Juvenile Law and Recidivism in Germany - New Evidence from the Old Continent

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Abstract

In this paper, we analyze the dependence of recidivism on juvenile and criminal law. Using a unique sample of German inmates, we are able to disentangle the selection into criminal and juvenile law from the subsequent recidivism decision of the inmate. We base our identification strategy on two distinct methods. First, we jointly model the selection and recidivism equation in a bivariate probit model. In a second step, we use the discontinuities in assignment created by German legislation and apply a (fuzzy) regression discontinuity design. In contrast to the bulk of the literature, which mainly relies on US data, we do not find that the application of criminal law increases juvenile recidivism. Rather, our results suggest that sentencing adolescents as adults reduces recidivism in Germany.

JEL Classification: K42, K14, C21, C14

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1. Introduction

Crime has been a major problem in all societies throughout time. However, there is still no clear answer to the debate on optimal criminal legislation. From an economist's perspective crime can be seen as the result of rational behavior. According to this approach, which goes back to Becker (1968), it is individually rational to commit a crime if illegal income opportunities outweigh the legal ones. Hence, legislation should result in severe punishments increasing the expected costs of crime and

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thus augmenting general deterrence. However, once an individual has been caught offending, the goal shifts to minimizing the probability of the individual re-offending, or specific deterrence. This reveals a potential dilemma: While the optimal punishment should result in costs high enough to deter potential offenders, it should not diminish the offender's chances of re-entering the legal labor market ex post. Western, Kling, and Weiman (2001) summarize the evidence on the influence of incarceration on future earnings and find stigma to be the most important mechanism. Further, incarceration can increase the individual payoffs from crime by inducing a taste for violence (Banister, Smith, Heskin, and Bolton, 1973) or other peer effects (Bayer, Hjalmarsson, and Pozen, 2009; Glaeser, Sacerdote, and Scheinkman, 1996). Thus, the severeness of punishment can have opposing effects.

This ambivalence is of particular importance if delinquents suffer from some kind of myopia - or simply do not correctly anticipate their future income opportunities - and commit crimes even though a fully rational actor would not have taken this decision. Youths seem to be especially prone to this kind of behavior. The literature on personal development found that they suffer from a maturity gap (Moffitt, 1993) which temporarily increases their inclination towards criminal activity (e.g. Thornberry, Huizinga, and Loeber, 2004). This leads to the belief that juveniles are more rehabilitable and less culpable than adults (Mears, Hay, Gertz, and Mancini, 2007). As a consequence, in the case of young offenders the general deterrence effect of harsh sentences is limited while the effect on reintegration into the legal job market gains relative importance.

In many countries, this line of thought led to a special treatment of juvenile offenders.[1] However, in the last decades, an increasing number of serious and highly aggressive acts of juvenile violence have called this policy into question (see Aebi, 2004; Oberwittler and Höfer, 2005). The most prominent reactions come from the US, where decreasing public support for a preferential treatment of minors resulted in

[1] The Illinois Juvenile Court Act of 1899 marks the beginning of an organized juvenile court system in the USA (Bishop and Decker, 2006, p. 17). In Germany, courts started developing special court chambers dealing with young delinquents in 1908 while the Juvenile Justice Act (JJA – Jugendgerichtsgesetz) was passed in 1923 (Dünkel, 2006, p. 226).

tougher laws transferring more juvenile offenders to a criminal court (Moon, Sundt, Cullen, and Wright, 2000). In Germany, the recent and ongoing coverage of violent crimes in the media has resulted in a strong pressure on politics (Bundestag, 2009) and leading criminologists (Heinz, 2008) to address the question of how to deal with juvenile and adolescent offenders.

German survey data seems to suggest a higher rate of recidivism of those sentenced under juvenile law. Jehle, Heinz, and Sutterer (2003) analyzed the official register survey data on recidivism for the years 1994 to 1998. The recidivism rate within four years after unconditional prison sentence under juvenile law was 79.0%, whereas it was 43.6% for those sentenced under criminal law. Does this mean that juvenile law has failed in Germany? Of course, descriptive statistics do not allow for causal interpretation and inference, especially, since the unconditional propensity to offend might be systematically different in the two groups independent of any treatment effect. Criminal behavior has been found to depend on age. Also, covariates might have a different influence depending on age, suggesting a restriction of the analysis to individuals at the transition between the two legal regimes. German legislation does not provide a sharp age limit separating juvenile from criminal law. Rather, the transition involves two steps. The application of criminal law is possible if the offender has turned 18 and becomes mandatory upon turning 21. In the discretionary phase between 18 and 21, the choice of applicable law is delegated to the judges allowing for individual decisions based on the offender's characteristics.

In this paper, we take advantage of this mechanism and analyze individuals in the discretionary phase. We hypothesize that there are unobservable factors influencing both the treatment assignment and the outcome variable. In order to avoid the emerging selection bias, we perform a simultaneous maximum likelihood estimation of the selection and treatment equation. Further, we use the step function in law assignment for a regression discontinuity analysis assuming a random distribution of individuals around the discontinuities. Our findings show that adolescents sentenced as adults have a lower self-reported probability of recidivism than those sentenced as juveniles. This result is obtained in both identification strategies and persists in several robustness checks.

Our analyses shed new light on the impact of juvenile legislation on recidivism, making several contributions to the literature. First, we apply modern econometric techniques to control for the suspected selection bias. Further, we base our research on German data, providing one of the few micro-level studies on the drivers of juvenile recidivism outside the US. Prison conditions and legislation in Germany - and in continental Europe in general - are substantially different as compared to the Anglo-Saxon world, questioning the external validity of US findings. In fact, combining our findings with US studies we postulate a U-shaped pattern between severity of punishment and recidivism, where Germany lies to the left and the US to the right of the minimum.

Moreover, our results have implications for juvenile legislation across Europe, since the Committee of Ministers of the Council of Europe is trying to establish European standards of juvenile law explicitly mentioning the German rules as a good example (see memorandum to recommendation Rec(2003)20). The exemplary character of German juvenile legislation is based on both its flexible mechanism and the general state based legal framework which resembles the legal structure of the European Union (Bochmann, 2009, p. 122).

The remainder of the paper is organized as follows. Section 2 summarizes the related literature. Section 3 describes the database and provides summary statistics from the sample. Section 4 provides the empirical specification. Sections 5 and 6 describe the identification strategies and report the results of our two alternative approaches, namely bivariate probit and regression discontinuity. In section 7 we discuss the results and section 8 concludes.

2. Related Literature

2.1. Empirical Evidence

The empirical literature has studied the influence of juvenile law on both general and specific deterrence. We start out by looking at the empirical evidence on general deterrence. The literature provides an ambiguous answer to the question of whether transferring juveniles to criminal courts deters any would-be offender (see

Redding (2006) for a good survey on this field). Levitt (1998) found increased general deterrence when transferring adolescents to adult courts. This would suggest rational behavior of the youths confirming the Becker hypothesis. However, other studies have found no general deterrence effect (Singer and McDowall, 1988; Steiner, Hemmens, and Bell, 2006) or even increased arrest rates (Jensen and Metsger, 1994). In a more recent paper, Lee and McCrary (2009) found evidence that young adults hardly respond to the harsher punishments they face upon turning 18. They argue that young offenders misjudge likelihood and severity of the imminent punishments and can thus be characterized as myopic. In summary we can say that even though there is no clear answer, the more recent - and perhaps more sophisticated - studies confirm the behavioral findings mentioned above questioning the rational offender hypothesis for the case of juvenile delinquents.

With respect to specific deterrence there is much clearer evidence. The majority of the studies using US data find that trying and sentencing juvenile offenders as adults increases the likelihood that they will reoffend. Fagan (1996) studied differences in recidivism rates of 15- and 16-year-old juveniles, taking advantage of the fact that in New Jersey young delinquents were sentenced by a juvenile court while in New York they appeared before a criminal court. He found significantly lower recidivism rates for those sentenced by juvenile courts, suggesting that the special jurisprudence for juvenile crimes is an effective measure. Confronted with the critique that the results might be driven by a selection bias, Kupchik, Fagan, and Liberman (2003) replicated the study including several control variables confirming the original results. In a related study, Bishop, Frazier, Lanza-Kaduce, and Winner (1996) analyzed recidivism in Florida, where the transfer of delinquents depends on the decision of the prosecutor. They found higher recidivism rates for those delinquents transferred to criminal courts. Again, they could not rule out the existence of a selection bias distorting the results. However, in a follow-up study Lanza-Kaduce, Lane, Bishop, and Frazier (2005) still found a positive effect of transfers when using both a richer dataset and matching techniques. Further studies by Myers (2003), Podkopacz and Feld (1995) and Thornberry, Huizinga, and Loeber (2004) point into the same direction.

Summarizing, the empirical evidence is mainly US-based and generally supports the claim that the application of criminal law increases juvenile recidivism. However, it is questionable whether these findings are also valid for Germany due to substantial differences in the legal systems. Moreover, most of the US evidence is based on the comparison of minors being either sent to a criminal or a juvenile court. The German legal system does not allow for such a situation, as summarized in the next subsection.

2.2. Juvenile Law in Germany

In Germany, juvenile law is mandatory for all minors, i.e. for all persons who have not yet turned 18 at the time the criminal act was committed. For adolescent delinquents, i.e. those aged between 18 and 21 years when offending, the legislator left the decision to the courts whether to apply juvenile or criminal law. In more detail, courts are asked to apply juvenile law whenever the offender acts "equal to a juvenile regarding moral and mental development at the time of the act" (§ 105 (1) Juvenile Justice Act – Jugendgerichtsgesetz). Finally, delinquents of at least 21 years have to be sentenced under criminal law. Comparing this fact with the US practice, we find no state where the maximum age of application of juvenile law has been extended as far as in Germany. In 2006, the automatic treatment as an adult started either at age 18 (37 states), age 17 (10 states) or age 16 (3 states) (see Bishop and Decker, 2006, p. 13). Summarizing, German legislation allows for a much wider application of juvenile law than its US counterpart.

A correct model for law assignment requires knowledge of the decision criteria. According to Dünkel (2006) judges think strategically when choosing whether to apply criminal or juvenile law.[2] Juvenile law allows for milder sanctions, since certain minimum penalties that exist in criminal law (e.g. 3 years in the case of robbery) do not have to be considered. This suggests that juvenile law is applied when judges find shorter punishment to be advantageous. Given this selection process, it seems

[2]The transferability of Dünkel's result might be limited since he is looking at the whole range of sentences, while we only consider incarceration.

to be very likely that offenders selected for juvenile law differ systematically from those who are not, also in the expected likelihood that they recidivate.

Besides the length of the punishment, the type of custody also can potentially influence recidivism. § 92 of the German Juvenile Justice Act (Jugendgerichtsgesetz) states that juveniles and adults have to be kept in separate prisons or at least in separate departments of the same prison in order to avoid contact between adult and juvenile offenders. Following Lange (2007) the most notable difference between juvenile and criminal prisons is that criminal prisons have the primary goal of punishment, while juvenile prisons are focused on social education e.g. by the provision of personal custodians for the delinquents. Furthermore, according to Dölling, Bauer, and Remschmidt (2007), juvenile law is generally less stigmatizing as opposed to criminal law.

Entorf, Möbert, and Meyer (2008, p. 139-152) summarize differences of juvenile and criminal prisons in Germany. The authors find that, on average, juvenile prisons have more money at their disposal and thus can offer a more convenient and stimulating environment. Juvenile prisons, for instance, offer more common rooms for eating, sports and other activities. Also, a higher fraction of juvenile delinquents is placed in a single room (83%) as compared to adult delinquents (55%). While in a criminal prison there are less than 50 employees for 100 inmates, there are almost 70 employees in juvenile prisons. This allows juvenile prisons to provide schooling opportunities and to offer more seminars, e.g. on how to deal with drug and alcohol problems.

The different facilities can affect recidivism in two ways. On the one hand, being an inmate in a more convivial prison environment can dampen the deterrence effect and lead to higher recidivism rates. On the other hand, juvenile prisons might decrease the likelihood of recidivism due to their educational concerns and their less stigmatizing effect on future job chances. Our results will provide an answer to the question which of the two effects dominates.

3. Data

Our analysis is based on a prison survey that was conducted in 31 German prisons in 2003 and 2004 using a questionnaire with 123 questions.[3] It uses a two-stage approach combining stratified and random sampling. First, a representative sample of the population of prisons in Germany was created. Second, a random selection from this population completed the sampling.

The questionnaire was given to 13,340 selected inmates in either the German, Turkish, Serbo-Croatian, Russian, Polish or English language to take account of the different nationalities of the inmates. It was completed by 1,771 respondents resulting in a response rate of 13.3%. The low response rate - even though it is a standard problem when dealing with survey data - might raise doubts about a potential selection bias. However, when comparing sample characteristics to those of the average prison population in Germany, there is no evidence of a selection bias.[4]

The original dataset can be grouped into three subsamples: inmates in pretrial custody, inmates sentenced under juvenile law and inmates sentenced under criminal law. Since we are interested in the effect of the type of law applied, we only use the last two subgroups. Further, our analysis focuses on adolescent delinquents. Hence, we also disregard all individuals younger than 14 and older than 25 when committing a crime. This leaves us with a sample of 245 inmates. When estimating the treatment assignment function we further restrict the sample to adolescents, yielding a subsample of 90 observations. The descriptive statistics for both samples can be found in table 1.

3.1. Expected Recidivism

Our target variable is a self-reported measure for expected recidivism. It is constructed from the response to the following survey question:

[3]The survey was initiated and carried out by Horst Entorf and a team of researchers from Darmstadt University of Technology.

[4]For a more detailed analysis of this issue and the dataset in general see Entorf (2009).

Table 1: Summary statistics

Sample	$14 \leq$ **ageoffense** ≤ 25				$18 \leq$ **ageoffense** ≤ 21	
Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**Mean**	**Std. Dev.**
expected recidivism	0.2531	0.4357	0	1	0.3	0.4608
age	22.8796	3.2604	16.5	35.5	21.4222	1.63
ageoffense	20.5276	2.666	14.5833	25	19.4546	1.0189
female	0.102	0.3033	0	1	0.0333	0.1805
city	0.3602	0.4811	0	1	0.4886	0.5027
married	0.0943	0.2928	0	1	0.1	0.3017
social contact	0.5432	0.4992	0	1	0.5444	0.5008
poor social capital	0.4898	0.5009	0	1	0.4556	0.5008
addiction	0.3029	0.4605	0	1	0.2778	0.4504
crim parents	0.1345	0.3419	0	1	0.1685	0.3765
job contact	0.3077	0.4625	0	1	0.3256	0.4713
abi	0.0372	0.1896	0	1	0.0333	0.1805
drugs deal	0.1633	0.3704	0	1	0.1556	0.3645
drugs consume	0.0857	0.2805	0	1	0.0889	0.2862
theft	0.3918	0.4892	0	1	0.3778	0.4875
robbery	0.2776	0.4487	0	1	0.3333	0.474
fraud	0.1837	0.388	0	1	0.2	0.4022
bodily injury	0.3265	0.4699	0	1	0.4222	0.4967
vandal	0.0939	0.2923	0	1	0.1444	0.3535
sexual	0.049	0.2163	0	1	0.0333	0.1805
murder	0.1184	0.3237	0	1	0.1111	0.316
open	0.1639	0.371	0	1	0.1111	0.316
sentence length	3.5192	3.1234	0.0833	15	2.9963	2.0739
criminal law	0.4939	0.501	0	1	0.1333	0.3418
Nobs	245	245	245	245	90	90

"Could it occur that after your release from custody you come into conflict with the law and end up in prison?"

Inmates were asked to answer this question on a 5-point scale, where a 1 stands for "no, never" and 5 corresponds to "absolutely certain". For reasons of small sample size, we translate the answers to this question into a binary variable *recidivism*. In the data, the answers are positively skewed: 43.5% of the respondents answered with the lower extreme "no, never" while only 4% said they were absolutely certain to reoffend. Therefore, we set recidivism to zero if the respondent chose either answer 1 or 2, and set the binary variable to one for those with a higher self-reported probability of ending up in prison again (answers 3-5).[5]

One might raise objections against using self-reported recidivism as a proxy for real recidivism. There are at least three arguments in favor of our approach. First, there is evidence that self-reported and real recidivism are correlated (Corrado, Cohen, Glackman, and Odgers, 2003). Second, using expected recidivism as compared to actual recidivism avoids the problem of a selection bias when conducting a follow-up survey to collect actual recidivism. Third, we do not face the problem of a potential omitted variable bias due to additional factors that influence actual recidivism after the release from prison.

Nevertheless, it might be enlightening to discuss possible effects of a measurement error. A general bias, affecting all individuals in the same way and resulting in a generally too high (or too low) rate of recidivism, would not pose a threat to the validity of the estimated treatment effect of criminal law. Our results lose validity, however, if individuals in the treatment group have a different measurement error than those in the control group. To generate such an effect, the applied law type must change the precision of the self-reported measures. One might suspect inmates in adult prisons to have a more precise estimate of their future while those in juvenile prisons systematically over- or underestimate their propensity to recidivate. Even

[5]This strategy has been suggested and used by Entorf (2009). We also tried different ways of bundling the original multinomial variable, which did not change the results.

though such effects are not likely to drive the results, we take this possibility into account when discussing our findings.

3.2. Age at offense

As shown in section 2.2, the years of age when committing the crime (*ageoffense*) are crucial for the assigned type of law. Since this information did not appear in the survey directly, we constructed it using both time and age when surveyed and the time when the crime was committed (both given at a monthly precision level). With regard to the latter, inmates could choose to indicate either a point in time or an interval. For a given point in time the calculation is straightforward. When dealing with an interval, we use the end of the interval.[6]

In addition, we have to deal with different precision levels of the relevant points in time. Age when surveyed is reported in (completed) years, which gives rise to a possible error of nearly 12 months. In order to minimize this mistake we added 6 months to the calculated age at offense.[7] The missing precision of this variable might threaten the regression discontinuity analysis, since *ageoffense* is the variable that is crucial for the applied type of law. However, checking for contradictions with the treatment assignment mechanism confirms the plausibility of this variable. Furthermore our analysis relies on two independent identification strategies and the variable is only crucial for one of them.

3.3. Additional Regressors

Throughout the study we use several control variables. First, we include personal characteristics of the inmate, such as *age* (at the time of the interview) and gender

[6] According to § 32 Juvenile Justice Act (Jugendgerichtsgesetz) judges have to stick to one type of law when dealing with multiple offenses. The crucial factor is the age when committing the "main offenses". Lacking a measure for severity in the data, we suspect the end of the interval to be more important, since judges might lack information on the start of the criminal activity or simply lend more weight to more recent offenses. We also used the mean as a robustness check, yielding similar results in the regressions and increased inconsistencies in the age classifications. Based on these assumptions we think that our variable is the best available proxy for the real age at offense.

[7] Assuming a uniform distribution of the variable, the transformation allows for a reduction of the average mistake from 0.5 to 0.25.

(*female*). Consistent with national criminal survey statistics, there is a strong majority of male inmates in our sample. The majority of the inmates does not live in a *city*. Only few inmates are married, which can be explained by the fact that we are only considering individuals aged 14 to 25 when committing the crime. A variable that might replace the marriage property for young individuals is frequent contact to a partner in the month before incarceration (*social contact*), which holds true for roughly half of the inmates in the sample. Further, we measure participation in social clubs, e.g. sports clubs or the voluntary fire brigade, mapping the lack of active participation into the dummy variable *poor social capital*. Almost half of the inmates in the sample reported no active participation in social clubs. Roughly one third of the inmates suffer from either alcohol or drug *addiction*. This might be linked to recidivism both in a direct way - in the sense that addicted people might more easily commit crimes under the influence of drugs - and in an indirect way - in the sense that these people might see criminal behavior as a way to finance their addiction (see e.g. Entorf and Winker, 2008; Goldstein, 1985; Harrison, 1992). Criminal family background is another ingredient that could matter for expected recidivism: the dummy variable *crim parents* captures past convictions of parents or siblings and applies for roughly every eighth inmate in our sample.

Another interesting aspect are variables that control for job opportunities. *Job contact* is a binary variable containing the information on whether inmates reported having a job opportunity when leaving jail or having at least contacted a future employer, which holds true for almost every third inmate. Also, schooling has been found to be a determinant of juvenile crime which can be explained by incapacitation effects (Kruger and Berthelon, 2011) or by the assumption that education is a positive asset in the legal labor market but of limited value for criminal activities (Entorf, 2009). In our sample, only very few inmates hold a German high school diploma equivalent (*abi*).

We also have information on the type of offense that led to the present incarceration. It is likely that different types of crime are connected with different probabilities of recidivism. For instance for organized and drug-related crimes there might be a higher probability of relapse due to physical addiction and the influence of the social network. Observe that inmates were allowed to report more than one type of crime, which means that the crime frequencies will not add up to one. In our sample, the most frequently reported crime is *theft*, followed by *bodily injury* and *robbery*. With regard to drug-related crimes, we observe drug dealing (*drugs deal*) more often than consumption (*drugs consume*).

In addition, we include length and type of the sentence the inmate is currently serving. In terms of applied legislation, almost half of the delinquents were sanctioned under *criminal law*. Further, we know the prison to which the delinquent has been assigned (see table 2). Roughly every sixth inmate in the sample is transferred to an *open* institution. We also observe the individual *sentence length* measured in years. In line with German legislation, we deem lifelong punishments to be a 15-year sentence, which represents the maximum length in our sample.

Table 2: Prisons of inmates in sample

JVA #	Location	Tried as Adults (Adolescents)	Tried as Adults (Total Sample)
1	Adelsheim	0.0%	0.0%
4	Bayreuth	0.0%	60.0%
8	Bützow	57.1%	80.0%
15	Flensburg	100.0%	100.0%
19	Heilbronn	0.0%	66.7%
20	JSA Berlin	0.0%	0.0%
23	JSA Rockenberg	0.0%	0.0%
27	Lübeck	100.0%	100.0%
38	Schwäbisch Gmünd	60.0%	87.0%
46	Würzburg	100.0%	100.0%
	Total	13.3%	49.4%

4. Empirical Specification

The goal of this study is to analyze the effect of being sentenced under criminal law (as opposed to juvenile law) on adolescent offenders' recidivism. Considering criminal law to be a treatment that influences recidivism, this translates into the identification of the corresponding treatment effect. Defining ER_i as a measure of expected recidivism and $T_i \in \{0,1\}$ as the treatment indicator of individual i, we can write

$$ER_i = (1 - T_i)ER_i^0(X_i) + T_i ER_i^1(X_i). \tag{1}$$

where $ER_i^0(X_i)$ is expected recidivism when juvenile law has been applied, while $ER_i^1(X_i)$ is expected recidivism when criminal law has been applied. Both expressions are a function of a list of variables X_i. While the influence of a continuous variable is usually measured in its marginal effect, the corresponding expressions for a binary variable like the treatment indicator are different conditional means (see e.g. Heckman and Navarro-Lozano, 2004). The most intuitive measure is the average treatment effect (ATE), which is simply the expected difference in the outcome variable conditional on the covariates. Based on the setup in (1) and dropping the observation index (i), this effect is defined by

$$\text{ATE} = E[ER^1 - ER^0|X]. \tag{2}$$

A related concept is the average treatment effect on the treated (ATET) which in our setup is defined by

$$\text{ATET} = E[ER^1 - ER^0|X, T = 1]. \tag{3}$$

Note that both effects describe a counter-factual outcome and would require the observation of the same individual in both situations, once receiving the treatment and once not receiving it. Since the two situations are mutually exclusive, each individual is observed only once. Hence, observational data only allow us to contrast the mean group outcomes conditional on covariates and treatment status.

$$\Delta_T = E[ER^1|X, T = 1] - E[ER^0|X, T = 0] \tag{4}$$

If treatment assignment is random and the sample is large enough, individuals in both groups have identical characteristics and $E[ER^j|T = 1] = E[ER^j|T = 0] =$

ER^j for $j \in (0,1)$. In this case, the three measures (2)-(4), coincide and can be identified by a simple treatment dummy whose estimate is the sample equivalent of Δ_T. However, if treatment assignment is not perfectly random the three measures can have different values.

First, if untreated offenders would respond differently to the treatment, ATET and ATE will diverge, which we call a reaction bias.

$$\text{ATET} = \text{ATE} + \underbrace{E[ER^1 - ER^0|X, T=1] - E[ER^1 - ER^0|X]}_{\text{Reaction bias}} \tag{5}$$

Further, it is possible to rewrite (4) and decompose Δ_T into a sum of the ATET and a selection bias.

$$\Delta_T = \underbrace{E[ER^1 - ER^0|X, T=1]}_{\text{ATET}} + \underbrace{E[ER^0|X, T=1] - E[ER^0|X, T=0]}_{\text{Selection bias}} \tag{6}$$

The selection bias in (6) is different from zero, if treated and untreated individuals have a different general propensity to recidivate, even when controlling for observables X. Put differently, whenever law assignment is determined at least in parts by the value of an unobserved variable which is correlated with expected recidivism, the sample analogue of Δ_T cannot identify a treatment effect. As Angrist and Pischke (2009, p. 243) point out, this may reflect some sort of omitted variables bias, that is, a bias arising from unobserved and uncontrolled differences between the two groups.

Hence, we have to model the selection process. Law assignment obviously depends on the age at offense which becomes clear when explicitly modeling the global treatment assignment function (GT_i) based on the German legal framework:

$$GT_i(ageoffense, W_i) = \begin{cases} 0 & if \quad ageoffense < 18 \\ T_i(W_i) & if \quad 18 \leq ageoffense < 21 \\ 1 & if \quad ageoffense > 21 \end{cases} \tag{7}$$

When restricting the sample to adolescents, cases with predetermined treatment assignment based on age at offense disappear. In this case, treatment assignment depends on a further set of variables (W). As described in section 2.2, German juvenile law asks judges to apply a maturity criterion in the selection process. Since maturity of the offender might also affect the likelihood of recidivism we have to assume a selection bias based on unobservable characteristics driving both the court's treatment selection and the outcome variable.

In order to overcome this selection bias, we suggest two approaches that allow us to identify the causal effect of treatment. First, we define a bivariate probit model which explicitly controls for treatment assignment and the emerging biases. Second, we apply a regression discontinuity framework which relies on jumps in the treatment assignment function to locally reestablish the random assignment property.

5. Bivariate Probit Approach

Heckman (1978) proposed a general class of simultaneous equation models with endogenous variables to control for a selection bias. However, since our target variable *recidivism* is binary[8], the OLS based estimator on the second stage will suffer from truncation bias (see e.g. Greene and Hensher, 2010, p. 106). This calls for the use of a binary choice model on the second stage also. Maddala (1983) was one of the first to extend Heckman's idea to a setting with two probit equations.[9] In our case, the structural probit equation contains expected recidivism as a function of regressors X_i and the potentially endogenous dummy for treatment assignment

[8] To use the original multinomial target variable for recidivism we would have to either assume identical differences between the categories and use OLS or use a multinomial ordered choice model. While the first assumption seems too strong, the weakness of a multinomial model are its additional cut-points that need to be estimated in addition to the target variable. This will hamper the interpretation of the model coefficients and reduce efficiency in a small sample which made us stick to the probit model. As a robustness check we nevertheless estimated the equation using an Ordered Probit model which did not yield any substantially different results.

[9] A probit model (see Bliss, 1934) bases the binary outcome on a latent function with a normally distributed error term. A second popular approach is the assumption of a logistic distribution function. However, the analysis of a bivariate logit model is fairly inconvenient (see e.g. Imai, King, and Lau, 2007).

$$ER_i^{j*} = X_i'\beta + T_i\delta + \varepsilon_i \quad \text{and} \quad ER_i^j = \begin{cases} 1 & \text{if } ER_i^{j*} > 0 \\ 0 & \text{otherwise} \end{cases} \tag{8}$$

where $j \in (0, 1)$ and the latent variable is denoted with a star ("*"). The second (reduced form) probit equation models treatment assignment as a function of another set of covariates (W_i').

$$T_i^* = W_i'\gamma + \eta_i \quad \text{and} \quad T_i = \begin{cases} 1 & \text{if } T_i^* > 0 \\ 0 & \text{otherwise} \end{cases} \tag{9}$$

However, it is necessary to impose an identifying restriction. In our context, this can be the assumption of an exclusion restriction, meaning that there must be at least one variable in W that is not included in X. We use *ageoffense* for our exclusion restriction, since this age measure is relevant for treatment assignment but should have no direct effect on recidivism. Only the actual age when surveyed should matter for recidivism directly.

In line with the standard bivariate model, we assume that the error terms of both processes, (8) and (9), share the following joint normal distribution

$$\begin{bmatrix} \varepsilon_i \\ \eta_i \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right) \tag{10}$$

where ρ captures their correlation. The joint density of the two error terms then equals

$$\phi(\varepsilon_i, \eta_i) = \frac{1}{2\pi\sqrt{1-\rho^2}} \exp\left[-\frac{1}{2}\left(\frac{\varepsilon_i^2 + \eta_i^2 - 2\rho\varepsilon_i\eta_i}{1-\rho^2} \right) \right]. \tag{11}$$

Correlation in the error terms, i.e. when ρ is not zero, poses a threat to the validity of a single equation model and yields misleading estimates of causal effects, even after controlling for a full set of covariates.[10]

[10] Based on the above density, we can replace the conditional expectations in (6) which allows us to rewrite the selection bias as $Pr\left(\varepsilon_i > -X_i'\beta | X_i, \eta_i > -W_i'\gamma\right) - Pr\left(\varepsilon_i > -X_i'\beta | X_i, \eta_i \leq -W_i'\gamma\right)$. Obviously, the two elements do not coincide if ε and η are not independent.

A solution to this problem is a simultaneous Maximum Likelihood estimator for both equations. An expression for the Log-Likelihood function can be found e.g. in Maddala (1983, p. 123). The Maximum Likelihood estimation will not be biased in the presence of the endogenous parameter in the first equation as pointed out by Greene and Hensher (2010, p.75).

Hence, we perform a simultaneous estimation of the two probit equations. The results can be found in tables 3 and 4. In column 1, we test a very simple model and find a negative but only weekly significant ($p = 0.13$) impact of criminal law on recidivism. In column 2, we simultaneously estimate the specifications that yielded the highest explanatory power in the single equations. As a robustness check we also allowed for richer models in columns 3 and 4.

The influence of *criminal law* on recidivism is always negative and does not vary a lot across the different model specifications. The estimated coefficients lie in each other's confidence intervals yielding a very robust finding. The coefficients of the remaining covariates are mainly in line with the literature and intuition, which gives further support for the estimated models. The estimate for the correlation between the two equations (*rho*) is significant in columns 3 and 4 and has a p-value lower than 20% in the other two specifications. Given that the estimate of the correlation between the error terms is always positive, this parameter is also quite robust.

For the first equation, we find that *age* has a significant (negative) influence on expected recidivism confirming our initial assumption. The best model for *age* is a quadratic expression, resulting in a monotonously decreasing and convex function. The nonlinear curve thus captures a general negative trend and a decreasing marginal change, both of which are in line with the literature. Further, we find that the propensity to recidivate decreases when the inmate has a job offer or at least job contacts (*job contact*). The negative influence of job opportunity on recidivism confirms the literature which finds broad evidence that worse general job market conditions increase crime rates (Fougère, Kramarz, and Pouget, 2009; Lin, 2008; Machin and Meghir, 2004; Raphael and Winter-Ebmer, 2001). Further, we find criminal background of the parents (*crim parents*) and *open* prisons to be positively correlated with expected recidivism. When including dummy variables for the

Table 3: Biprobit Equation 1: Drivers of expected recidivism

	(1)	(2)	(3)	(4)
	recidivism	recidivism	recidivism	recidivism
age	-2.757**	-3.820***	-4.432***	-4.030***
	(0.017)	(0.001)	(0.000)	(0.000)
age2	0.0643**	0.0882***	0.1022***	0.0933***
	(0.022)	(0.002)	(0.000)	(0.000)
criminal law	**-1.183**	**-1.247****	**-1.336*****	**-1.370*****
	(0.131)	**(0.025)**	**(0.010)**	**(0.008)**
drugs deal		0.397***	0.3142***	0.315**
		(0.002)	(0.020)	(0.036)
job contact		-0.433***	-0.401**	-0.511***
		(0.000)	(0.014)	(0.001)
poor social capital		0.473*	0.486*	0.447
		(0.067)	(0.096)	(0.125)
robbery			0.067	
			(0.468)	
fraud			0.211	
			(0.649)	
theft			-0.267	-0.279
			(0.112)	(0.125)
open			0.474**	0.625**
			(0.096)	(0.049)
city				-0.369
				(0.240)
crim parents				0.472***
				(0.002)
Constant	29.02**	40.70***	47.32***	42.87***
	(0.016)	(0.001)	(0.000)	(0.000)
Observations	90	86	86	85

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

type of crime committed, only drug dealing (*drugs deal*) turns out to be a significant driver of expected recidivism.

Table 4: Biprobit Equation 2: Treatment Assignment

	(1)	(2)	(3)	(4)
	crim. law	crim. law	crim. law	crim. law
ageoffense	0.921***	1.238***	1.223***	1.088***
	(0.000)	(0.000)	(0.000)	(0.000)
poor social capital		1.075***	0.866*	0.899**
		(0.007)	(0.051)	(0.013)
robbery		-7.574***	-5.996***	-7.043***
		(0.000)	(0.000)	(0.000)
fraud		-0.560	-0.999	
		(0.299)	(0.154)	
female			2.377***	
			(0.000)	
abi			0.136	
			(0.742)	
vandal				-6.176***
				(0.000)
city				-0.848
				(0.203)
Constant	-19.48***	-26.04***	-25.73***	-22.95***
	(0.000)	(0.000)	(0.000)	(0.000)
rho	0.396	0.544	0.683**	0.621*
	(0.154)	(0.160)	(0.044)	(0.078)
Observations	90	86	86	85

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the treatment assignment equation, we control for types of crime assuming that the treatment decision could be influenced by the crime-specific ranges of punishments under the two different types of law. However, with the exception of vandalism we cannot confirm this hypothesis - the remaining dummies were dropped due to insignificance. Further, we also include offender characteristics which might proxy maturity. An obvious proxy for maturity is *ageoffense*, since it separates the two treatments and might be used as a moderator between the two regimes. This influence is confirmed in the data. In addition, we find *female* and *poor social capital* to be positively correlated with treatment assignment, while high school graduation (*abi*) and location (*city*) do not seem to affect the judge's decision.

To facilitate interpretation and comparison between the subsequent regression discontinuity design, we also report the average treatment effects. Following Christofides, Stengos, and Swidinsky (1997) and Greene (1998), the conditional means of a dummy variable are identical to the univariate probit case and can be computed as defined in (12) and (13). Hence, the average treatment effect can be computed as the average value of the individual changes in the likelihood of recidivism, induced by the treatment:

$$\begin{aligned} \text{ATE} &= \Pr(ER^1 = 1|X) - \Pr(ER^0 = 1|X) \\ \widehat{\text{ATE}} &= \tfrac{1}{N}\textstyle\sum_{i=1}^{N}\left[\Phi(X_i\widehat{\beta} + \widehat{\delta}) - \Phi(X_i\widehat{\beta})\right] \end{aligned} \tag{12}$$

where $\widehat{\delta}$ is the estimated coefficient of *criminal law* treatment and T_i is a dummy for treatment assignment. Further, the average treatment affect on those treated is

$$\begin{aligned} \text{ATET} &= \Pr(ER^1 = 1|X, T = 1) - \Pr(ER^0 = 1|X, T = 1) \\ \widehat{\text{ATET}} &= \tfrac{1}{N_T}\textstyle\sum_{i=1}^{N_T}\left[\Phi\left(\frac{X_i\widehat{\beta}+\widehat{\delta}-\widehat{\rho}W_i\widehat{\gamma}}{\sqrt{1-\widehat{\rho}^2}}\right) - \Phi\left(\frac{X_i\widehat{\beta}-\widehat{\rho}W_i\widehat{\gamma}}{\sqrt{1-\widehat{\rho}^2}}\right)\right] \end{aligned} \tag{13}$$

where the sum is restricted to all observations where treatment was received (N_T). The estimated treatment effects indicate a drop in recidivism of around 30%(ATE) and 40% (ATET). This clear result is robust across model specifications (see table 5).

Table 5: Average Treatment Effects Bivariate Probit

	(1)	(2)	(3)	(4)
ATE	-0.290	-0.288	-0.295	-0.302
ATET	-0.389	-0.394	-0.356	-0.424

6. Regression Discontinuity Design

In a second step, we check whether the results from the bivariate probit estimations can be confirmed in a regression discontinuity (RD) approach. Introduced by psychologists Thistlethwaite and Campbell (1960), RD did not draw too much of the attention in the economic literature until the late 1990s.[11] RD avoids the problem of a selection bias by taking advantage of a discontinuity in treatment assignment. Instead of differencing conditional means based on treatment status, here we contrast means based on a dummy variable that captures whether the individual has passed the cut-off point or not. Following Imbens and Lemieux (2008) we estimate the average treatment effect by

$$\begin{aligned} \widehat{ATE} \quad = E\left[\beta|(X_i = c)\right] \quad &= \frac{\lim_{x \downarrow c} E[ER_i|X_i = x] - \lim_{x \uparrow c} E[ER_i|X_i = x]}{\lim_{x \downarrow c} E[T_i|X_i = x] - \lim_{x \uparrow c} E[T_i|X_i = x]} \\ &= \frac{\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}}{\widehat{\alpha}_{Tr} - \widehat{\alpha}_{Tl}} \end{aligned} \tag{14}$$

where X_i is the variable *ageoffense* and c is the cut-off point where the treatment assignment function jumps. In our setting, the global treatment assignment function (7) suggests two potential discontinuities: at 18 and 21 years of age at offense. This means that we will compare individuals who are 18 (21) or a little older to their peers a little younger than 18 (21). The numerator of the estimator is the difference in limits of the value of the dependent variable at the cut-off point, approximated both from the left and the right. More intuitively, $\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}$ is the difference in the estimated intercepts when regressing estimated recidivism on age at offense, where the variable $ageoffense$ has been centered around the cut-off point: $\widehat{\alpha}_{ERr}$ is the intercept when taking into account only observations with an age above the cut-off and $\widehat{\alpha}_{ERl}$ is the intercept when using only those below the cut-off age. The same intuition holds for the denominator, which represents the differences in the limit of treatment probability from both sides of the cut-offs. These limits can be represented as the estimated intercepts $\widehat{\alpha}_{Tr}$ and $\widehat{\alpha}_{Tl}$, stemming from regressions of

[11] Today, however, there is a growing body of literature on RD applications initiated by Angrist and Lavy (1999) and Black (1999) amongst others. Lee and Lemieux (2010) provide a good survey on this emerging strand of the empirical literature.

the treatment indicator T on the centered variable $ageoffense$. Dividing by the difference in treatment probability can be seen as a normalization which yields the treatment effect if all subjects got the treatment.[12] This normalization is necessary, since in our "fuzzy" setting the jump in treatment probability is expected to be smaller than one at both cut-offs.

Underlying this identification strategy is the assumption that unobservable characteristics do not vary discontinuously at the cut-off points while treatment assignment does. Identification is possible when comparing only those individuals sufficiently close to the cut-off point (see Van der Klaauw (2008) for a formal derivation). Hence, the optimal bandwidth around the cut-off point needs to be sufficiently small, but needs to take into account that increased comparability comes at the price of decreased sample size. We calculate the optimal bandwidth according to Imbens and Kalyanaraman (2009) yielding a size of 2 years. In addition, we also apply different bandwidths to increase the robustness of the estimates.

6.1. Comparability of treatment and control group and self selection

To test for comparability of the sample on both sides of the cut-offs we contrast the observable characteristics. On table 6 we summarize the variables with significantly different means to the left and right of at least one cut-off point (at the 10% level).[13]

Looking at the treatment (*criminal law*), we see that there is no significant difference at the cut-off of 18. Even though judges can apply criminal law once the offender has turned 18 when committing the crime, our data show that they rarely do so. Looking at 21, however, we can reject the hypothesis of mean equality and do find a discontinuity in treatment assignment. We find a jump from around 25% just before 21, to 100% after 21.

[12] Note the similarity of this concept to a well-known "Wald" estimator in an instrumental variable approach. As was first pointed out by Hahn, Todd, and Van der Klaauw (2001), the property "having passed the cut-off point" can be interpreted as an instrument for treatment assignment. In this sense the denominator of (14) is the result of the first stage regression of *criminal law* on age at offense while in the numerator we have the second stage regression of expected recidivism on a list of variables including age at offense.

[13] Please refer to table A.10 in the appendix for a list with all variables.

Table 6: Covariates with significantly different means at 21 and 18

	21+	21-	bdw 2	18+	18-	bdw 2
N	54	51	105	52	45	97
age	25.07	22.25	2.83***	21.08	19.26	1.82***
	(0.31)	(0.2)	(0.37)	(0.2)	(0.18)	(0.28)
ageoffense	22.06	20.23	1.83***	19.03	17.19	1.85***
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)	(0.12)
female	0.24	0.04	0.2***	0.02	0.04	-0.03
	(0.06)	(0.03)	(0.07)	(0.02)	(0.03)	(0.04)
crim parents	0.06	0.16	-0.1*	0.18	0.19	-0.01
	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.08)
drugs deal	0.15	0.16	-0.01	0.21	0.09	0.12*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)	(0.07)
theft	0.37	0.39	-0.02	0.27	0.6	-0.33***
	(0.07)	(0.07)	(0.1)	(0.06)	(0.07)	(0.1)
bodily injury	0.17	0.39	-0.23***	0.46	0.49	-0.03
	(0.05)	(0.07)	(0.09)	(0.07)	(0.08)	(0.1)
vandal	0	0.16	-0.16***	0.1	0.2	-0.1
	(00)	(0.05)	(0.05)	(0.04)	(0.06)	(0.07)
criminal law	1	0.24	0.76***	0.04	0	0.04
	(00)	(0.06)	(0.06)	(0.03)	(00)	(0.03)

standard errors in parentheses, one sided test of mean equality
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Comparing the means of the other variables, our observations certainly differ in terms of age, which by itself is not a problem since the differences are rather small. Also, there seem to be more female inmates above 21 than below. A notable difference is in the type of crime committed. Here we find that younger individuals commit more "juvenile" crimes such as vandalism, theft and violent crimes. Drug dealing is committed more often by people above 18. For other crimes we do not find significant differences.

These differences in covariates might have some effect on recidivism. Therefore we will subsequently control for these and other variables in order to assure that the estimated effect on recidivism is driven by the actual treatment.

Summarizing, we do not find evidence for self selection based on observables. However, theoretically there might be perfect sorting based on unobservables which we cannot analyze. We do not see an argument that would justify self selection into treatment, since this would result in more severe punishment. There could, however, be the chance of sorting in the sense that juveniles bring forward the offense to a point in time when milder punishments will still be applied. To test this possibility, we check the distribution of observation around the cut-offs. If self selection were an issue, we should see a peak in density shortly before 18 and shortly before 21, since individuals would try to avoid the tougher punishment regime. However, this does not seem to be the case (see table 7). Furthermore, empirical evidence suggests that young offenders are myopic with respect to their punishment (see for example Lee and McCrary (2009)) giving further support for the view that we should not suffer from a problem of self selection.

Table 7: Observations RD bins

range ageoffense	17-18	18-19	19-20	20-21	21-22
# of observations	25	30	22	29	27

6.2. Estimated jumps in expected recidivism

The elements of (14) can be estimated either nonparametrically or local-linearly. In addition, further covariates might be included in the regressions. We apply the RD design using a nonparametric regression and allow for covariates. Looking at the data, the cut-off at 21 seems to have a much stronger appeal than the one at 18. A nonparametric approximation of treatment assignment shows a jump at 21 (of approx. 60 %) but no change at 18 (see figure 1).

Based on this observation, the theoretical change in treatment assignment at 18 is not an effective one. Hence, we focus on the second cut-off point at 21. In table 8, we provide estimates for the average treatment effect as defined in (14) using

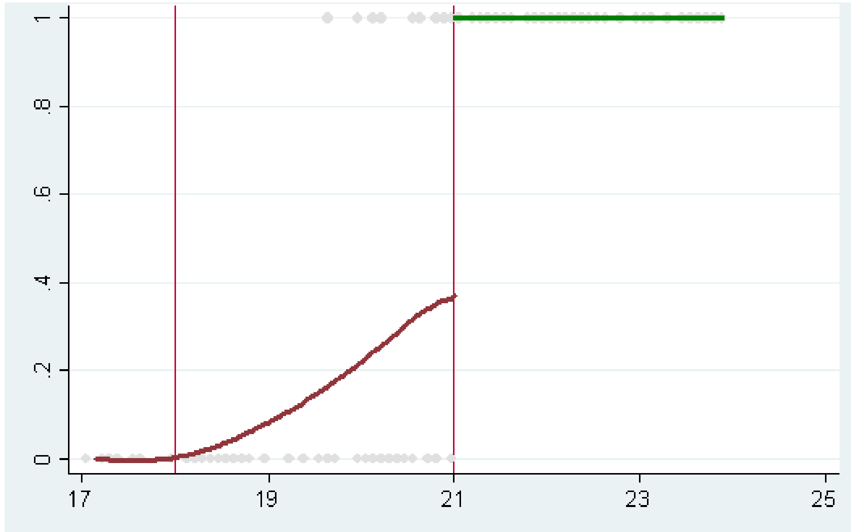

Figure 1: Treatment assignment over age at offense

different specifications and bandwidths. We see a drop in expected recidivism with a magnitude between 0.2 and 0.3, depending on the bandwidth. Our results show the magnitude of this change to be quite robust in the different specifications. For the smallest bandwidth the jump in recidivism is significant. Increasing the bandwidth reduces significance to a level of 12-13%. However, controlling for different covariates we get a better fit with decreased standard errors and find a significant jump. While the additional covariates affect the standard errors, the size of the estimates is only slightly changed. This gives an additional indication that our finding is due to the treatment change and not due to some selection bias. Also, including the covariates where we found significant differences in means does not change our results (see table 8, column 6). Dividing the jump in recidivism (diffER) by the jump in treatment assignment (diffT) serves as a normalization and provides the average treatment effects. The results are provided in table 8 and yield an estimated drop in recidivism of 0.36 to 0.56 if all delinquents got criminal treatment.

For the second cut-off (c=18, table 9) we did not find any significant jump in the treatment assignment function. Nevertheless, we check this cut-off point, suspecting an effect of the general regime change in the sense that the mere threat of adult law

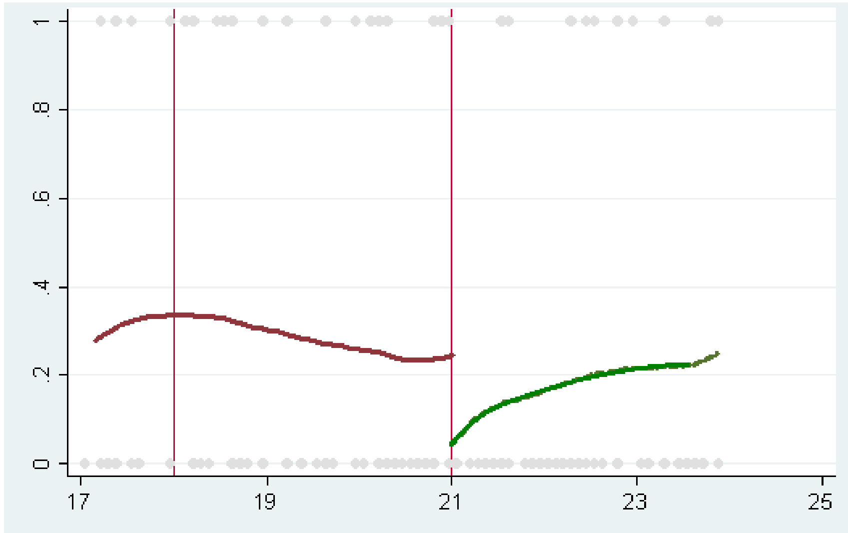

Figure 2: Expected recidivism over age at offense

results in a different effect of ths same (juvenile) punishment. The probability of being treated with the new legal regime jumps from zero to one when turning 18. Hence, the denominator becomes 1 and the treatment effect boils down to the Jump of *recid* ($\alpha_{ERr} - \alpha_{ERl}$). Without covariates, this jump is positive but not significantly different from zero. When we include further covariates the size of the jump does hardly change, but significance also stays at the same level. However, even if there were a jump it is much harder to attribute it to the different treatment because at 18 many things change that could harm our identification - for instance, people are also given a set of new rights which might influence recidivism independently from the new sanction regime. Therefore we can not really draw any conclusions from this finding.

Summarizing, we can not find any conclusive evidence at 18, but we find a significant drop at 21.

6.3. Robustness Check: Placebo estimates

Having found the drop at 21, we want to be sure that it was actually due to a causal effect of criminal law on recidivism and not due to other factors. We have

Table 8: RD estimates Part A Cut-off 21

21	(1) bdw=1	(2) bdw=2	(3) bdw=2.5	(4) bdw=2	(5) bdw=2	(6) bdw=2
N	50	102	131	102	100	100
bad_pr 21-(α_{ERr})	0.264	0.252	0.245	0.252	0.308	0.203
bad_pr 21+ (α_{ERl})	-0.038	0.034	0.048	0.018	-0.011	-0.031
diffER	-0.301*	-0.218	-0.197	-0.234*	-0.32*	-0.234**
	(0.051)	(0.126)	(0.135)	(0.063)	(0.052)	(0.042)
adult 21-(α_{Tr})	0.229	0.35	0.37	0.347	0.429	0.461
adult 21+(α_{Tl})	1	1	1	1	1	1
diffT	0.771	0.65	0.63	0.653	0.571	0.539
ATE	**-0.391***	**-0.335**	**-0.313**	**-0.359***	**-0.56***	**-0.435***
	(0.061)	(0.134)	(0.141)	(0.057)	(0.055)	(0.059)
age	no	no	no	no	yes	no
female	no	no	no	no	no	yes
city	no	no	no	yes	no	yes
bodily injury	no	no	no	no	no	yes
crim parents	no	no	no	no	yes	yes
drugs deal	no	no	no	yes	no	no
vandal	no	no	no	no	no	yes
poor social capital	no	no	no	yes	no	no
social contact	no	no	no	no	no	yes

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

partly checked this already by using different bandwidths and covariates, but will subsequently try to increase robustness of the estimation by performing placebo estimates.

Using the same specifications as above, we will try to estimate discontinuities in expected recidivism for cut-offs where no actual law change in terms of punishment arises. We will perform these placebo estimates every 6 months starting from 17 up to 22 and will thus run the 6 RD specifications described above, using the different bandwidths and covariates. If we find significant effects for some cut-offs except 18 and 21, this means that our RD results could be caused by the change in the regulation, but they could also be caused by spurious results due to some unobserved

Table 9: RD estimates Part B Cut-off 18

18	(1) bdw=1	(2) bdw=2	(3) bdw=2.5	(4) bdw=2	(5) bdw=2	(6) bdw=2
N	53	93	107	93	89	87
bad_pr 18-(α_{ERr})	0.337	0.319	0.318	0.299	0.34	0.398
bad_pr 18+ (α_{ERl})	0.473	0.495	0.459	0.421	0.495	0.515
ATE	0.136	0.175	0.141	0.122	0.156	0.117
	(0.31)	(0.35)	(0.40)	(0.58)	(0.444)	(0.579)
age	no	no	no	no	yes	no
female	no	no	no	no	no	yes
city	no	no	no	yes	no	yes
bodily injury	no	no	no	no	no	yes
crim parents	no	no	no	no	yes	yes
drugs deal	no	no	no	yes	no	no
vandal	no	no	no	no	no	yes
poor social capital	no	no	no	yes	no	no
social contact	no	no	no	no	no	yes

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

factors or biases. Since there is no law change at the placebo cut-offs, we will not divide by the change in treatment (the denominator of 14). We only look at the change in recidivism. The full estimates can be found in the appendix (Tables A.11 and A.12).

Looking at the results of our placebo estimates, we find that the cut-off at 21 has the highest level of significance in most specifications. The second highest significance can be found at the cut-off of 20, where we seem to have a positive jump. However, the estimates are only significant in 2 out of 6 specifications, compared to 21, which is significant in 4 out of 6. For all other placebos we do not find any significant jumps. Therefore, we can be confident about having found a causal effect of criminal treatment on recidivism.

7. Discussion

The main result of our analyses is that the application of criminal law does not stimulate juvenile recidivism, as suggested by many US studies, but rather decreases it. Based on the bivariate probit estimates, the treatment *criminal law* reduces recidivism by 30%, while the RD approach identifies a drop of about 40%. The results of both approaches are thus similar in sign and significance. It is possible that the small differences are due to different samples underlying the estimations: While in the bivariate probit model we look at adolescents only, the regression discontinuity design requires observations beyond the cut-off point (age 21). Hence, on average individuals in the latter analysis are much older. In addition, a regression discontinuity design gives more weight to the observations close to the cut-off point and thus only provides a weighted average treatment effect (Lee and Lemieux, 2010). Even though the deviation of the estimated effect from the average treatment effect cannot be identified, it is possible, given the results we have, that the effect is bigger for those close to the cut-off than for the rest of the population.

To what extent could the results be driven by a measurement error in the outcome variable? Continuing from the discussion in section 3.1, our proxy for recidivism might be subject to a bias. What could be the direction of such an effect? In juvenile prisons, there are more schooling possibilities and personal custodians. Along with general education also crime deterrence education might take place, leading to a temporary underestimation of the real rate of recidivism. In contrast, one might also think of stronger peer pressure in juvenile prisons which might lead to competition in toughness and an exaggerated report of recidivism. While the first case should lead to an underestimation of the treatment effect, the second case might result in an issue. However, if such a peer effect exists, it is likely to not only affect self-reported measures of recidivism but might also drive the real behavior after release (see Bayer, Hjalmarsson, and Pozen, 2009). Hence, we cannot find a convincing argument that would damage our results. Furthermore, due to the fact that we find so few individuals who consider themselves certain to reoffend (only 4% in our sample), an exaggerated report of recidivism is unlikely to be the case.

7.1. Further robustness checks

In addition to the presented results, we performed several robustness checks which are briefly summarized in this subsection. First, we also estimated a bivariate ordered probit version of the model. The extension of the described specification is straightforward. The results confirm the estimates, increasing the robustness of our findings.

Second, we conjected that juvenile law might affect expected recidivism differently depending on whether it is still applicable when the inmate is released from prison. One way to test this hypothesis is to check whether there is an additional effect when the "age when leaving" supersedes 21. If the inmate can expect to leave prison after turning 21, he can be sure that criminal law will be applied in case of reoffending. This could result in a different probability of recidivism when compared to a subject that leaves prison before turning 21 (the same logic applies at 18). We tested for this possibility by including both "age when leaving" and a dummy if this age was smaller than 21. However, the regressors were almost never significant and did not change our estimates of the causal effect of criminal law on recidivism. This might be due to the fact that we are mainly analyzing adolescents and thus most of them are already older than 21 when leaving prison (average leaving age is 23.5 years). In addition, there is a lot of uncertainty with regard to the actual point in time when the inmate leaves the prison since the German penal code includes the possibility of early release (see §§ 57, 57a, 57b Strafgesetzbuch).

7.2. Reconciliation with US findings

Furthermore, the question arises why our results are so different from the US evidence. Looking more closely, even though the results are in stark contrast to the literature on juveniles transferred to criminal courts, there is also evidence from the US which finds reduced juvenile recidivism after stricter sanctions. Hjalmarsson (2009) shows that incarceration in juvenile facilities can be an effective measure in combating juvenile crime as opposed to even milder punishments such as a probation or a fine. She argues that, in the case of the US American juvenile prisons she analyzes, the deterrent effect seems to outweigh the drawbacks of incarceration, in particular its stigma and potential peer effects. A similar argument might hold for German criminal prisons when compared to juvenile prisons, where the net effect of a harsher environment seems to be that criminal behavior on the part of adolescent inmates is discouraged.

Combining the results with the reported effects of tougher US transfer laws could also suggest, at least for adolescents, a U-shaped pattern of the relationship between harshness of punishment and recidivism. Keeping this picture in mind, German prisons seem to be to the left of the minimum point - and thus incarceration in harsher criminal prisons results in reduced recidivism. US criminal prisons, on the other hand, seem to be to the right of the minimum already - and thus more harshness increases recidivism. The results from Chen and Shapiro (2007) lend further support to this hypothesis by showing that increased harshness in US criminal prisons is likely to result in increased recidivism. This explanation would point to generally stricter sanctions in the US when compared to Germany (or Europe in general) - a view which seems to find support in the literature. As Whitman (2003) writes in the introduction to his book on the difference between the legal systems in the two continents, "criminal punishment in America is harsh and degrading - more so than anywhere else in the liberal west." Based on this assessment, in the US system adolescents are punished more severely in general, especially after ending up in criminal prison, and therefore might not be able to reintegrate into society after such an experience. In contrast, the German system is rather mild and sees incarceration as the "ultima ratio", especially for juveniles.

A second explanation might be found in the different age groups we are analyzing. While US transfer laws usually refer to 16 or 17-year-old offenders, we base our analysis on individuals older than 18. Hence, the relative gains from harsh sanctions might increase with age, which could be explained by the limited deterrent effects for (myopic) adolescents found by Lee and McCrary (2009).

A third driver of criminal behavior is peer effects. As reported by Bayer, Hjalmarsson, and Pozen (2009), incarceration can enforce subsequent criminal behavior, especially for individuals with similar crime types. The difference in results might thus be caused by stronger peer effects in German juvenile prisons when compared to their US counterparts. However, even though the German characterization of incarceration as "ultima ratio" might lead to a more negative selection of the "toughest guys", we do not see why peer pressure should be stronger than in the US.

8. Conclusion

In this paper, we have analyzed the impact of sanction type on inmates' expectations of their subsequent criminal behavior. To overcome the identified bias due to the selection process into criminal law, we first used a bivariate probit model that provides an unbiased estimate of the treatment coefficient, given that the model is correctly specified. In a second step, we exploited the fact that in Germany there are two potential jumps in the probability of being sentenced under criminal law. By taking advantage of the discontinuity at the age of 21, we isolated the causal impact of criminal law on expected recidivism in a regression discontinuity design.

The results from both approaches suggest that being sentenced under criminal law discourages young people from recidivism. This finding is in stark contrast to the literature on US transfer laws and shows that the legal framework in Germany seems to be substantially different from its North American counterpart.

In terms of policy recommendations, our results tend to suggest that it might be fruitful to widen the application of criminal law. The group of adolescents is exactly the group for whom recommendation Rec(2008)11 "European Rules for Juvenile Offenders Subject to Sanctions and Measures" suggests an extended application of juvenile law. Our results question the optimality of this policy.

One last remark is that Germany should catch up with the English-speaking countries in terms of data gathering and should collect data from inmates on a regular basis. In this way, researchers could obtain even more conclusive results, enabling them to provide more robust policy advice.

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Appendix A. Appendix

Table A.10: Covariates mean comparison at 21 and 18

	21+	21-	bdw 2	18+	18-	bdw 2
age	25.07	22.25	2.83***	21.08	19.26	1.82***
	(0.31)	(0.2)	(0.37)	(0.2)	(0.18)	(0.28)
ageoffense	22.06	20.23	1.83***	19.03	17.19	1.85***
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)	(0.12)
female	0.24	0.04	0.2***	0.02	0.04	-0.03
	(0.06)	(0.03)	(0.07)	(0.02)	(0.03)	(0.04)
city	0.27	0.16	0.11	0.06	0.16	-0.1
	(0.06)	(0.05)	(0.08)	(0.03)	(0.06)	(0.06)
married	0.17	0.12	0.05	0.08	0	0.08
	(0.05)	(0.05)	(0.07)	(0.04)	(00)	(0.04)
social contact	0.48	0.59	-0.11	0.58	0.37	0.2
	(0.07)	(0.07)	(0.1)	(0.07)	(0.07)	(0.1)
poor social capital	0.57	0.49	0.08	0.37	0.49	-0.12
	(0.07)	(0.07)	(0.1)	(0.07)	(0.08)	(0.1)
addiction	0.38	0.27	0.11	0.31	0.27	0.04
	(0.07)	(0.06)	(0.09)	(0.06)	(0.07)	(0.09)
crim parents	0.06	0.16	-0.1*	0.18	0.19	-0.01
	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.08)
job contact	0.43	0.5	-0.07	0.48	0.57	-0.09
	(0.07)	(0.07)	(0.1)	(0.07)	(0.08)	(0.11)
abi	0.04	0.04	0	0.04	0	0.04
	(0.03)	(0.03)	(0.04)	(0.03)	(00)	(0.03)
drugs deal	0.15	0.16	-0.01	0.21	0.09	0.12*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)	(0.07)
drugs consume	0.15	0.1	0.05	0.08	0.04	0.03
	(0.05)	(0.04)	(0.06)	(0.04)	(0.03)	(0.05)

Table A.10: (continued)

	21+	21-	bdw 2	18+	18-	bdw 2
theft	0.37	0.39	-0.02	0.27	0.6	-0.33***
	(0.07)	(0.07)	(0.1)	(0.06)	(0.07)	(0.1)
robbery	0.19	0.31	-0.13	0.33	0.47	-0.14
	(0.05)	(0.07)	(0.08)	(0.07)	(0.08)	(0.1)
fraud	0.19	0.2	-0.01	0.21	0.11	0.1
	(0.05)	(0.06)	(0.08)	(0.06)	(0.05)	(0.08)
bodily injury	0.17	0.39	-0.23***	0.46	0.49	-0.03
	(0.05)	(0.07)	(0.09)	(0.07)	(0.08)	(0.1)
vandal	0	0.16	-0.16***	0.1	0.2	-0.1
	(00)	(0.05)	(0.05)	(0.04)	(0.06)	(0.07)
sexual	0.11	0.04	0.07	0	0.04	-0.04
	(0.04)	(0.03)	(0.05)	(00)	(0.03)	(0.03)
murder	0.13	0.1	0.03	0.15	0.13	0.02
	(0.05)	(0.04)	(0.06)	(0.05)	(0.05)	(0.07)
open	0.17	0.1	0.07	0.13	0.16	-0.02
	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.07)
criminal law	1	0.24	0.76***	0.04	0	0.04
	(00)	(0.06)	(0.06)	(0.03)	(00)	(0.03)

standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one sided test of mean equality)

Table A.11: Placebo estimates (1)

	(1)	(2)	(3)	(4)	(5)	(6)
17	-0.109	-0.091	-0.045	-0.118	-0.071	0.006
	(0.636)	(0.647)	(0.814)	(0.454)	(0.732)	(0.968)
N	*43*	*80*	*89*	*80*	*77*	*75*
17.5	0.119	0.125	0.162	0.098	0.169	0.155
	(0.746)	(0.577)	(0.413)	(0.64)	(0.491)	(0.304)
N	*50*	*85*	*101*	*85*	*82*	*80*
18	0.136	0.175	0.141	0.122	0.156	0.117
	(0.613)	(0.396)	(0.449)	(0.525)	(0.444)	(0.579)
N	*53*	*93*	*107*	*93*	*89*	*87*
18.5	0.241	0.073	0.053	0.108	0.064	0.15
	(0.449)	(0.719)	(0.777)	(0.578)	(0.749)	(0.409)
N	*49*	*96*	*122*	*96*	*92*	*90*
19	0.157	-0.071	-0.092	-0.154	-0.058	-0.048
	(0.626)	(0.73)	(0.614)	(0.436)	(0.772)	(0.802)
N	*50*	*103*	*126*	*103*	*100*	*99*

RD estimates of ATE, columns represent model specifications as in table 8 and 9
p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Placebo estimates (2)

	(1)	(2)	(3)	(4)	(5)	(6)
19.5	-0.056	0.026	-0.002	0.059	0.041	0.072
	(0.81)	(0.89)	(0.991)	(0.732)	(0.82)	(0.673)
N	*46*	*101*	*129*	*101*	*99*	*99*
20	0.463	0.305	0.265	0.32	0.343*	0.278*
	(0.102)	(0.11)	(0.122)	(0.076)	(0.05)	(0.134)
N	*50*	*105*	*130*	*105*	*104*	*104*
20.5	-0.288	-0.179	-0.144	-0.265	-0.298	-0.158
	(0.224)	(0.32)	(0.374)	(0.118)	(0.076)	(0.324)
N	*52*	*105*	*131*	*105*	*103*	*103*
21	-0.301*	-0.218	-0.197	-0.234*	-0.32*	-0.234**
	(0.051)	(0.126)	(0.135)	(0.063)	(0.052)	(0.042)
N	*55*	*102*	*131*	*102*	*100*	*100*
21.5	0.322	0.163	0.144	0.132	0.145	0.098
	(0.041)	(0.22)	(0.239)	(0.337)	(0.318)	(0.48)
N	*59*	*107*	*130*	*107*	*105*	*105*
22	-0.06	0.154	0.14	0.224	0.143	0.242
	(0.64)	(0.274)	(0.294)	(0.123)	(0.35)	(0.104)
N	*52*	*109*	*138*	*109*	*108*	*108*
22.5	0.234	0.213	0.202	0.255	0.207	0.231
	(0.291)	(0.216)	(0.195)	(0.119)	(0.239)	(0.16)
N	*55*	*116*	*135*	*116*	*115*	*115*

RD estimates of ATE, columns represent model specifications as in table 8 and 9
p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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