.011
	(0.009)	(0.009)
Years of education	-0.090**	-0.071*
	(0.034)	(0.035)
Gross income (10.000s)	-0.213	-0.058
	(0.180)	(0.187)
In employment	0.015	0.013
	(0.009)	(0.010)
Violence	0.023**	0.023**
	(0.007)	(0.007)
Property	-0.036***	-0.039***
	(0.008)	(0.009)
Sexual	-0.001	-0.000
	(0.002)	(0.002)
Weapon	0.003	0.002
	(0.004)	(0.004)
Other penal	0.012**	0.014**
	(0.005)	(0.005)
Observations	66911	66911
Running variable	X	X
Month FE		X

Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 Note: Table shows estimates from regressing each covariate on a dummy indicating whether charges occurred after the reform and running variables and month FE. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 4: Test for discontinuities in predicted subsequent convictions

	P(convicted)			**# convictions**		
Years	All	Fast	Slow	All	Fast	Slow
1 year	-0.001	-0.000	-0.000	-0.001	-0.001	-0.000
	(0.002)	(0.001)	(0.000)	(0.002)	(0.002)	(0.000)
2 years	-0.000	0.000	-0.000	-0.001	-0.001	-0.001
	(0.003)	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)
3 years	0.000	0.000	-0.001	-0.001	-0.000	-0.001
	(0.003)	(0.003)	(0.001)	(0.007)	(0.006)	(0.002)
Observations	66911	66911	66911	66911	66911	66911

Standard errors in parentheses. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Note: Table shows results from first regressing subsequent convictions on covariates measured before the initial charge (these covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month FE), and then regressing the predicted outcomes on the after-reform dummy and running variables. This is done to examine whether differences in covariates before and after the reform predict discontinuities in outcomes around the reform. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 5: Charges and convictions for crimes committed before DNA profiling

	P(charged)	# charges	P(convicted)	# convictions
	-0.006	0.015	-0.009	-0.011
	(0.019)	(0.062)	(0.013)	(0.015)
Observations	66911	66911	66911	66911

Note: The table shows estimated changes in the probability of being charged, number of charges, probability of being convicted, and number of convictions for crimes committed *before* DNA profiling but where charges were not pressed until *after* the DNA profiling. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 6: First stage estimation results

	DNA registration		
	(1)	(2)	(3)
Charged after reform	0.350***	0.347***	0.347***
	(0.007)	(0.007)	(0.007)
Observations	66911	66911	66911
Running variables	X	X	X
Covariates		X	X
Month FE			X

Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows estimates from first-stage regressions regressing DNA registration on timing of charge (before/after reform). Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 7: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions

	P(convicted)			**# convictions**		
	All	Fast	Slow	All	Fast	Slow
Years	(1)	(2)	(3)	(4)	(5)	(6)
1 year	-0.064***	-0.057**	-0.016	-0.093**	-0.075**	-0.017+
	(0.019)	(0.018)	(0.010)	(0.029)	(0.026)	(0.010)
2 years	-0.074**	-0.084***	-0.030+	-0.162***	-0.123**	-0.039*
	(0.024)	(0.023)	(0.016)	(0.048)	(0.041)	(0.019)
3 years	-0.047+	-0.067**	-0.006	-0.0128*	-0.126*	-0.002
	(0.0.25)	(0.024)	(0.019)	(0.061)	(0.051)	(0.026)
Observations	66911	66911	66911	66911	66911	66911
Placebo test	P(charged)			# charges		
Previous charges	0.001			0.048		
	(0.019)			(0.159)		
Observations	66911			66911		
Pre-reform baseline						
1 year	0.153	0.132	0.029	0.189	0.158	0.031

Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows 2SLS estimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform). Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 8: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions by initial crime type

		P(convicted)			# convictions		
		All	Fast	Slow	All	Fast	Slow
Crime type		(1)	(2)	(3)	(4)	(5)	(6)
Property	1 year	-0.052^{+}	-0.051^{+}	-0.013	-0.088*	-0.072^{+}	-0.016
		(0.029)	(0.027)	(0.016)	(0.044)	(0.040)	(0.016)
	2 years	-0.072*	-0.084*	-0.038	-0.170*	-0.121^{+}	-0.049^{+}
		(0.035)	(0.034)	(0.025)	(0.074)	(0.063)	(0.030)
	3 years	-0.035	-0.060^{+}	0.003	-0.097	-0.112	0.016
		(0.036)	(0.036)	(0.029)	(0.950)	(0.079)	(0.040)
	Observations	37443	37443	37443	37443	37443	37443
Violence	1 year	-0.067**	-0.058**	-0.012	-0.077**	-0.065*	-0.012
		(0.021)	(0.020)	(0.010)	(0.028)	(0.025)	(0.010)
	2 years	-0.085**	-0.082**	-0.025	-0.129**	-0.101**	-0.028
		(0.027)	(0.026)	(0.016)	(0.045)	(0.039)	(0.019)
	3 years	-0.066*	-0.074**	-0.011	-0.129*	-0.122*	-0.007
		(0.029)	(0.028)	(0.020)	(0.059)	(0.049)	(0.025)
	Observations	18116	18116	18116	18116	18116	18116
Sexual	1 year	0.020	0.002	0.016	0.015	-0.009	0.024
		(0.040)	(0.033)	(0.023)	(0.045)	(0.037)	(0.027)
	2 years	0.063	-0.008	0.045	0.023	-0.033	0.057
		(0.064)	(0.055)	(0.042)	(0.089)	(0.072)	(0.045)
	3 years	0.071	0.033	0.033	0.025	-0.003	0.028
		(0.073)	(0.066)	(0.051)	(0.125)	(0.096)	(0.061)
	Observations	1576	1576	1576	1576	1576	1576
Other penal	1 year	-0.119*	-0.086	-0.034	-0.173*	-0.134^{+}	-0.039
		(0.060)	(0.057)	(0.027)	(0.081)	(0.075)	(0.028)
	2 years	-0.035	-0.048	-0.021	-0.219^{+}	-0.187	-0.032
		(0.082)	(0.076)	(0.047)	(0.131)	(0.117)	(0.052)
	3 years	-0.095	-0.102	-0.074	-0.362*	-0.277*	-0.085
		(0.087)	(0.083)	(0.058)	(0.163)	(0.139)	(0.074)
	Observations	5735	5735	5735	5735	5735	5735
Weapon	1 year	-0.314	-0.201	-0.159	-0.420	-0.251	-0.170
		(0.230)	(0.213)	(0.101)	(0.301)	(0.272)	(0.107)
	2 years	-0.393	-0.466^{+}	-0.103	-0.857^{+}	-0.621	-0.236
		(0.286)	(0.280)	(0.164)	(0.502)	(0.435)	(0.191)
	3 years	-0.097	-0.227	-0.018	-0.588	-0.338	-0.250
		(0.290)	(0.285)	(0.195)	(0.616)	(0.514)	(0.252)
	Observations	4041	4041	4041	4041	4041	4041
Pre-reform baseline, 1 year							
Property		0.168	0.143	0.033	0.209	0.174	0.035
Violence		0.140	0.121	0.025	0.165	0.139	0.026
Sexual		0.042	0.031	0.011	0.043	0.032	0.011
Other penal		0.113	0.099	0.018	0.143	0.124	0.019
Weapon		0.159	0.140	0.027	0.192	0.164	0.028

Standard errors in parentheses + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 Note: Table shows 2SLS estimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform) by type of initial crime and days between crime and charge. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges and month fixed effects. Standard errors are clustered by personal id number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table 9: Effects of DNA profiling on family outcomes, overall and by previous charges

	All Offenders			First charge			Recidivist		
Year	Married (1)	Same partner (2)	Living with child and mother (3)	Married (4)	Same partner (5)	Living with child and mother (6)	Married (7)	Same partner (8)	Living with child and mother (9)
1 year	0.007	0.110	0.124+	0.030*	0.004	-0.047	0.002	0.131+	0.156*
	(0.006)	(0.069)	(0.068)	(0.013)	(0.163)	(0.168)	(0.007)	(0.074)	(0.072)
2 years	0.002	0.070	0.053	0.048*	0.138	-0.000	-0.009	0.051	0.065
	(0.008)	(0.067)	(0.070)	(0.019)	(0.166)	(0.173)	(0.009)	(0.071)	(0.074)
3 years	0.011	0.067	0.067	0.068*	0.155	-0.080	-0.002	0.044	0.060
	(0.011)	(0.068)	(0.068)	(0.024)	(0.166)	(0.176)	(0.011)	(0.071)	(0.074)
Placebo	0.001			-0.006			0.002		
	(0.005)			(0.011)			(0.005)		
Observations	66911	9527	11767	16226	2532	2148	50685	6995	9619
Pre-reform baseline									
Year 1	0.058	0.467	0.307	0.065	0.551	0.484	0.050	0.436	0.268

Standard errors in parentheses. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows IV estimates of regressing family outcomes on DNA profiling (instrumented by timing of initial charge - before/after reform). Panel A shows results for the likelihood of being married. Panel B shows results for the likelihood of living with the same partner for those who had a partner prior to charge. Panel C shows results for the likelihood of father living with child's mother for all children born prior to father's charge (some of these children will not have been born by year -1). Covariates include child's age and gender, and father's age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies, and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculation based on Data from Statistics Denmark and the National Police.

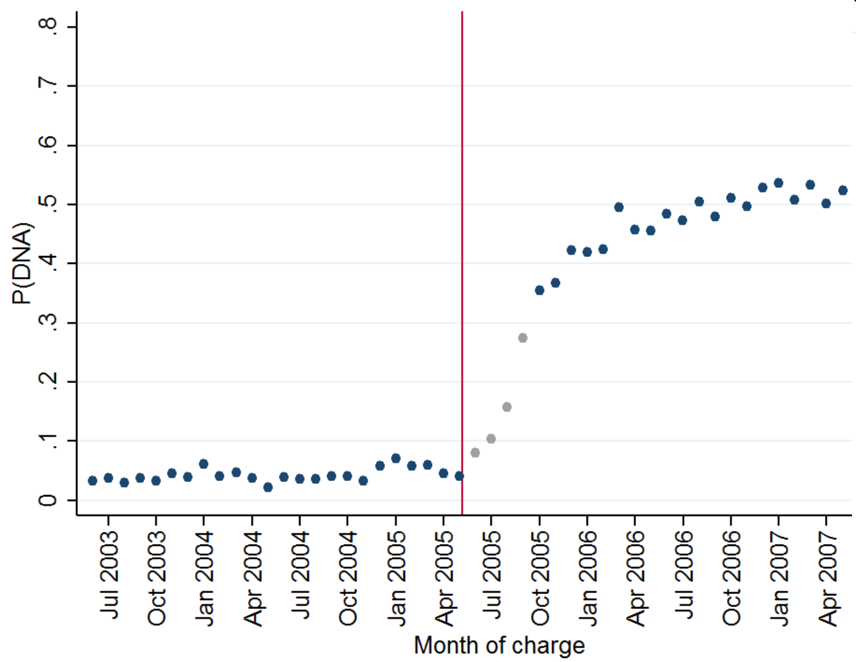
Table 10: Deterrence and detection effects on subsequent new crime

	Deterrence effect Δ		**Detection effect δ**		Clearance rate	Elasticity of #new crimes
	P(new crime)	#new crimes	P(new crime)	#new crimes	w. 3 weeks $p\pi$	with respect to p
	(1)	(2)	(3)	(4)	(5)	(6)
A:Any Crime						
1 year	-0.241 ***	-0.322 ***	0.021 +	0.032 *	0.249	-2.5
2 years	-0.349 ***	-0.512 ***	0.022 +	0.038 +		-3.4
3 years	-0.280 ***	-0.521 ***	0.035 *	0.076 **		-1.7
B: Property						
1 year	-0.197 **	-0.267 ***	0.020 *	0.028 *	0.174	-1.7
2 years	-0.281 ***	-0.357 ***	0.020	0.020		-3.1
3 years	-0.256 ***	-0.302 *	0.023	0.029		-1.8
C: Violence						
1 year	-0.043 **	-0.049 **	0.002	0.004	0.616	-7.5
2 years	-0.052 *	-0.055 *	0.002	0.002		-16.9
3 years	-0.049 *	-0.056 +	0.017 *	0.017 +		-2.0
D: Weapon						
1 year	-0.011 *	-0.012 *	0.000	0.001	0.827	
2 years	-0.022 **	-0.022 *	0.000	0.000		
3 years	-0.031 ***	-0.036 ***	0.000	0.000		
Pre-reform baseline / clearance rate ($\bar{p}$), 1 year						
	P(new crime)	# new crimes				
Any Crime	0.614	0.759				
Property	0.523	0.638				
Violence	0.080	0.086				
Weapon	0.013	0.013				

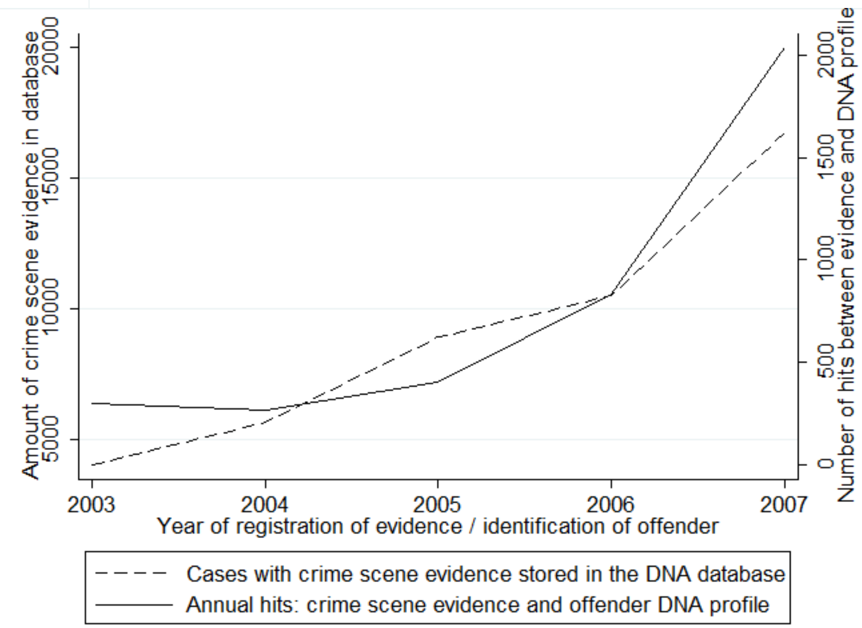
+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows estimates of deterrence and detection effects calculated on the basis of IV-estimation (including covariates and month FE) from 100 bootstrapped samples. Clearing rates were calculated on the basis of all charges and all reported crime in 2005. In these measures we excluded crime types such as bicycle theft which is heavily reported (often for insurance purposes) but rarely solved and leading to a charge (<10% of the time) in order not to inflate estimates by an extremely low clearance rate. The fraction of crimes solved within 3 weeks is 0.623 overall, 0.569 for property crimes, 0.752 for violent crimes, and 0.894 for weapons crimes. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Figure 1: DNA profiling of offenders and DNA samples from crime scenes

(a) Likelihood of DNA profiling



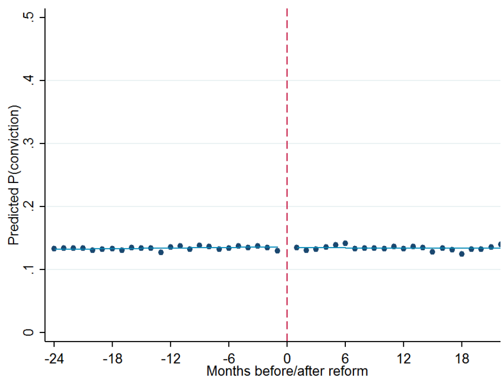
(b) DNA samples from crime scenes and hits between evidence and offenders



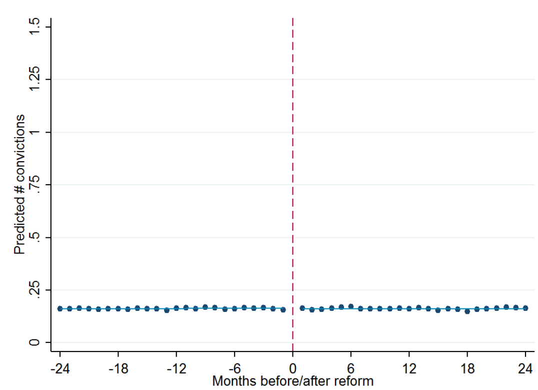
Note: Figure A shows the fraction of offenders in the sample who are registered in the DNA register by month of charge. The vertical line marks the timing of the reform. Source: Own calculations based on Data from Statistics Denmark and the National Police. Figure B shows the number of cases with DNA evidence from crime scenes in the DNA database (dashed line) and the annual number of hits between offender profiles and crime scene evidence (solid line). Source: Own calculations based on Data from Statistics Denmark and the National Police.

Figure 2: Predicted probability of conviction and number of convictions from observable characteristics around the timing of the reform

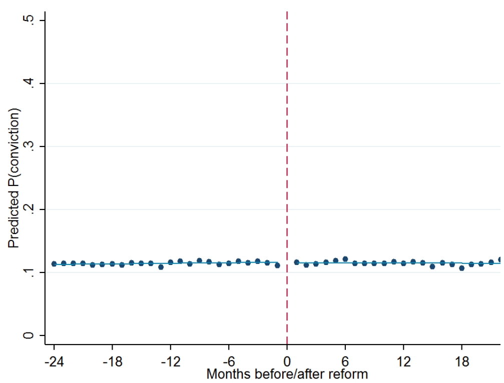
(a) P(conviction), 1 year



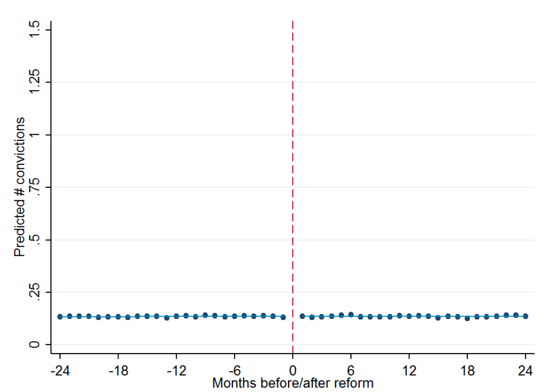
(b) # convictions, 1 year



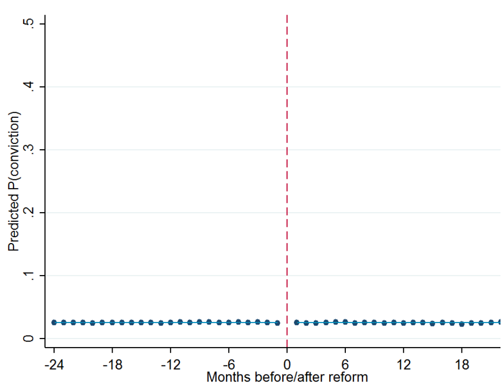
(c) P(conviction w. fast charge), 1 year



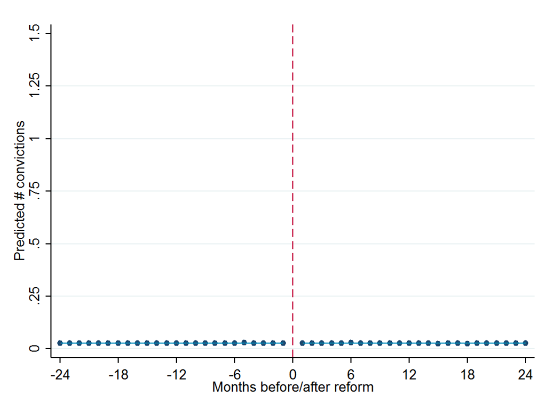
(d) # convictions w. fast charge, 1 year



(e) P(conviction w. slow charge), 1 year



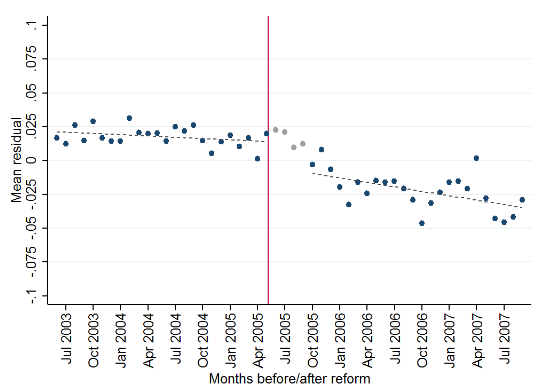
(f) # convictions w. slow charge, 1 year



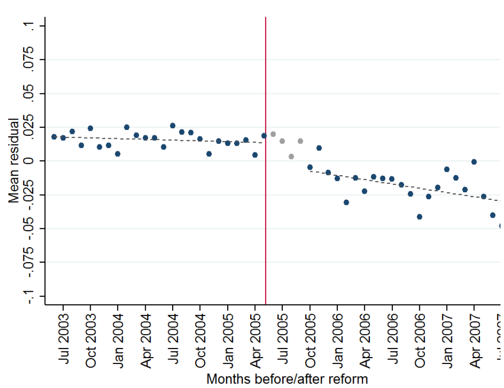
Note: Figures show predicted probability of any conviction and number of convictions for crimes that occurred within a year after a given crime charge, predicted from estimation results regressing outcomes on covariates, crime types and month FE. Figures A, C and E show predictions for the binary outcomes. Figure B, D and F show predictions for the number of subsequent convictions. Source: Own calculations based on Data from Statistics Denmark and the National Police.

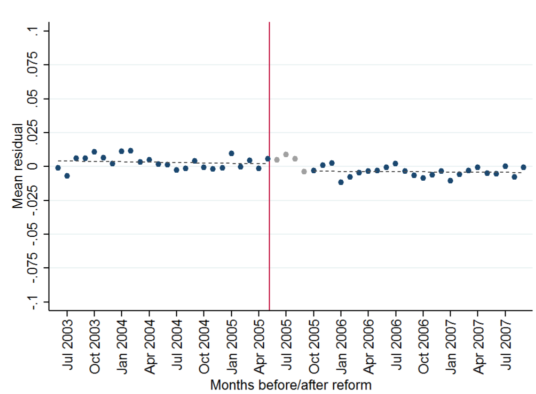
Figure 3: Monthly means of binary outcomes around the timing of the reform, by timing between date of crime and date of charge

(a) P(conviction), 1 year



(b) P(conviction w. fast charge), 1 year (c) P(conviction w. slow charge), 1 year

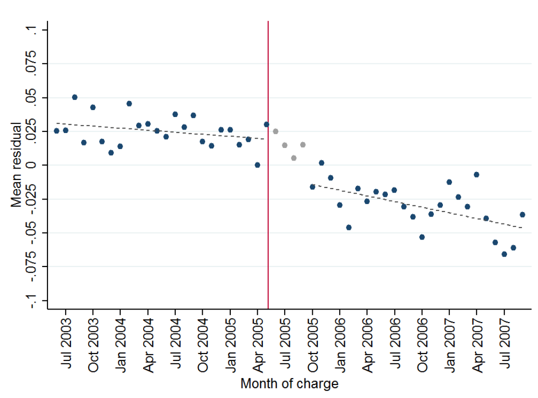




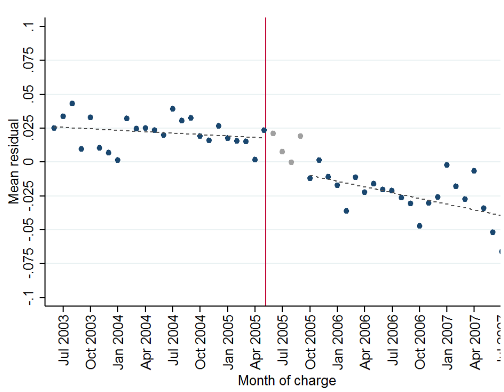
Note: Figures show monthly means of probability of receiving at least one conviction within one year. Figure A shows results for all convictions, Figure B shows means for charges filed within three weeks from the date of crime, and Figure C shows results for crime charges filed after three weeks from the date of crime. We condition on covariates in all figures. Therefore the figures show deviations around the conditional sample mean and not absolute levels. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Figure 4: Monthly means of count outcomes around the timing of the reform, by timing between date of crime and date of charge

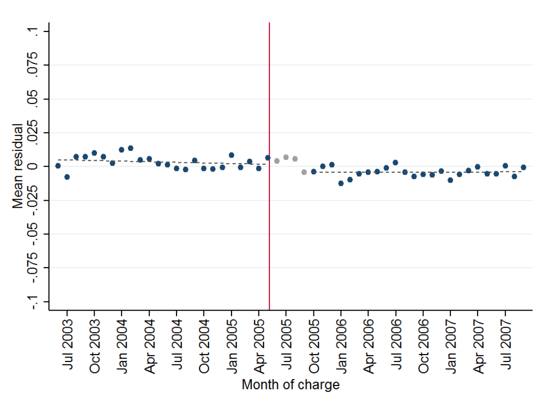
(a) # convictions, 1 year



(b) # convictions w. fast charge, 1 year



(c) # convictions w. slow charge, 1 year



Note: Figures show monthly means number of convictions within one year. Figure A shows results for all convictions, Figure B shows means for charges filed within three weeks from the date of crime, and Figure C shows results for crime charges filed after three weeks from the date of crime. We condition on covariates in all figures. Therefore the figures show deviations around the conditional sample mean and not absolute levels. Source: Own calculations based on Data from Statistics Denmark and the National Police.

A For Online Publication Only: Appendix Tables and Figures

Table A-1: Crime categories

Criminal Code	Main categories of crime	Subcategories of crime	Our category
Penal Code	All sexual offenses	Incest	Sexual
		Rape	Sexual
		Pedophilia	Sexual
		Voyerism, flashing	Sexual
		Other sexual violations	Sexual
	Violent crime	Violence against public servant	Violence
		Disturbance of public peace	Violence
		Murder, manslaughter (+ attempted)	Violence
		Simple violence	Violence
		Major violence	Violence
		Threats	Violence
		Other violent assaults	Violence
	Property crimes	Fraud	Property
		Arson	Property
		Theft	Property
		Burglary	Property
		Robbery	Property
		Vandalism	Property
		Other property crime	Property
	Other crimes against penal code	Crimes against/as a public servant	Other (penal)
		Drug smuggling or sales	Other (penal)
		Obstruction of justice	Other (penal)
		Restrain orders	Other (penal)
		Other crimes, penal code	Other (penal)
Special Acts	Violations of Traffic Act	Accidents and speeding	-
		Traffic accidents w. alcohol	-
		Drunk driving	-
		Other traffic offenses	-
	Violations of Drug Act	Possession and or drugs sales -	
	Violations of Weapons/Arms Act	Explosives, firearms, knives	Weapon
	Smuggling, construction, health, social fraud, other		-

Table A-2: Mean of crime and family outcomes, by timing of charge relative to the reform

	Pre reform	Post reform	Pre reform	Post reform	Pre reform	Post reform
A) Crime outcomes:	*P(conviction)*		*# convictions*			
Any Crime						
1 year	0.153	0.114	0.189	0.133		
2 years	0.298	0.246	0.449	0.341		
3 years	0.375	0.338	0.652	0.553		
Property						
1 year	0.091	0.058	0.111	0.067		
2 years	0.186	0.136	0.263	0.176		
3 years	0.238	0.198	0.375	0.292		
Violence						
1 year	0.049	0.044	0.053	0.047		
2 years	0.103	0.096	0.120	0.112		
3 years	0.141	0.138	0.177	0.170		
Sexual						
1 year	0.002	0.001	0.002	0.001		
2 years	0.003	0.002	0.003	0.002		
3 years	0.005	0.003	0.005	0.004		
Other penal						
1 year	0.012	0.008	0.012	0.009		
2 years	0.034	0.026	0.036	0.027		
3 years	0.051	0.047	0.055	0.050		
Weapon						
1 year	0.011	0.009	0.011	0.009		
2 years	0.026	0.023	0.027	0.024		
3 years	0.038	0.035	0.051	0.048		
Observations	34829	32082	34829	32082		
B) Family outcomes:	*Married*		*Same partner*		*Living with child and mother*	
1 year	0.058	0.042	0.467	0.444	0.307	0.290
2 years	0.064	0.050	0.418	0.390	0.288	0.268
3 years	0.075	0.064	0.386	0.347	0.280	0.252
Observations	34829	32082	5106	4421	6614	5153

Note: The table shows means of crime and family outcomes for those charged before and after the reform separately. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-3: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions by previous charges

	P(convicted)			**# convictions**		
	All	Fast	Slow	All	Fast	Slow
	(1)	(2)	(3)	(4)	(5)	(6)
First charge						
1 year	-0.048^{+}	-0.043^{+}	-0.004	-0.037	-0.034	-0.002
	(0.025)	(0.022)	(0.011)	(0.028)	(0.025)	(0.011)
2 years	-0.081*	-0.081*	-0.007	-0.065	-0.060	-0.005
	(0.035)	(0.032)	(0.019)	(0.048)	(0.042)	(0.020)
3 years	-0.030	-0.055	0.016	-0.029	-0.046	0.017
	(0.040)	(0.036)	(0.023)	(0.063)	(0.054)	(0.027)
Observations	16226	16226	16226	16226	16226	16226
Recidivist						
1 year	-0.067**	-0.059**	-0.018	-0.105**	-0.084**	-0.021^{+}
	(0.023)	(0.022)	(0.012)	(0.035)	(0.032)	(0.013)
2 years	-0.070*	-0.082**	-0.035^{+}	-0.182**	-0.136**	-0.046*
	(0.028)	(0.027)	(0.019)	(0.058)	(0.050)	(0.023)
3 years	-0.048^{+}	-0.067^{+}	-0.010	-0.146^{+}	-0.141*	0.012
	(0.029)	(0.028)	(0.023)	(0.075)	(0.062)	(0.031)
Observations	50685	50685	50685	50685	50685	50685
Pre-reform baseline, 1 year						
First charge	0.061	0.052	0.009	0.068	0.058	0.010
Recidivist	0.183	0.157	0.036	0.228	0.190	0.037

Standard errors in parentheses + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 Note: Table shows 2SLS estimates of the effect of DNA profiling on subsequent crime charges by number of prior crime charges (no prior charges vs. 1-10 prior charges). Separate estimates for first-time offenders and recidivists are obtained by interacting the reform dummy with the first-time and recidivist dummies. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-4: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions by age

	P(convicted)			**# convictions**		
	All	Fast	Slow	All	Fast	Slow
	(1)	(2)	(3)	(4)	(5)	(6)
Aged 18-23						
1 year	-0.089***	-0.079***	-0.026*	-0.129***	-0.101**	-0.028*
	(0.023)	(0.022)	(0.012)	(0.035)	(0.032)	(0.012)
2 years	-0.085**	-0.094***	-0.039*	-0.189**	-0.141**	-0.048*
	(0.028)	(0.027)	(0.020)	(0.058)	(0.050)	(0.024)
3 years	-0.050+	-0.067*	-0.008	-0.143+	-0.140*	-0.003
	(0.029)	(0.028)	(0.023)	(0.075)	(0.062)	(0.032)
Observations	45297	45297	45297	45297	45297	45297
Aged 24-30						
1 year	0.003	0.002	0.014	0.005	-0.008	0.013
	(0.033)	(0.030)	(0.019)	(0.048)	(0.041)	(0.020)
2 years	-0.049	-0.059	-0.008	-0.091	-0.077	-0.015
	(0.043)	(0.040)	(0.029)	(0.080)	(0.067)	(0.031)
3 years	-0.043	-0.068	-0.000	-0.089	-0.088	-0.001
	(0.046)	(0.043)	(0.033)	(0.099)	(0.084)	(0.039)
Observations	21614	21614	21614	21614	21614	21614
Pre-reform baseline, 1 year						
Aged 18-23	0.177	0.152	0.033	0.219	0.184	0.035
Aged 24-30	0.107	0.092	0.021	0.129	0.108	0.022

Standard errors in parentheses. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows 2SLS estimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform) by age-group and days between crime and charge. Covariates include immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal id number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-5: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions by child/no child at time of charge

	P(convicted)			**# convictions**		
	All	Fast	Slow	All	Fast	Slow
	(1)	(2)	(3)	(4)	(5)	(6)
Child						
1 year	-0.048	-0.058	0.022	-0.063	-0.080	0.017
	(0.050)	(0.047)	(0.027)	(0.072)	(0.063)	(0.028)
2 years	-0.141^{*}	-0.177^{**}	0.009	-0.193^{+}	-0.203^{*}	0.010
	(0.066)	(0.062)	(0.045)	(0.116)	(0.099)	(0.049)
3 years	-0.115^{+}	-0.188^{**}	0.050	-0.182	-0.215^{+}	0.033
	(0.069)	(0.066)	(0.053)	(0.146)	(0.124)	(0.065)
Observations	8113	8113	8113	8113	8113	8113
No child						
1 year	-0.066^{**}	-0.056^{**}	-0.020^{+}	-0.096^{**}	-0.075^{**}	-0.022^{+}
	(0.021)	(0.020)	(0.011)	(0.031)	(0.028)	(0.011)
2 years	-0.066^{**}	-0.072^{**}	-0.035^{*}	-0.158^{**}	-0.113^{**}	-0.045^{*}
	(0.025)	(0.024)	(0.017)	(0.051)	(0.044)	(0.021)
3 years	-0.039	-0.052^{*}	-0.013	-0.122^{+}	-0.115^{*}	-0.007
	(0.026)	(0.025)	(0.020)	(0.066)	(0.055)	(0.028)
Observations	58798	58798	58798	58798	58798	58798
Pre-reform baseline, 1 year						
Child	0.124	0.106	0.027	0.154	0.124	0.030
No child	0.157	0.135	0.030	0.194	0.163	0.031

Standard errors in parentheses. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows 2SLS estimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform) by parent-status and days between crime and charge. Covariates include age, immigrant background, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal id number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-6: Effects of DNA profiling on subsequent convictions (accumulated) by different conditioning sets

	P(convicted)			**# convictions**		
	(1)	(2)	(3)	(1)	(2)	(3)
A: All convictions						
1 year	-0.065**	-0.065***	-0.064***	-0.095**	-0.095***	-0.093**
	(0.020)	(0.019)	(0.019)	(0.029)	(0.028)	(0.029)
2 years	-0.074**	-0.075**	-0.074**	-0.174***	-0.173***	-0.162***
	(0.025)	(0.023)	(0.024)	(0.050)	(0.047)	(0.048)
3 years	-0.047+	-0.049*	-0.047+	-0.140*	-0.140*	-0.128*
	(0.026)	(0.024)	(0.025)	(0.065)	(0.060)	(0.061)
B: Convictions w. fast charge						
1 year	-0.057**	-0.057**	-0.057**	-0.078**	-0.077**	-0.075**
	(0.019)	(0.018)	(0.018)	(0.026)	(0.025)	(0.026)
2 years	-0.082***	-0.083***	-0.084***	-0.129**	-0.129**	-0.123**
	(0.024)	(0.022)	(0.023)	(0.042)	(0.040)	(0.041)
3 years	-0.064*	-0.056**	-0.067**	-0.129*	-0.131**	-0.126*
	(0.025)	(0.023)	(0.024)	(0.053)	(0.050)	(0.051)
C: Convictions w. slow charge						
1 year	-0.016	-0.015	-0.016	-0.018+	-0.017+	-0.017+
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
2 years	-0.034*	-0.034*	-0.030+	-0.045*	-0.044*	-0.039*
	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)
3 years	-0.012	-0.011	- 0.006	-0.011	-0.009	-0.002
	(0.019)	(0.019)	(0.019)	(0.026)	(0.026)	(0.026)
Observations	66911	66911	66911	66911	66911	66911
Running variables	X	X	X	X	X	X
Covariates		X	X		X	X
Month FE			X			X

Standard errors in parentheses + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 Note: Table shows 2SLS estimates of regressing subsequent crime on DNA profiling (instrumented by timing of initial charge - before/after reform) by different conditioning sets. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-7: Effects of DNA profiling on subsequent convictions (accumulated) by different bandwidth specifications

	P(convicted)				**# convictions**			
	BW:12	BW:18	BW:24	BW:30	BW:12	BW:18	BW:24	BW:30
A: All convictions								
1 year	-0.054^{+}	-0.064^{**}	-0.064^{***}	-0.073^{***}	-0.087^{*}	-0.106^{***}	-0.093^{**}	-0.108^{***}
	(0.030)	(0.022)	(0.019)	(0.017)	(0.042)	(0.032)	(0.029)	(0.025)
2 years	-0.041	-0.063^{*}	-0.074^{**}	-0.099^{***}	-0.107	-0.164^{**}	-0.162^{***}	-0.212^{***}
	(0.037)	(0.027)	(0.024)	(0.021)	(0.068)	(0.052)	(0.048)	(0.044)
3 years	-0.040	-0.061^{*}	-0.047^{+}	-0.069^{**}	-0.117	-0.175^{**}	-0.128^{*}	-0.182^{**}
	(0.038)	(0.028)	(0.025)	(0.022)	(0.088)	(0.067)	(0.061)	(0.057)
B: Convictions w. fast charge								
1 year	-0.054^{+}	-0.064^{**}	-0.057^{**}	-0.066^{***}	-0.061	-0.096^{***}	-0.075^{**}	-0.089^{***}
	(0.028)	(0.021)	(0.018)	(0.016)	(0.039)	(0.029)	(0.026)	(0.023)
2 years	-0.069^{*}	-0.079^{**}	-0.084^{***}	-0.105^{***}	-0.088	-0.135^{**}	-0.123^{**}	-0.166^{***}
	(0.035)	(0.026)	(0.023)	(0.020)	(0.059)	(0.044)	(0.041)	(0.038)
3 years	-0.065^{+}	-0.078^{**}	-0.067^{**}	-0.085^{***}	-0.119	-0.157^{**}	-0.126^{*}	-0.162^{***}
	(0.037)	(0.027)	(0.023)	(0.021)	(0.074)	(0.055)	(0.051)	(0.047)
C: Convictions w. slow charge								
1 year	-0.024	-0.009	-0.016	-0.015^{+}	-0.026	-0.011	-0.017^{+}	-0.018^{*}
	(0.016)	(0.012)	(0.010)	(0.009)	(0.016)	(0.012)	(0.010)	(0.009)
2 years	-0.011	-0.021	-0.030^{+}	-0.035^{*}	-0.019	-0.029	-0.039^{*}	-0.046^{**}
	(0.024)	(0.019)	(0.016)	(0.015)	(0.028)	(0.022)	(0.019)	(0.017)
3 years	-0.007	-0.020	-0.006	-0.017	0.001	-0.018	-0.002	-0.020
	(0.028)	(0.022)	(0.019)	(0.017)	(0.037)	(0.029)	(0.026)	(0.024)
Observations	33509	50245	66911	83147	33509	50245	66911	83147

Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows 2SLS estimates of regressing subsequent crime on DNA profiling (instrumented by timing of initial charge - before/after reform) by different bandwidth specifications. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-8: Effects of DNA profiling on subsequent convictions (accumulated) by different caps on prior charges

	P(convicted)			**# convictions**		
	Max. 5	Max. 10	Max. 15	Max. 5	Max. 10	Max. 15
A: All convictions						
1 year	-0.053**	-0.064***	-0.078***	-0.054*	-0.093**	-0.115***
	(0.019)	(0.019)	(0.020)	(0.026)	(0.029)	(0.031)
2 years	-0.058*	-0.074**	-0.079***	-0.097*	-0.162***	-0.190***
	(0.025)	(0.024)	(0.023)	(0.044)	(0.048)	(0.050)
3 years	-0.023	-0.047+	-0.053*	-0.060	-0.128*	-0.153*
	(0.027)	(0.025)	(0.024)	(0.057)	(0.061)	(0.064)
B: Convictions w. fast charge						
1 year	-0.046*	-0.057**	-0.069***	-0.042+	-0.075**	-0.090**
	(0.018)	(0.018)	(0.019)	(0.024)	(0.026)	(0.028)
2 years	-0.065**	-0.084***	-0.085***	-0.069+	-0.123**	-0.139**
	(0.024)	(0.023)	(0.023)	(0.038)	(0.041)	(0.044)
3 years	-0.044+	-0.067**	-0.065**	-0.049	-0.126*	-0.139*
	(0.025)	(0.024)	(0.023)	(0.048)	(0.051)	(0.054)
C: Convictions w. slow charge						
1 year	-0.011	-0.016	-0.024*	-0.012	-0.017+	-0.026*
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
2 years	-0.024	-0.030+	-0.039*	-0.028	-0.039*	-0.051*
	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)
3 years	-0.008	-0.006	-0.013	-0.011	-0.002	-0.014
	(0.019)	(0.019)	(0.020)	(0.024)	(0.026)	(0.027)
Observations	51550	66911	76531	51550	66911	76531

Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows 2SLS estimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform) by different caps on prior charges. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-9: Reduced form estimates predicting probability of convictions (with a charge occurring within three weeks of the crime date) from timing of initial charge in placebo samples

	P(convicted) w. fast charge	
Reform year	1 year	3 years
2002, placebo reform	0.001	0.002
	(0.007)	(0.009)
2003, placebo reform	0.005	-0.007
	(0.007)	(0.008)
2004, placebo reform	0.006	-0.011
	(0.007)	(0.008)
2005, actual reform	-0.020**	-0.023**
	(0.006)	(0.008)
2006, placebo reform	-0.009	0.011
	(0.006)	(0.008)

Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows reduced form estimates from regressing subsequent convictions on a "after-reform"-dummy (along with running variables, covariates and month FE) in a series of placebo samples. The placebo samples mirrors the original sample except that the reform is artificially set to occur in e.g. 2002 instead of 2005, and as in the original samples the sampling window is defined as +/-24 months around the reform (except from June-September in the reform year). Standard errors are clustered on personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police

Table A-10: Effects of DNA profiling on subsequent accumulated probability of crime charge and number of crime charges

	A: All charges		B: Fast charges		C: Slow charges	
	P(charged)	#charges	P(charged)	#charges	P(charged)	#charges
1 year	-0.013	-0.037	-0.035	-0.080	0.017	0.044
	(0.023)	(0.087)	(0.023)	(0.066)	(0.020)	(0.036)
2 years	-0.019	-0.102	-0.040^{+}	-0.097	0.001	-0.004
	(0.024)	(0.140)	(0.024)	(0.104)	(0.023)	(0.057)
3 years	-0.004	-0.014	-0.033	-0.083	0.007	0.069
	(0.023)	(0.183)	(0.024)	(0.133)	(0.024)	(0.075)
Observations	66911	66911	66911	66911	66911	66911
Pre-reform baseline						
1 year	0.332	0.718	0.287	0.533	0.130	0.185

Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows IVestimates of regressing subsequent crime on DNA registration (instrumented by timing of initial charge - before/after reform). Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police

Table A-11: Effects of DNA profiling on subsequent convictions - adjusted for time spent incarcerated

	# convictions			**# convictions (fast charge)**			**# convictions (slow charge)**		
	Adj. no cap	Adj. cap=0.5	Adj. cap=0.75	Adj. no cap	Adj. cap=0.5	Adj. cap=0.75	Adj. no cap	Adj. cap=0.5	Adj. cap=0.75
1 year	-0.098^{**}	-0.104^{**}	-0.100^{**}	-0.078^{**}	-0.084^{**}	-0.081^{**}	-0.020^{+}	-0.020^{+}	-0.020^{+}
	(0.032)	(0.032)	(0.032)	(0.030)	(0.029)	(0.029)	(0.011)	(0.011)	(0.011)
Observations	66901	66911	66911	66901	66911	66911	66901	66911	66911
2 years	-0.185^{***}	-0.187^{***}	-0.186^{***}	-0.141^{**}	-0.143^{**}	-0.142^{**}	-0.044^{*}	-0.044^{*}	-0.044^{*}
	(0.053)	(0.053)	(0.053)	(0.046)	(0.046)	(0.046)	(0.021)	(0.021)	(0.021)
Observations	66907	66911	66911	66907	66911	66911	66907	66911	66911
3 years	-0.155^{*}	-0.156^{*}	-0.156^{*}	-0.146^{**}	-0.147^{**}	-0.146^{**}	-0.009	-0.009	-0.009
	(0.068)	(0.068)	(0.068)	(0.056)	(0.056)	(0.056)	(0.028)	(0.028)	(0.028)
Observations	66908	66911	66911	66908	66911	66911	66908	66911	66911

Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows IV estimates of regressing subsequent convictions on DNA profiling (instrumented by timing of initial charge - before/after reform). Number of subsequent convictions have been divided by the proportion of the follow up period not spent incarcerated with different caps on the maximum proportion of time spent incarcerated. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies, and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculation based on Data from Statistics Denmark.

Table A-12: Effects of DNA profiling, including summer months

	Full sample	First charge	Recidivist	Aged 18-23	Aged 24-30	Child	No child
P(convicted), all crime							
1 year	-0.036	-0.041	-0.039	-0.076*	0.072	0.068	-0.049
	(0.031)	(0.042)	(0.038)	(0.038)	(0.055)	(0.089)	(0.033)
2 years	-0.062	-0.099+	-0.057	-0.094*	0.022	-0.063	-0.061
	(0.038)	(0.059)	(0.046)	(0.045)	(0.072)	(0.114)	(0.040)
3 years	-0.041	-0.031	-0.049	-0.057	-0.007	-0.001	-0.046
	(0.039)	(0.065)	(0.047)	(0.046)	(0.077)	(0.119)	(0.041)
# convictions, all crime							
1 year	-0.075	-0.038	-0.038	-0.073*	0.066	0.037	-0.044
	(0.047)	(0.038)	(0.036)	(0.036)	(0.051)	(0.083)	(0.031)
2 years	-0.146+	-0.116*	-0.084+	-0.111*	-0.026	-0.149	-0.079*
	(0.076)	(0.054)	(0.044)	(0.081)	(0.067)	(0.108)	(0.039)
3 years	-0.122	-0.086	-0.070	-0.074	-0.067	-0.141	-0.061
	(0.097)	(0.060)	(0.046)	(0.045)	(0.073)	(0.115)	(0.040)
P(convicted), crime solved fast							
1 year	-0.035	-0.020	-0.094	-0.136*	0.095	0.126	-0.099*
	(0.029)	(0.048)	(0.058)	(0.057)	(0.083)	(0.148)	(0.050)
2 years	-0.087*	-0.042	-0.182+	-0.197*	-0.015	0.006	-0.164*
	(0.036)	(0.082)	(0.044)	(0.093)	(0.128)	(0.200)	(0.082)
3 years	-0.069+	-0.018	-0.161	-0.137	-0.102	0.070	-0.144
	(0.038)	(0.107)	(0.121)	(0.121)	(0.159)	(0.251)	(0.105)
# convictions, crime solved fast							
1 year	-0.060	-0.030	-0.071	-0.105*	0.063	0.060	-0.074
	(0.043)	(0.043)	(0.053)	(0.053)	(0.071)	(0.122)	(0.046)
2 years	-0.123+	-0.075	-0.143+	-0.158+	-0.034	-0.092	-0.126+
	(0.066)	(0.071)	(0.082)	(0.081)	(0.108)	(0.167)	(0.071)
3 years	-0.130	-0.096	-0.149	-0.139	-0.120	-0.054	-0.138
	(0.082)	(0.090)	(0.103)	(0.102)	(0.133)	(0.212)	(0.089)
First stage on probability of DNA profiling:							
Charged post reform	0.212***						
	(0.006)						

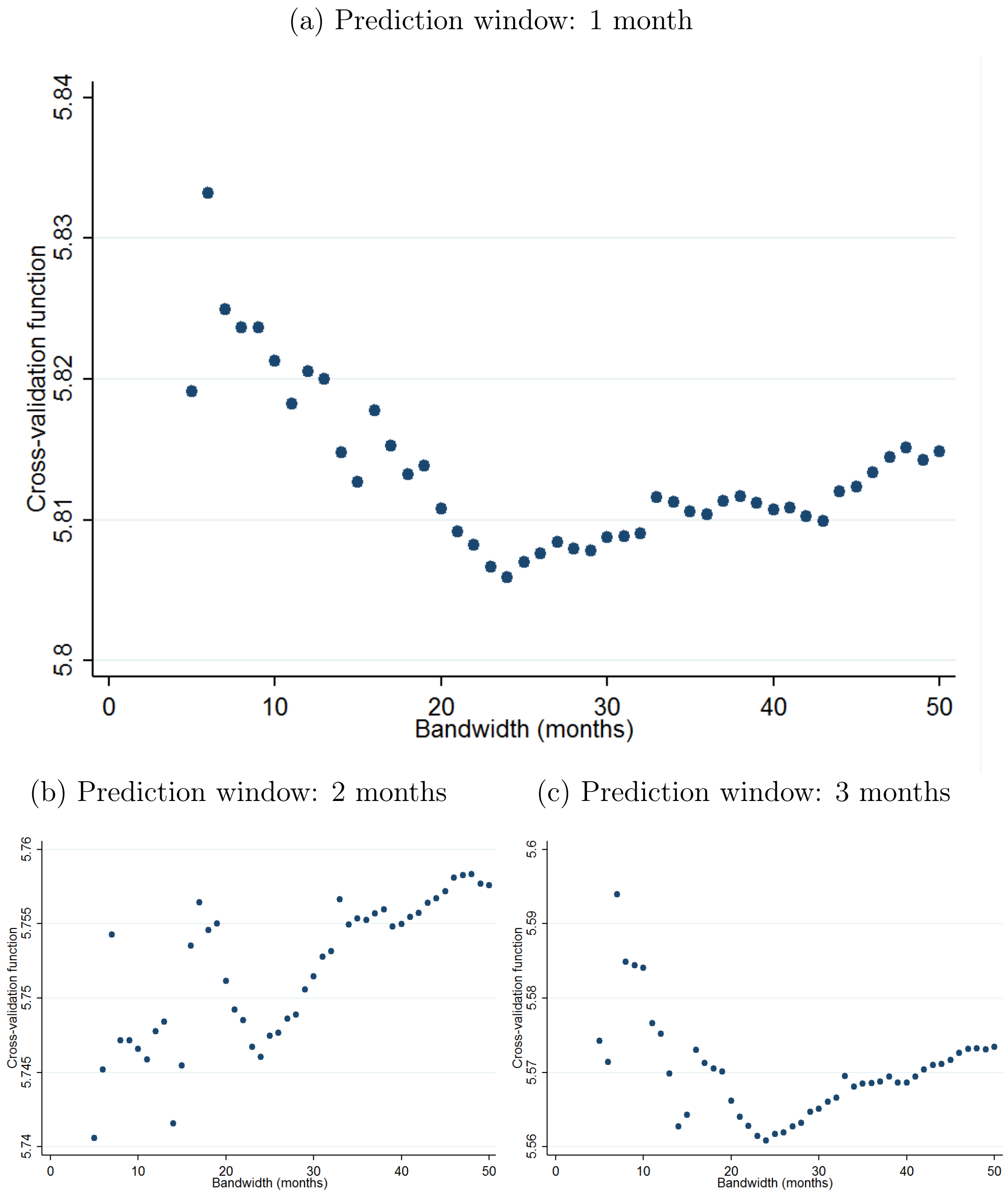
Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Note: Table shows estimates of the effect of DNA profiling on subsequent crime including the months that are excluded in the main analysis. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Observations: 72,338. Standard errors are clustered by personal id number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Table A-13: Effects of DNA profiling on subsequent accumulated probability of conviction and number of convictions by subsequent crime type

	P(convicted)			**# convictions**		
	All	Fast	Slow	All	Fast	Slow
	(1)	(2)	(3)	(4)	(5)	(6)
A: Property						
1 year	-0.031^{+}	-0.032*	-0.006	-0.050*	-0.043*	-0.007
	(0.016)	(0.015)	(0.009)	(0.023)	(0.021)	(0.009)
2 years	-0.048*	-0.047*	-0.018	-0.087*	-0.059^{+}	-0.028^{+}
	(0.021)	(0.020)	(0.014)	(0.038)	(0.033)	(0.016)
3 years	-0.037	-0.043*	-0.010	-0.062	-0.051	-0.011
	(0.022)	(0.021)	(0.016)	(0.049)	(0.040)	(0.021)
B: Violence						
1 year	-0.031*	-0.025*	-0.006	-0.035*	-0.029*	-0.006
	(0.012)	(0.012)	(0.005)	(0.014)	(0.013)	(0.005)
2 years	-0.034*	-0.030^{+}	-0.009	-0.041^{+}	-0.032	-0.009
	(0.017)	(0.016)	(0.008)	(0.022)	(0.020)	(0.008)
3 years	-0.026	-0.029	-0.006	-0.027	-0.033	0.006
	(0.020)	(0.018)	(0.011)	(0.031)	(0.024)	(0.011)
C: Sexual						
1 year	-0.000	-0.000	0.000	-0.000	-0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
2 years	0.001	0.000	0.000	0.001	0.000	0.000
	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
3 years	0.004	0.003	0.000	0.002	0.003	-0.000
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
D: Other penal						
1 year	0.001	0.005	-0.003	0.004	0.007	-0.003
	(0.006)	(0.005)	(0.002)	(0.006)	(0.005)	(0.002)
2 years	-0.013	-0.012	-0.002	-0.014	-0.014	-0.001
	(0.010)	(0.009)	(0.005)	(0.011)	(0.010)	(0.006)
3 years	0.004	-0.010	0.004	-0.008	-0.014	0.006
	(0.013)	(0.011)	(0.007)	(0.014)	(0.012)	(0.008)
E: Weapon						
1 year	-0.010^{+}	-0.009	-0.001	-0.011^{+}	-0.010	-0.001
	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)
2 years	-0.021*	-0.019*	-0.002	-0.021*	-0.019*	-0.002
	(0.009)	(0.009)	(0.002)	(0.009)	(0.009)	(0.002)
3 years	-0.031**	-0.026*	-0.004	-0.034**	-0.031*	-0.003
	(0.011)	(0.011)	(0.010)	(0.002)	(0.011)	(0.002)
Observations	66911	66911	66911	66911	66911	66911
Pre-reform baseline, 1 year						
Property	0.091	0.111	0.076	0.091	0.019	0.020
Violence	0.049	0.053	0.042	0.044	0.008	0.008
Sexual	0.002	0.002	0.001	0.001	0.000	0.000
Other penal	0.012	0.012	0.010	0.010	0.002	0.002
Weapon	0.011	0.011	0.011	0.011	0.000	0.000

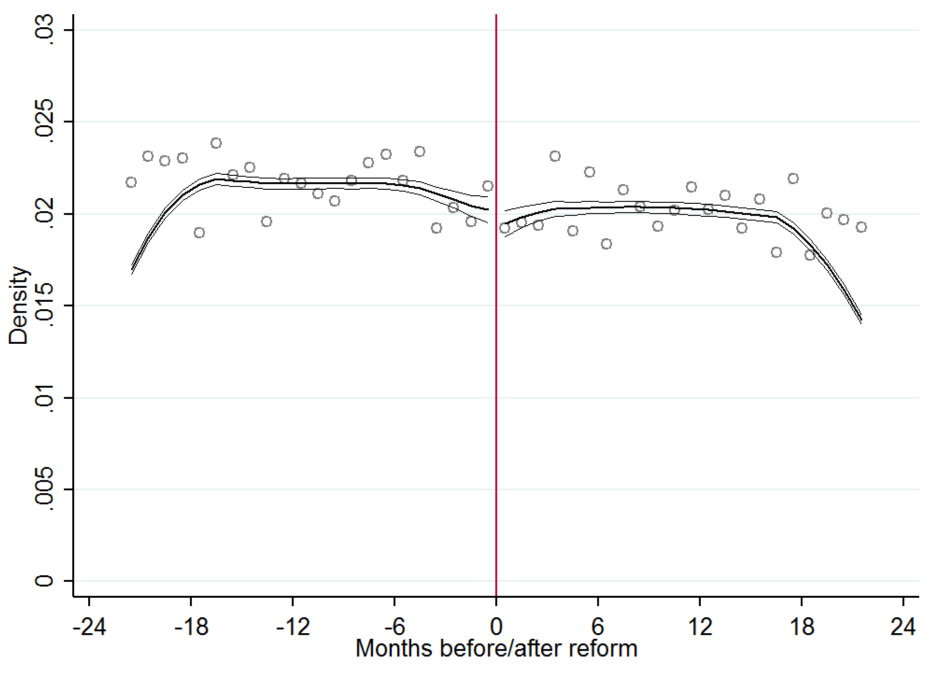
Standard errors in parentheses + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ Note: Table shows estimates of the effect of DNA profiling by type of subsequent crime and distance between date of crime and charge. Covariates include age, immigrant background, has children, single, years of education, gross income, employment status, number of prior charges, crime type dummies and month fixed effects. Standard errors are clustered by personal identification number. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Figure A-1: Cross-validation function by bandwidth



Note: The figures shows the cross-validation (CV) function plotted against different bandwidths. The CV function is calculated in two steps (as described in Lee and Lemieux (2010) and Ludwig and Miller (2005)). First, we estimated the reduced form estimates with a dummy variable indicating before/after reform and running variables measuring months before or after the reform (+ covariates), but leaving out observations in the 1-3 month preceding and following the reform. Second, we used the estimates to predict the outcome for the observations in the excluded window around the reform, and calculate the mean prediction error for each outcome. The prediction errors (CV functions) were then aggregated across the outcomes. This was done for bandwidths ranging from 5 to 50 months before/after the reform. Source: Own calculations based on Data from Statistics Denmark and the National Police.

Figure A-2: McCrary density test



Note: Figure shows density before and after reform in bins of one month. A McCrary test for discontinuity in density (with default bandwidth) gives a theta of -0.041 with standard error of 0.030 and a t-value of -1.339. Source: Own calculations based on Data from Statistics Denmark and the National Police.