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CRIME PREVENTION EFFECTS OF DATA RETENTION POLICIES

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Crime Prevention Effects of Data Retention Policies

Abstract: Adding to the extensive political and legal debates on data retention, this is the first study to analyse the impacts of data retention on crime prevention in Europe. Using an estimator that captures dynamic effects and is robust to heterogeneous treatment effects, we find a significant negative effect on aggregate crime rates. However, our findings indicate that a minimum of one year is required following the implementation of the obligations for a decline in crime rates to occur, indicating a gradual adjustment in delinquent behaviour. Moreover, distinct effects occur not only after the introduction of data retention laws but also when these laws are cancelled. In addition, we present evidence that the effects on aggregate crime rates are likely to be driven by changes in property crime rates and fraud, while violent crime rates remain unaffected.

Keywords: Crime, Anticrime politics, Data retention, Deterrence, Difference-in-Differences
Jel: K42, K14, C21, C23, H76
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1. Introduction

Economic analyses on the efficient design and implementation of anticrime politics rely heavily on Becker (1968), who models the choice to commit most crimes as a rational decision. While potential offenders consider both the detection probability and possible consequences that may follow from apprehension as relevant factors, legal institutions face a tradeoff between the costs of implementing anticrime measures and the expected benefits that arise in cases where criminal actions are successfully deterred. Hence, the identification of key variables that influence the rationale of potential offenders and the question of how to efficiently design policy measures that will alter these variables has become a relevant field in the economic literature.

Potential caveats may result from endogeneity when analysing the efficiency of anticrime policies targeting detection rates, police presence, or conviction rates (e.g., Levitt 1996). For example, rising crime rates may also result in higher detection rates when the responsible authorities provide crime-intensive regions with more resources. As a result, empirical estimates may lead to biased results if they fail to account for these endogeneity concerns. To overcome this problem, many scholars have exploited natural experiments or exogenous variations when analysing anticrime politics. Di Tella and Schargrodsky (2004) and Klick and Tabarrok (2005) use exogenous changes to police presence to examine the effect of policing on crime. Both studies find strong evidence that an increased police presence deters criminal behaviour. Blesse and Diegmann (2022) and Bindler and Hjalmarsson (2021) report similar effects by exploiting the formation or closing of police stations. Other studies have taken advantage of juridical changes to estimate the effect of institutional decisions on crime rates. In this context, Drago, Galbiati, and Vertova (2009) show that increasing the expected sanctions substantially reduces recidivism.

In this study, we determine the impact of the controversial Directive 2006/24/EC on Communication Data Retention using a quasi-experimental approach. In 2006, the European Parliament obliged member states to establish a data retention scheme for investigative purposes, which has met with resistance in several member states due to related privacy concerns. Proponents of the directive have emphasized the potential benefits of data retention in preventing crime and identifying potential offenders. However, to the best of our knowledge, no empirical analysis has evaluated the potential effects of data retention schemes on crime rates. Our paper is the first to isolate the effects of introducing a data retention scheme on aggregate crime rates, as well as violent and property crime rates. Due to the large discrepancies among member states in the time until the law has been implemented, we are able to estimate the effect of data retention schemes using a difference-in-differences approach. Likewise, we exploit the cancellation of the law by numerous national courts to compare regions that use preserved data and regions that abstain from such a practice. Our findings suggest that data retention schemes reduce aggregate and property crime rates significantly, but do so only after the policy has been established for at least one year.

Since we focus on the effect of national policies on aggregate crime rates, our study is most closely related to Doleac (2017), who estimates the effect of DNA databases on aggregate crime rates in the US. The results imply that growth in the DNA database from 2000 to 2010 led to a decline in violent crime by 7-45 percent and a decline in property crime by 5-35. Furthermore, our paper is related to studies that evaluate the effect of technical innovations in crime prevention. Several studies have examined the impact of public surveillance cameras on local crime rates, with some recent papers finding that public camera surveillance may be effective in reducing crime rates (Gómez, Mejía, and Tobón 2021; Munyo and Rossi 2020). Similarly, Di Tella and Schargrodsky (2013) show that offenders who are monitored electronically via GPS exhibit significantly lower recidivism rates than those who experience imprisonment. In contrast, other studies have shown that the social stigma created by the registration of sex offenders may undermine the effect of such policies (Agan 2011; Prescott and Rockoff 2011).

Finally, our paper connects to the extensive literature exploring the socioeconomic determinants of criminal activity. Several studies have reported a significant effect of income and unemployment rates on crime rates (e.g., Dezhbakhsh and Shepherd 2006; Levitt 1996; Raphael and Winter-Ebmer 2001). While unemployment tends to raise crime rates, evidence on the effect of income is less conclusive. More prosperous areas may experience more crime—at least in the case of burglary, robbery, and theft—as they seem more attractive to potential offenders. On the other hand, improved economic conditions may decrease incentives to commit a crime due to more worthwhile outside options. Other studies have found a negative relation between education and criminal activities (Deming 2011; Machin, Marie, and Vujić 2011) and increased crime rates in areas that exhibit high population densities (Bianchi, Buonanno, and Pinotti 2012; Dezhbakhsh, Rubin, and Shepherd 2003). Likewise, crime rates may depend on the age structure of societies (Sweeten, Piquero, and Steinberg 2013), waves of immigration

(Alonso-Borrego, Garoupa, and Vázquez 2012; Bianchi et al. 2012), capital punishments (Shepherd 2004), or abortion laws (Donohue and Levitt 2001).

The remainder of this paper is structured as follows. Section 2 discusses the institutional background and presents our data, while in Section 3, we describe our identification strategy. Section 4 displays our empirical results and robustness tests, and Section 5 concludes the paper.

2. Institutional Background and Data Description

2.1. Directive 2006/24/EC on the Retention of Communication Data

In March 2006, the European Parliament and the Council of the European Union presented a directive that obliged member states to adapt national legislation concerning the retention of communication data by public providers. Each member state should ensure that the data stored by telecommunication providers are available for investigational purposes. The directive aims at retaining traffic and location data that are necessary to identify the source of a communication. Providers should store the data for at least 6 months but not more than two years.

Thus, member states had to adjust their national legislation to implement the obligation into national law. The process of implementation varied substantially between member states, as Table 1 demonstrates. While in Italy, a data retention scheme had already been in place before the data retention directive, other member states, such as Austria or Belgium, delayed the implementation process considerably due to concerns about privacy and data protection.

Not long after being implemented, several national courts declared the legislation concerning the retention of communication data as unconstitutional and cancelled the corresponding laws. For example, in Germany, the data retention scheme was cancelled in 2010 by the Bundesgerichtshof (German National Court) and has never been

Table 1. Implementation process of data retention schemes in Europe

Country	Implementation Process of Data Retention Schemes
Austria	A data retention scheme was introduced in 2012 but was cancelled again in 2014. Since 2018, data retention has only been made available in cases of concrete suspicions.
Belgium	The Belgian government implemented a data retention scheme in 2013. The obligation was cancelled in 2015 but was reintroduced shortly after.
Czech Republic	The data retention obligation has been active since 2005 even though it was declared unconstitutional in 2011.
Denmark	In Denmark, the obligation has been in place since 2007.
Finland	Finland implemented the provisions of the European Union in 2008, and data retention obligations are still in force.
Germany	The German government introduced a data retention scheme in 2008. However, the obligation was cancelled by the national court in 2010; since then, no data have been retained.
Italy	In Italy, communication data has been retained since 2003, even before the ruling of the European Court of Justice.
Netherlands	The data retention obligation was introduced in 2009 but was cancelled again in 2015. Since then, communication data has not been retained.
Poland	Data retention obligations came into force in 2009 and are still active.
Portugal	The data retention obligations implemented in 2009 were cancelled in 2022, which is outside of our analysed time period.
Slovakia	The judgement of the European Court of Justice was transposed into national law in 2011. In 2015, the law was officially suspended, but the retention of data had already ended in 2014.
Spain	A data retention scheme has been in place since 2007.

Data sources: Vainio and Miettinen (2015); National codes of law

reintroduced. As a result, the European Court of Justice declared the data retention directive as invalid in 2014, leading to further annulments of national laws. However, many states have maintained their current retention scheme despite the ruling of the European Court of Justice.

The country-specific institutional challenges in the implementation process and the heterogeneous responses to the ruling of the European Court of Justice have led to a scenario where at any point in time at least one country does not have an active data retention scheme, while other countries use communication data for investigative purposes. It seems reasonable to assume that these differences are caused by political attitudes towards data protection and privacy rather than differing crime rates such that we can treat the implementation or cancellation of a data retention scheme as an exogenous event.

2.2. Data

We construct a dataset that contains 147 regions in 12 European countries by combining several national data sources. Data coverage varies between countries (Table 2), resulting in an unbalanced panel dataset with observations from 2000 to 2019. We extract the data on crime rates, offences, and clearance rates from local statistics offices, police reports, and the responsible ministries of the interior; details are provided in Table A1 in the appendix.

In line with the previous literature, we define the aggregate crime rate (crime) and the crime rates of fraud (fraud), violent crime (violent), and property crimes (property) as the number of offences per thousand inhabitants. Potential concerns regarding country-specific definitions of offences and computational differences are eliminated by using regional fixed effects in the estimations. The clearance rate (clear) reports the number of solved offences as a share of aggregate offences. Belgium, Denmark, Portugal, and Slovakia do not provide any data on clearance rates, which reduces our sample size whenever we use the clearance rate in our analysis.

We include the yearly regional GDP per capita (gdp) as a measure of economic performance. Using inflation data obtained from the World Bank, we adjust the nominal values of income to obtain the real GDP. Moreover, we use the unemployment rate (unemp) as an indicator of alternative income opportunities in contrast to income from

Table 2. Summary statistics.

Country	Period	*crime*	*property*	*violent*	*fraud*	*clear*	*gdp*	*unemp*	*dens*	*age*	*educ*
Austria	2005-2019	58.49	37.62	10.03	-	48.29	40695	4.64	545	6.44	17.28
	n = 135	*(23.20)*	*(19.22)*	*(2.64)*		*(7.61)*	*(7381)*	*(1.91)*	*(1306)*	*(0.73)*	*(3.35)*
Belgium	2000-2019	88.30	34.69	7.17	-	-	37177	7.89	927	6.36	29.43
	n = 242	*(25.61)*	*(17.73)*	*(1.83)*			*(1384)*	*(3.93)*	*(1855)*	*(0.77)*	*(8.01)*
Czech Republic	2005-2019	25.01	14.71	1.62	-	49.38	15860	5.58	295	6.88	16.49
	n = 224	*(12.06)*	*(10.03)*	*(0.55)*		*(11.36)*	*(6579)*	*(2.76)*	*(622)*	*(0.72)*	*(3.72)*
Denmark	2008-2019	81.91	57.48	3.49	2.86	-	46837	4.72	649	5.58	-
	n = 154	*(28.53)*	*(26.36)*	*(0.89)*	*(2.19)*		*(13985)*	*(1.34)*	*(1235)*	*(2.37)*	
Finland	2000-2019	147.15	43.24	6.63	3.90	75.68	40222	9.90	47	6.11	17.30
	n = 110	*(23.39)*	*(12.68)*	*(0.95)*	*(1.74)*	*(7.07)*	*(8198)*	*(4.27)*	*(58)*	*(0.71)*	*(5.80)*
Germany	2000-2019	85.65	35.22	2.85	12.46	55.13	34913	8.29	695	6.12	13.58
	n = 320	*(30.01)*	*(16.84)*	*(1.24)*	*(4.87)*	*(7.12)*	*(10177)*	*(4.77)*	*(1054)*	*(0.87)*	*(5.42)*
Italy	2004-2019	39.55	20.07	2.75	2.20	21.34	29438	9.55	174	5.77	43.05
	n = 336	*(10.02)*	*(7.53)*	*(0.41)*	*(0.69)*	*(4.56)*	*(7997)*	*(5.07)*	*(110)*	*(0.75)*	*(7.64)*
Netherlands	2005-2019	60.73	35.15	0.38	1.71	27.43	39801	5.28	415	5.95	26.03
	n = 180	*(15.60)*	*(10.31)*	*(0.17)*	*(0.65)*	*(3.88)*	*(8446)*	*(1.41)*	*(323)*	*(0.70)*	*(4.08)*
Poland	2002-2019	27.86	14.98	0.72	-	67.18	8857	10.93	188	7.82	12.83
	n = 288	*(8.38)*	*(6.03)*	*(0.22)*		*(7.77)*	*(2039)*	*(5.71)*	*(243)*	*(0.63)*	*(4.22)*
Portugal	2006-2019	36.79	19.27	9.68	-	-	18214	10.64	248	6.19	63.70
	n = 98	*(10.17)*	*(8.05)*	*(2.43)*			*(3470)*	*(3.78)*	*(296)*	*(0.72)*	*(10.93)*
Slovakia	2001-2019	17.30	7.41	1.36	-	-	13330	11.14	132	7.96	-
	n = 160	*(7.10)*	*(5.10)*	*(0.47)*			*(7852)*	*(6.06)*	*(68)*	*(0.72)*	
Spain	2002-2019	41.29	15.18	2.45	3.15	38.91	24887	15.27	211	6.94	47.12
	n = 306	*(14.97)*	*(8.56)*	*(0.89)*	*(1.91)*	*(11.14)*	*(5013)*	*(7.06)*	*(200)*	*(1.42)*	*(9.34)*
Total	n = 2553	55.20	26.97	3.57	4.91	45.85	28082	8.98	371	6.54	28.25
		(35.71)	*(18.92)*	*(3.20)*	*(5.00)*	*(18.70)*	*(14280)*	*(5.66)*	*(854)*	*(1.26)*	*(16.49)*

Notes: Descriptive Statistics of included variables. The table shows mean values with standard errors in parentheses.

illicit activities. The unemployment rate is calculated based on the number of officially registered unemployed rather than obtained from labour force surveys.

In addition, we include a control for the density of regions (dens) by dividing the total population by the regions' area in square kilometres and a control for the age structure (age) by relating the number of inhabitants between 25 and 29 years to the total population size. We calculate these indicators using census data obtained from local statistics offices. Moreover, we measure educational attainment (educ) by the share of the population between 25 and 64 years old that has an education level less than lower secondary school.

Finally, we generate two treatment variables to determine the treatment and control groups. The first variable indicates whether a region makes use of a data retention scheme in the corresponding year. Likewise, the second indicator shows whether a region that previously implemented a data retention obligation has prohibited the use of such schemes.

3. Empirical Strategy

Our identification strategy exploits the country-specific differences in the implementation of data retention obligations and is closely related to the approach used by Blesse and Diegmann (2022). Due to delayed implementations and the cancellation of the law in some countries, we are able to estimate the effect of data retention schemes by a difference-in-difference estimator. We use regions that have not yet implemented data retention schemes as the control group and regions that have implemented data retention obligations as the treatment group. As a robustness check, we use an alternative specification, in which we define the cancellation of data retention schemes as the treatment and compare treated regions to regions that still use retained data for investigative purposes.

Similar to Blesse and Diegmann (2022), we estimate the effects of data retention schemes using an event-study model that accounts for linear time trends and regional and time fixed effects. This approach allows us to capture dynamic effects (i.e., effects may differ by year), preventing potential misspecifications noted by Feddersen and Maennig (2012). Since our assignment into the treatment and control groups is not driven by differences in crime rates, we do not need to use matching approaches; instead, we account for demographic and economic variations across regions using a vector of control variables. As a result, we obtain the following baseline model:

$$log\left(crime_{i,s,t}\right) = \sum_{\tau=-4}^{3} \beta_{\tau} D_{\tau(s,t)} + A_i + B_t + \gamma \boldsymbol{X}_{i,s,t} + \sigma_i t + \epsilon_{i,s,t} \qquad (1)$$

In this baseline model, we consider the logarithmic crime rates in a specific region i in country s at time t as the outcome variable of interest. B_t and A_i denote time and regional fixed effects, respectively, whereas $\boldsymbol{X}_{i,s,t}$ represents a vector of controls. We control for real GDP (in logs), the unemployment rate, the age distribution, the regions' density (in logs) and percentage of the population with less than a lower secondary education, as these variables have been identified as relevant forces in explaining the development of criminal activities (see, for example, Chalfin and McCrary 2017; Sweeten, Piquero, and Steinberg 2013). The treatment dummy $D_{\tau(s,t)}$ indicates whether a country has an active data retention scheme. Therefore, β_τ for $\tau \geq 0$ measures the (dynamic) impact of data retention schemes, while β_τ for $\tau < 0$ is used to evaluate the existence of common trends pre-treatment by introducing a placebo treatment prior to the introduction. In the case of common pre-trends, we expect the estimates of β_τ for $\tau < 0$ to be insignificant. Last, $\epsilon_{i,s,t}$ describes the error term. In an alternative specification, we exclude the linear time trends, as seen in Ciacci and Sansone (2023).

Recently, De Chaisemartin and D'Haultfœuille (2020) pointed out that in the case of differences in treatment timing or heterogeneous treatment effects, the estimated coefficients $\widehat{\beta_\tau}$ and the true coefficients β_τ may be of opposite sign due to negative weights being assigned to individual treatment effects. Heterogeneous effects may occur in treated regions, as the effectiveness of anticrime measures depends critically on their acceptance by law enforcement officials (Dharmapala, Garoupa, and McAdams 2016), who may have different attitudes towards the use of retained data. In contrast to Blesse and Diegmann (2022), we find that nearly 50 percent of our weights are negative, implying that little heterogeneity is needed so that our estimate $\hat{\beta}$ and the true treatment effect are of opposite signs.

Therefore, we employ the estimator developed by De Chaisemartin and D'Haultfoeuille (2022) that circumvents this caveat by comparing treated and yet-not treated regions. Let F_s indicate the first period at which country s experiences a change in its treatment. Furthermore, $N_{s,t}$ denotes the number of regions (number of observations) within a country at period t, and $D_{s,t}$ denotes a country's treatment status in period t. Finally, we define $N_t^U = \sum_{s:D_{s,1}=0,F_s>t} N_{s,t}$ as the sum of untreated regions from period 1 to t and N_l^1 as the sum of regions being treated for l periods. Then, for every treatment duration l, we can estimate the treatment effect $\hat{\delta}_{l,s}$ in group s by comparing treated regions and not-yet treated regions as follows:

$$\hat{\delta}_{l,s} = log(crime)_{s,F_s+l} - log(crime)_{s,F_s-1} - \left(\boldsymbol{X}_{s,F_s+l} - \boldsymbol{X}_{s,F_s-1}\right)'\hat{\theta}$$
$$- \sum_{s':D_{s',1}=0,F_{s'}>F_s+l} \frac{N_{s',F_s+l}}{N^U_{F_s+l}} \Big(log(crime)_{s',F_s+l} - log(crime)_{s',F_s-1} \qquad (2)$$
$$- \left(\boldsymbol{X}_{s',F_s+l} - \boldsymbol{X}_{s',F_s-1}\right)'\hat{\theta}\Big)$$

Here, $\boldsymbol{X}_{s,t}$ is a vector containing covariates and group-specific linear trends, while $\hat{\theta}$ captures the effect of time differences in $\boldsymbol{X}_{s,t}$ on time differences in crime rates. The average effect of all groups being treated for l periods is then the weighted average of the individual effects. As a potential drawback of this estimation method, we are unable to explicitly determine the impact of covariates or provide any measure on the fit of our model. Therefore, we also estimate Equation (1) using the common two-way fixed effects estimator.

4. Empirical Results

4.1 The Effect of Introducing Data Retention Schemes

The empirical strategy adapted from Blesse and Diegmann (2022) allows us to identify different effects for each year after the introduction of data retention regulations, thus taking into account potential time-lagged effects. In addition, this approach implements placebo tests before the treatment that indicate whether the control and treatment groups exhibit divergent trends prior to the treatment.

We estimate the effects of implementing a data retention scheme up to three years after the introduction while testing the common trend assumption from four periods before the implementation. We examine the effects of data retention schemes on aggregate, property, and violent crime. Note that there is no uniform definition of crime categories

Table 3. Two-way fixed effects (TWFE) estimation on the effect of introducing data retention schemes.

	Dependent Variable: Log Crime Rates		
	Aggregate Crime	**Property Crime**	**Violent Crime**
	(I) TWFE	(II) TWFE	(III) TWFE
Average Treatment Effects (ATE)			
ATE $_{(t = 0)}$	0.0083 *(0.0117)*	0.0304 *(0.0220)*	0.0200 *(0.0195)*
ATE $_{(t = 1)}$	0.0171* *(0.0088)*	0.0461*** *(0.0158)*	0.0161 *(0.0177)*
ATE $_{(t = 2)}$	0.0139** *(0.0613)*	0.0109 *(0.0108)*	0.0205* *(0.0107)*
ATE $_{(t = 3)}$	0.0088 *(0.0070)*	0.0079 *(0.0108)*	0.0335*** *(0.0080)*
Placebo Estimates (PL)			
PL $_{(t = -1)}$	0.0029 *(0.0080)*	0.0085 *(0.0132)*	-0.0046 *(0.0140)*
PL $_{(t = -2)}$	-0.0107 *(0.0092)*	0.0006 *(0.0146)*	0.0081 *(0.0140)*
PL $_{(t = -3)}$	-0.0105* *(0.0062)*	0.0089 *(0.0118)*	0.0036 *(0.0109)*
Controls			
GDP (Logs)	-0.2110*** *(0.0796)*	-0.3808*** *(0.1129)*	0.1382 *(0.1094)*
Unemployment	-0.0012 *(0.0021)*	0.0041 *(0.0047)*	-0.0046 *(0.0037)*
Share 20-25	0.0260* *(0.0145)*	0.0202 *(0.0214)*	0.0457* *(0.0248)*
Density (Logs)	-0.8012* *(0.0457)*	-0.3700 *(0.8458)*	1.442* *(0.7905)*
Education	0.0004 *(0.0021)*	0.0044 *(0.0032)*	-0.0031 *(0.0030)*
Fixed Effects	YES	YES	YES
Linear Time Trend	YES	YES	YES
R^2	0.87	0.89	0.87
Observations	1329	1101	1100

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively.*

across Europe or beyond; thus, any international crime study may suffer if crime definitions vary between the treatment and control groups. We choose to analyse property crimes and violent crimes to test whether the effect of data retention schemes varies substantially between certain types of crime, as we expect the least inconsistencies and variations between countries to occur in these categories. In addition, we can partly account for potential region-specific differences by including regional fixed effects. However, the precision of our estimates may be affected due to potential inconsistencies.

In the first step, we estimate our model using the common two-way fixed effects (TWFE) estimator. Although the results may be biased, as discussed in more detail in Section 4, these estimates may still provide information about the validity of our data, the explanatory power of our model, and the magnitude and direction of the potential bias. We present the results in Table 3.

In line with Detotto and Pulina (2013), we observe a significant negative effect of real GDP on aggregate and property crime, while the share of youth has a positive effect, at least for aggregate and violent crime. Interestingly, more dense areas exhibit higher rates of violent crime per capita but less aggregate crime per capita.

Our placebo estimates $PL_{(t=\tau)}$ indicate no violation of the common trend assumption in our specifications. Surprisingly, the average treatment effects $ATE_{(t=\tau)}$ that we obtain for the two-way fixed effects approach suggest a significant positive impact of data retention schemes on all three crime categories. The results further alleviate concerns regarding potential biases from heterogeneous treatment effects on the estimated effects, given the counterintuitive nature of a rise in crime rates resulting from data retention schemes. Therefore, we estimate the same model using the estimator by De Chaisemartin and D'Haultfoeuille (2022) to obtain more robust results. Table 4 displays the estimated coefficients.

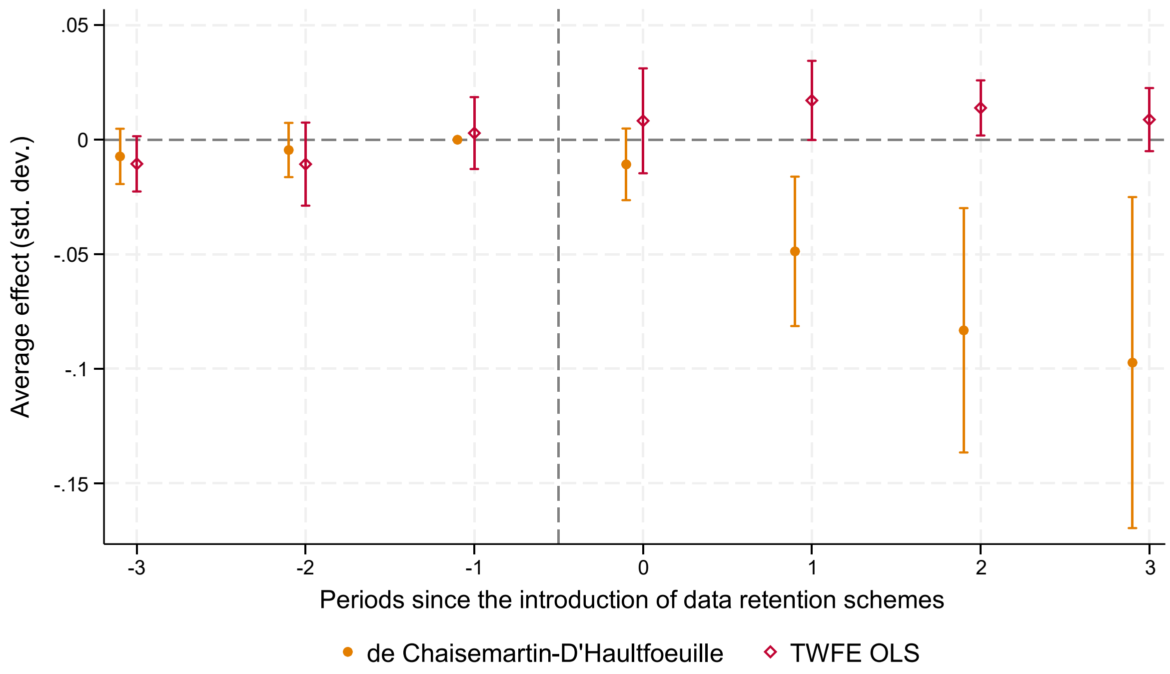
Table 4. Estimation on the effect of introducing data retention schemes using the robust estimator of De Chaisemartin and D'Haultfoeuille (2022) (DCDH).

	Dependent Variable: Log Crime Rates					
	Aggregate Crime		**Property Crime**		**Violent Crime**	
	(I) DCDH		(II) DCDH		(III) DCDH	
Average Treatment Effects (ATE)		*N*		*N*		*N*
ATE $_{(t = 0)}$	-0.0108 *(0.0073)*	*267*	-0.0089 (0.1360)	*248*	0.0302** *(0.0132)*	*260*
ATE $_{(t = 1)}$	-0.0487** *(0.0173)*	*192*	-0.0361 (0.0233)	*178*	0.0328 *(0.0263)*	*187*
ATE $_{(t = 2)}$	-0.0832*** *(0.0264)*	*149*	-0.0816** (0.0352)	*138*	0.0383 *(0.0348)*	*144*
ATE $_{(t = 3)}$	-0.0973*** *(0.0355)*	*116*	-0.0725 (0.0480)	*105*	0.0672 *(0.0561)*	*111*
Placebo Estimates (PL)						
PL $_{(t = -2)}$	-0.0045 *(0.0053)*	*260*	-0.0091 (0.0075)	*241*	0.0158* *(0.0081)*	*253*
PL $_{(t = -3)}$	-0.0073 *(0.0055)*	*158*	-0.0124 (0.0100)	*144*	0.0284*** *(0.0093)*	*153*
PL $_{(t = -4)}$	-0.0060 *(0.0127)*	*121*	0.0054 (0.0077)	*110*	-0.0031 *(0.0079)*	*115*
Controls						
Fixed Effects	YES		YES		YES	
Linear Time Trend	YES		YES		YES	
Demographic and Economic Determinants	YES		YES		YES	

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively. N is the number of observations that are used to calculate the treatment effect.*

In the case of violent crime, the treatment and control groups seem to deviate before the introduction of data retention schemes; thus, we do not interpret the estimates of Column III in Table 4. In the case of aggregate and property crime, our placebo estimates provide evidence that the common trend assumption holds before the data retention schemes become active in the treatment group. The coefficients of our placebo estimates are insignificant and close to zero, which mitigates concerns that the estimates are only insignificant due to large standard errors.

Figure 1. The effect of data retention schemes on aggregate crime rates.



Notes: The x-axis indicates the time period (in years) before and after a data retention scheme has been implemented in the treatment groups (implementation at t = 0). The markers display the corresponding differences in aggregate crime rates (in logs) between regions that introduced a data retention scheme at t = 0 and the control group. Estimates before the implementation show placebo estimates to test the common trend assumption. The estimates on the right of t = 0 present the effect of being treated for up to three years. We estimate standard errors using 100 bootstrap replications clustered at the regional level and report 95 percent confidence intervals.

The estimates reveal a lagged but significant reduction in aggregate crime rates if a country implements a data retention scheme (Column I of Table 4). In the year of introduction, crime rates in the treatment and control groups are not significantly different. However, the estimates display a gradual increase in the effect of data retention schemes over time. The effect appears to increase continuously up to an 8.3-percent decrease in crime rates after two years of treatment and a reduction in crime rates of 9.7 percent after maintaining an active data retention scheme for three years. We do not report any estimates for the effect of being treated for more than three years because the estimated effects would apply to no more than 25 percent of our sample.

The coefficients vary substantially from the results of the two-way fixed effects approach, which highlights the importance of robust estimators in the case of differences in treatment timing or potentially heterogeneous treatment effects. Figure 1 highlights the differences between both estimator groups.

Similarly, we observe a significant reduction in property crime rates if regions have implemented a data retention scheme for at least two years (Column II of Table 4). The effect is similar in magnitude to the previous estimate of overall crime rates. The results do not show any significant effects in other time periods.

In an alternative specification, we estimate the same model without linear time trends. The inclusion of linear trends changes the common trend assumption; thus, a valid comparison of treatment and control groups only requires common deviations from linear trends (de Chaisemartin and D'Haultfoeuille 2022). As Table A2 in the appendix shows, the effects do not differ qualitatively from the estimates in Table 4 if we do not account for time trends. In addition, there is no sign that the common trend assumption may be violated, even in the absence of linear trends. Likewise, controlling for local clearance rates does not significantly change the results but rather leads to less precise estimates due to a smaller sample size (see Table A3 in the appendix).

4.2 Robustness Checks

Our results indicate a significant decrease in aggregate and property crime rates after the introduction of data retention schemes. However, as our identification strategy only allows us to compare treated and not-yet treated regions, we can only estimate the effect of introducing an obligation to retain communication data for the time period until the last region in our sample receives the treatment. Another way to approach the analysis of data retention schemes is to compare crime rates in regions that have cancelled data retention schemes to regions that have maintained these obligations even after the European Court of Justice ruled against such schemes. Hence, we define

Table 5. Estimation on the effect of cancelling data retention schemes using the robust estimator of De Chaisemartin and D'Haultfoeuille (2022) (DCDH).

	Dependent Variable: Log Crime Rates					
	Aggregate Crime		**Property Crime**		**Violent Crime**	
	(I) DCDH		(II) DCDH		(III) DCDH	
Average Treatment Effects (ATE)		*N*		*N*		*N*
ATE $_{(t = 0)}$	0.0200*** *(0.0063)*	*342*	0.0409*** (0.0092)	*291*	-0.0010 *(0.0132)*	*303*
ATE $_{(t = 1)}$	0.0407*** *(0.0110)*	*330*	0.0826*** (0.0170)	*279*	0.0044 *(0.0263)*	*291*
ATE $_{(t = 2)}$	0.0433** *(0.0187)*	*330*	0.0885*** (0.0248)	*279*	0.0254 *(0.0348)*	*291*
ATE $_{(t = 3)}$	0.0384* *(0.0203)*	*330*	0.0964*** (0.0294)	*268*	0.0366 *(0.0561)*	*277*
Placebo Estimates (PL)						
PL $_{(t = -2)}$	-0.0029 *(0.0053)*	*341*	0.0100 (0.0097)	*290*	-0.0070 *(0.0081)*	*302*
PL $_{(t = -3)}$	-0.0101 *(0.0080)*	*328*	-0.0117 (0.0121)	*277*	0.0391* *(0.0201)*	*289*
PL $_{(t = -4)}$	-0.0055 *(0.0106)*	*327*	0.0012 (0.0153)	*276*	0.0661*** *(0.0193)*	*288*
Controls						
Fixed Effects	YES		YES		YES	
Linear Time Trend	YES		YES		YES	
Demographic and Economic Determinants	YES		YES		YES	

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively. N is the number of observations that are used to calculate the treatment effect.*

the regions that have cancelled their data retention laws as the treatment group and define regions that have maintained their regulations regarding the retention of communication data as the control group. We estimate the potential effect of the cancellation of data retention laws using the same model and the same estimator that we introduced in Section 4.

Table 5 shows that in the case of aggregate crime, the placebo estimates are again insignificant, indicating that the control and treatment groups allow a valid comparison. Column I of Table 5 shows that aggregate crime rates increase by 2 percent in the year that the data retention scheme is cancelled. In the following year, we estimate a difference between the treatment and control groups of 4.1 percent, suggesting a further increase in aggregate crime due to the cancelled data retention schemes. Over the next years, the effect remains rather stable at a difference of approximately 4 percent. Applying a z-test, as suggested by Paternoster et al. (1998), we cannot reject the null hypothesis that the effect of cancelling data retention policies is equal to the previously estimated effect of introducing such policies. Figure 2 highlights the distinct effect of cancelling data retention laws on aggregate crime. For completeness, the figure also shows the coefficients of the two-way fixed effects, which again demonstrate a large bias due to heterogeneous treatment effects[1].

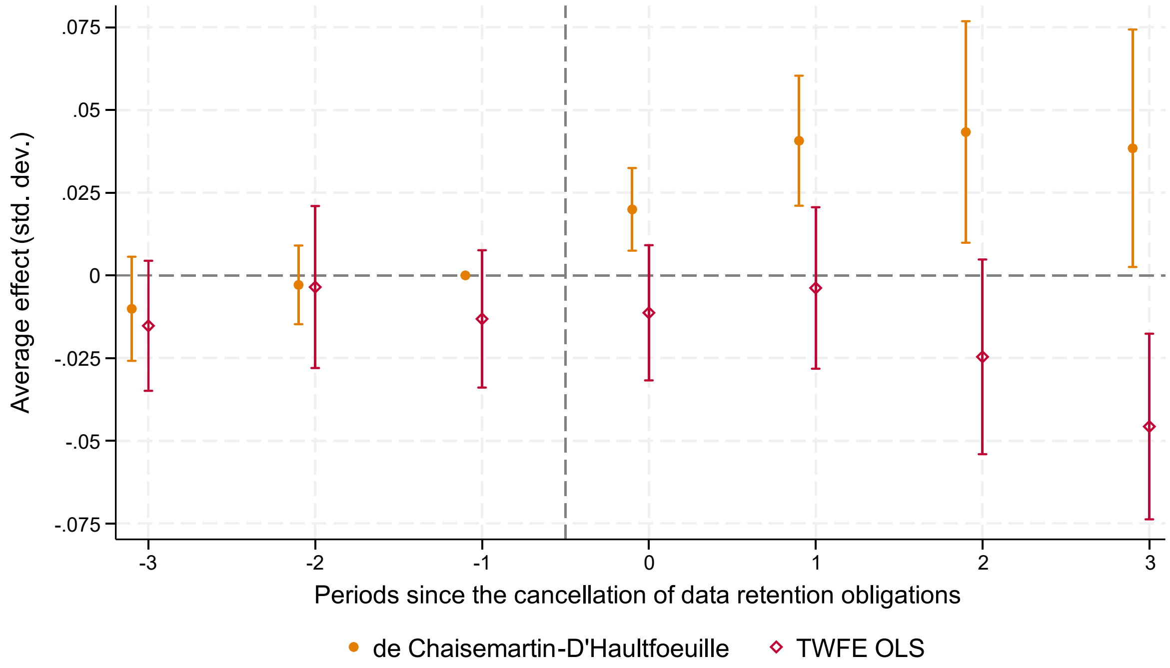
For property crime rates, we observe a trend that is comparable to that found in the prior analysis of aggregate crime. In the year in which the data retention scheme is lifted, our estimates indicate a rise in property crime of 4.1 percent. Again, the effect is aggravated in the following year and remains at a similar level thereafter. Similar to the estimates of introducing data retention schemes, the results show an increase in property crimes by 8 to 9 percent in the treatment group compared to their untreated counterparts. Our placebo estimates are close to zero and insignificant.

Regarding violent crime, we do not find a significant difference in crime rates between the control and treatment groups. Note that the placebo tests indicate a violation of the common trend assumption three years before the data retention obligation is cancelled; the estimated coefficients provide no more than an indication of the direction of the effects. However, these estimates are broadly in line with the earlier estimates (Table 4), suggesting that violent crimes are barely affected by data retention schemes.

Data retention schemes are likely to also have an impact on other types of crime where the lack of data coverage prevents a detailed analysis. Crime categories that may potentially be affected include, for example, all types of cybercrime and (economic)

[1] In the appendix, we provide a table containing the two-way fixed effects estimation results.

Figure 2. The effect of cancelling data retention schemes on aggregate crime rates.



Notes: The x-axis indicates the time period (in years) before and after a data retention scheme has been cancelled in the treatment groups (implementation at t = 0). The markers display the corresponding differences in aggregate crime rates (in logs) between regions that cancelled a data retention scheme at t = 0 and the control group. Estimates before the implementation show placebo estimates to test the common trend assumption. The estimates on the right of t = 0 present the effect of being treated for up to three years. We estimate standard errors using 100 bootstrap replications clustered at the regional level and report 95 percent confidence intervals.

fraud. While there is not enough data on cybercrime to conduct a valid analysis, the data coverage regarding fraud is sufficient to provide intuition about the direction of effects, at least in terms of cancelling data retention obligations. As displayed in Table A5 (Column III) in the appendix, fraud rates may react even stronger to data retention obligations than property crimes. Intuitively, potential offenders may evaluate apprehension probabilities more carefully in the case of fraud than in other types of crime.

Overall, the estimates confirm the implications of our main analysis. While we observe a decline in aggregate and property crime after a data retention scheme has been introduced, our robustness tests show that these crime rates increase significantly if the scheme is cancelled. Violent crime rates do not react to either the introduction or cancellation of data retention laws. The coefficients are largely consistent concerning alternative specifications and unlikely to result from different pre-trends. In addition, our identification strategy ensures that our results are not biased by heterogeneous treatment effects or dynamic effects.

5 Summary and Outlook

We evaluate the effect of data retention schemes on crime rates in Europe. Therefore, we exploit differences in the implementation between member states to identify the effects using a difference-in-difference estimator. We conduct several placebo tests to evaluate the validity of the underlying common trend assumption and control for demographic characteristics and time trends to ensure comparability of the treatment and control groups.

Accounting for heterogeneous treatment and dynamic effects, we find a negative effect of data retention schemes on aggregate crime rates. The results are robust against alternative specifications and indicate a continued reduction in aggregate crime rates of between 5.9 and 9.7 percent up to three years after introduction. (Potential) offenders seem to learn only gradually about policy changes. Our observation supports findings by other studies that delinquents are largely uninformed about changes in crime policies. Potential offenders respond to first-hand experiences with the law, even if they are provided with information on harsher anticrime policies (Dušek and Traxler 2022).

In addition, we provide evidence that the effects vary depending on the type of crime that we analyse. We do not find any effect of data retention schemes on violent crime, while we show that property crime rates are reduced by 7 to 8.2 percent two years after the introduction of such a scheme. The results are broadly in line with the findings of Klick and Tabarrok (2005), who report a significant effect of increased police presence on property crimes but not on violent crimes. Data retention schemes may be of less help in the case of violent crimes because they may also be driven by emotions rather than rational decisions. Note that our findings on the effect of data retention schemes on violent crime contrast with findings of other studies that show a significant impact of other anticrime policies on violent crime (Doleac 2017; Gómez et al. 2021).

In our sample, property crimes account for approximately 50 percent of all crimes. While the effect on aggregate crime is seen to be approximately half of the effect on property crime in the robustness test, we observe coefficients of similar magnitude when analysing the introduction of data retention schemes. Thus, the impact on aggregate crime may not be exclusively attributable to the changes in property crimes. Data retention strategies could plausibly influence cybercrime, fraud, or other forms of misconduct. Nevertheless, insufficient data limit more comprehensive investigations into additional categories of criminal offenses.

The obligation to retain communication data has been heavily discussed among members of the European Union. In particular, the question of whether potential benefits in crime prevention outweigh concerns regarding data protection has led to a controversial debate. To the best of our knowledge, this is the first paper to provide empirical evidence on the effect of data retention schemes. Although our analysis does not address the economic value of data protection and privacy, our estimates may contribute to a future calculation of a cost–benefit analysis of data retention obligations. Acquisti, Taylor, and Wagman (2016) provide an overview of the theoretical and empirical literature that tries to evaluate the economic value of personal data protection, which may serve as a basis of the discussion of privacy concerns.

We acknowledge potential limitations in our paper. First, an even larger data basis would be desirable. Additional analyses about the effects of data retention schemes on further types of crime would enhance our research. In this context, an estimation of changes in cybercrime rates would be a natural choice when evaluating the impact of data retention schemes. However, only a few countries reported their number of cybercrimes during our period of interest. Nevertheless, we find significant changes in aggregate crime rates, which may lead to the conclusion that the effect may be distinct for digital crimes, as previous studies have shown significant negative effects of deterrence policies in the case of cybercrime (Maimon et al. 2014).

To date, it is still debated in some European countries whether data retention schemes should be reintroduced or completely cancelled. As data coverage on cybercrime has improved considerably, researchers may exploit possible introductions in the future to complement and affirm our results.

Appendix

A1 Data

Our dataset combines regional data from various sources. Table A1 lists all data sources used in this paper. Further information is available on request.

Table A1. Data Sources

Country	Demographic Data	Crime Data
Austria	Statistic Austria, Eurostat	Federal Ministry of the Interior Austria
Belgium	StatBel, State Office for Employment, Eurostat	Federal Police
Czech Republic	Czech Statistical Office, Eurostat	Czech Statistical Office
Denmark	Statistics Denmark, Eurostat	Statistics Denmark
Finland	Statistics Finland, Eurostat	Statistics Finland
Germany	German Statistical Office, Eurostat	Police Crime Statistics (PKS)
Italy	IStat, Eurostat	IStat
Netherlands	Statistics Netherlands, Eurostat	Statistics Netherland
Poland	Statistics Poland, Eurostat	Statistics Poland
Portugal	Statistics Portugal, Eurostat	Statistics Portugal
Slovakia	Statistical Office of the Slovak Republic, Eurostat	Ministry of Interior
Spain	National Statistics Institute, Eurostat	Crime Statistics Portal, Ministry of Interior

A2 Additional Estimation Results

For completeness, Table A2 and Table A3 present estimates on the effect of introducing data retention schemes. The effects are qualitatively similar to our main results. In the specifications that also control for clearance rates, the smaller sample alleviates the precision of our estimates and partially violates the common trend assumption. Therefore, the results solely offer a directional intuition of the effect.

Similarly, Table A4 and A5 displays additional results regarding the effect of cancelling data retention laws. First, we show the results of our two-way fixed effects approach, which is likely to produce biased results. Second, we present estimates concerning fraud rates, indicating a strong positive effect of canceling data retention schemes on fraud rates. Note that linear trends are necessary to generate a common trend.

Table A2. Additional estimates on the effect of introducing data retention schemes.

	Dependent Variable: Log Crime Rates					
	Aggregate Crime		**Property Crime**		**Violent Crime**	
	(I) DCDH		(II) DCDH		(III) DCDH	
Average Treatment Effects (ATE)		*N*		*N*		*N*
ATE $_{(t = 0)}$	0.0005 *(0.0074)*	*267*	-0.0089 (0.1360)	*248*	0.0166 *(0.0122)*	*260*
ATE $_{(t = 1)}$	-0.0133 *(0.0126)*	*192*	-0.0361 (0.0233)	*178*	-0.0171 *(0.0177)*	*187*
ATE $_{(t = 2)}$	-0.0411** *(0.0167)*	*149*	-0.0816** (0.0352)	*138*	-0.0304 *(0.0220)*	*144*
ATE $_{(t = 3)}$	-0.0591*** *(0.0213)*	*116*	-0.0725 (0.0480)	*105*	-0.0004 *(0.0335)*	*111*
Placebo Estimates (PL)						
PL $_{(t = -2)}$	-0.0080 *(0.0063)*	*260*	-0.0091 (0.0075)	*241*	0.0079 *(0.0090)*	*253*
PL $_{(t = -3)}$	-0.0154* *(0.0093)*	*158*	-0.0124 (0.0100)	*144*	0.0253 *(0.0190)*	*153*
PL $_{(t = -4)}$	-0.0034 *(0.0146)*	*121*	0.0054 (0.0077)	*110*	0.0063 *(0.0283)*	*115*
Controls						
Fixed Effects	YES		YES		YES	
Linear Time Trend	NO		NO		NO	
Demographic and Economic Determinants	YES		YES		YES	

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively. N is the number of observations that are used to calculate the treatment effect.*

Table A3. Additional estimates on the effect of introducing data retention schemes.

	Dependent Variable: Log Crime Rates					
	Aggregate Crime		**Property Crime**		**Violent Crime**	
	(I) DCDH		(II) DCDH		(III) DCDH	
Average Treatment Effects (ATE)		*N*		*N*		*N*
ATE $_{(t = 0)}$	0.0005 *(0.0074)*	*165*	-0.0089 (0.1360)	*148*	0.0166 *(0.0122)*	*160*
ATE $_{(t = 1)}$	-0.0133 *(0.0126)*	*117*	-0.0361 (0.0233)	*105*	-0.0171 *(0.0177)*	*114*
ATE $_{(t = 2)}$	-0.0411** *(0.0167)*	*90*	-0.0816** (0.0352)	*81*	-0.0304 *(0.0220)*	*87*
Placebo Estimates (PL)						
PL $_{(t = -2)}$	-0.0080 *(0.0063)*	*165*	-0.0091 (0.0075)	*148*	0.0079 *(0.0090)*	*160*
PL $_{(t = -3)}$	-0.0154* *(0.0093)*	*96*	-0.0124 (0.0100)	*84*	0.0253 *(0.0190)*	*93*
Controls						
Fixed Effects	YES		YES		YES	
Linear Time Trend	NO		NO		NO	
Demographic and Economic Determinants	YES		YES		YES	
Clearance Rates	YES		YES		YES	

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively. N is the number of observations that are used to calculate the treatment effect.*

Table A4. Two-way fixed effects estimation on the effect of cancelling data retention schemes.

	Dependent Variable: Log Crime Rates		
	Aggregate Crime	**Property Crime**	**Violent Crime**
	(I) TWFE	(II) TWFE	(III) TWFE
Average Treatment Effects (ATE)			
ATE $_{(t=0)}$	-0.0113 *(0.0104)*	-0.0297 *(0.0193)*	-0.1279*** *(0.0287)*
ATE $_{(t=1)}$	-0.0038 *(0.0125)*	-0.0143 *(0.0211)*	-0.1207*** *(0.0244)*
ATE $_{(t=2)}$	-0.0246 *(0.0150)*	-0.0374** *(0.0187)*	-0.0932*** *(0.0284)*
ATE $_{(t=3)}$	-0.0457*** *(0.0143)*	-0.0497*** *(0.0157)*	-0.0638*** *(0.0179)*
Placebo Estimates (PL)			
PL $_{(t=-1)}$	0.0029 *(0.0080)*	-0.0438** *(0.0204)*	-0.1314*** *(0.0262)*
PL $_{(t=-2)}$	-0.0035 *(0.0125)*	-0.0084 *(0.0227)*	-0.1554*** *(0.0247)*
PL $_{(t=-3)}$	-0.0132 *(0.0106)*	-0.0310* *(0.0158)*	-0.0418 *(0.0391)*
Controls			
GDP (Logs)	0.1161 *(0.1139)*	0.3168** *(0.1458)*	-0.1685 *(0.1326)*
Unemployment	-0.0038** *(0.0018)*	0.0080 *(0.0061)*	0.0053 *(0.0037)*
Share 20-25	0.0669*** *(0.0208)*	0.0759** *(0.0338)*	0.0107 *(0.0305)*
Density (Logs)	1.1055** *(0.5367)*	0.8104 *(0.9992)*	1.6842** *(0.7626)*
Education	-0.0013 *(0.0023)*	0.0003 *(0.0044)*	-0.0064* *(0.0037)*
Fixed Effects	YES	YES	YES
Linear Time Trend	YES	YES	YES
R^2	0.87	0.87	0.84
Observations	1571	1357	1357

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively.*

Table A5. Estimated effect of cancelling data retention schemes on fraud rates.

	Dependent Variable: Log Crime Rates					
	Fraud					
	(I) DCDH		(II) DCDH		(III) DCDH	
Average Treatment Effects (ATE)		*N*		*N*		*N*
ATE $_{(t=0)}$	0.1945*** *(0.0585)*	*131*	0.2403*** *(0.0577)*	*109*	0.3229*** (0.0640)	*109*
ATE $_{(t=1)}$	0.1610* *(0.0846)*	*131*	0.2437** *(0.0968)*	*109*	0.3166*** (0.1105)	*109*
ATE $_{(t=2)}$	0.0255 *(0.0693)*	*131*	0.0951 *(0.0908)*	*109*	0.2753** (0.1216)	*109*
ATE $_{(t=3)}$	-0.0735 *(0.0798)*	*131*	-0.0039 *(0.0984)*	*109*	0.2817* (0.1514)	*109*
Placebo Estimates (PL)						
PL $_{(t=-2)}$	0.0661* *(0.0338)*	*131*	0.0344 *(0.0305)*	*109*	-0.0038 (0.0264)	*109*
PL $_{(t=-3)}$	0.1578* *(0.0818)*	*120*	0.1136* *(0.0650)*	*109*	0.0525 (0.0585)	*109*
PL $_{(t=-4)}$	0.2401*** *(0.0698)*	*120*	0.1861*** *(0.0601)*	*109*	0.1152*** (0.0425)	*109*
Controls						
Fixed Effects	YES		YES		YES	
Demographic and Economic Determinants	NO		YES		YES	
Linear Time Trend	NO		NO		YES	

*Notes: Standard errors are in parenthesis; *, **, and *** indicate significance at the 10 percent, 5 percent or 1 percent level, respectively. N is the number of observations that are used to calculate the treatment effect.*

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