"ECONOMICS OF CULTURE, INSTITUTIONS, AND CRIME"

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Juvenile Law and Recidivism

A Discontinuity-Based Analysis

Stefan Pichler and Daniel Römer

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Abstract

This paper analyzes the influence of juvenile law on (expected) recidivism rates using a data set with 1150 inmates from 31 German prisons. We first analyze this dataset using standard econometric models and identify several social and socioeconomic factors of expected recidivism, like job expectation, education, social networks, gender and age. Further we perform a regression discontinuity analysis in order to identify the effect of criminal law on juvenile offenders. The hypothesis that criminal law increases recidivism cannot be confirmed. Rather, our results suggest that the juridic framework adolescents are faced with reduces recidivism in Germany.

JEL classification: K42, K14, C21, C14

Note: This study uses Data collected in an inmate survey in 2003/2004 as part of the research project "Cost and Benefit of incarceration and deterrence" („Kosten und Nutzen von Haft und Haftvermeidung“) financially supported by VolkswagenStiftung. Originator of the project was Prof. Dr. Horst Entorf, TU Darmstadt, Chair for Applied Econometrics. Both the originator and his affiliation do not bear any responsibility for the use of the data in this study.

1 Introduction

"The history of criminal justice clearly shows cruel punishments have been superseded by milder ones. The advance from brute to more human, from simple to more diversified forms of punishments has proceeded while now future paths emerge that need yet to be followed." BVerfGE 45, S. 187, 229.

Crime has been a major problem in all economic societies throughout time. Over the centuries different methods for punishment have been applied. 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 choice behavior. According to this approach, which goes back to Becker (1968), it is individually rational to commit crime if illegal income opportunities outreach the legal ones. This gives rise to two potentially conflicting goals. On the one hand, legislation should result in preferably severe punishments increasing the expected costs of crime and thus general deterrence. On the other hand, in order to increase specific deterrence, i. e. the likelihood that offenders will not recidivate, delinquents should have preferably good chances to enter the legal job market after having served their sentence. Hence, crime legislation should not only be seen in the light of its effect on general deterrence, as e. g. in Levitt (1998) or Katz, Levitt, and Shustorovich (2003), but should also aim at minimizing recidivism.

The role of recidivism is especially relevant for young delinquents. Firstly, it seems to be especially fruitful to put them back on the "right track". Secondly, the literature on personal development found that youths are generally more inclined towards criminal activity (e. g. Heinz (2004); Thornberry, Huizinga, and Loeber (2004)), which can be explained by a contemporary maturity gap (Moffitt 1993). This leads to the belief that juveniles are more rehabilitatable and less culpable then adults (Mears, Hay, Gertz, and Mancini 2007). As a consequence, there are special treatments for juvenile offenders in many countries.[1] However, due to a rise in juvenile crime in the 1990s, there has been decreasing public support for a preferential treatment of juvenile offenders which led to tougher transfer laws (Moon, Sundt, Cullen, and Wright 2000). In the last years, this topic has also seen increasing attention in Germany (Pruin 2006). Since 2007, pictures of violent juveniles have become much more present in the media which resulted in a public call for a tougher legislation and led both politics (Bundestag 2009) and academia (Heinz 2008) to touch upon the question of how to deal with juvenile offenders.

Empirically, there is ambiguous evidence, whether the special treatment of young people is an efficient measure (see Redding (2005) for an overview over the literature). Levitt (1998) found evidence that general deterrence can be increased by stricter punishments for juveniles. However, other studies have found no deterrence effect (Singer and McDowall 1988); (Steiner, Hemmens, and Bell 2006) or even increased arrest rates (Jensen and Metsger 1994).

In this paper we do not want to add to the debate on general deterrence, but rather try to explore the effect of juvenile law on recidivism. For the US, the majority of the empirical evidence suggests a positive effect of transfers to criminal courts on recidivism. Taking advantage of the fact that in New Jersey young delinquents were sentenced by a juvenile court while in New York they were charged by a criminal court, Fagan (1996) studied differences in recidivism rates of 15- and 16-year-old juveniles. He found significantly lower recidivism rates for those sentenced by juvenile courts suggesting that the special jurisprudence for juvenile crimes is an effective measure. This study has been replicated with more control variables by Kupchik, Fagan, and Liberman (2003), increasing

[1] In the US, the first juvenile courts were established as early as 1899, in Germany in 1908.

the robustness of the results.

Bishop, Frazier, Lanza-Kaduce, and Winner (1996) analyzed recidivism in Florida where some delinqents are transfered, depending on the decision of the prosecutor. They found higher recidivism rates for those delinquents transferred to criminal courts. An obvious objection is the possibility of a selection bias that drives the results. For example one could imagine that young delinquents end up in a criminal court because their recidivism rates are believed to be higher. However, in a follow-up study Lanza-Kaduce, Lane, Bishop, and Frazier (2005) still found a positive effect of transfers when using a richer dataset and matching techniques in order to rule out a selection bias. Further studies by Myers (2003b), Podkopacz and Feld (1995) and Thornberry, Huizinga, and Loeber (2004) point into the same direction.

From an argumentative point of view it might seem to be counterintuitive that transfers to criminal courts should decrease specific deterrence. However, there are different mediators that provide some intuition for this result. Chen and Shapiro (2007) reports that too harsh prison conditions might stimulate aggressive and criminal thinking and thus increase recidivism. There is also evidence that worse job opportunities imply a higher rate of recidivism (Entorf 2008). Both factors are found to be more present when sentenced by criminal courts: Myers (2003a) finds that juveniles transferred to adult court were treated more harshly than those retained in juvenile court. Also, criminal sanctions signal higher or more violent criminal activity as opposed to juvenile sanctions and therefore a person being sentenced under criminal law will have most probably worse job possibilities. Further, the perceived fairness of the sentence is inversely correlated with recidivism (Corrado, Cohen, Glackman, and Odgers 2003). Following this line of thought offenders could interpret the transfer to a criminal court as unfair since there are other delinquents not being transferred.

So far, there is no empirical evidence whether the negative effect of criminal law on juveniles' recidivism found in the US is also valid for Germany. Results might not be transferrable since German juvenile legislation is somewhat different to the US. In Germany, all minors, i. e. persons under the age of 18, are sentenced under juvenile law leading to less severe punishments, like corrective methods, arrest or juvenile prison.[2] After coming of age, juveniles enter the phase of adolescence that lasts until they turn 21. Depending on the severity of the crime, adolescent delinquents can be sentenced according to either juvenile or criminal law. Hence, there is no special court for juveniles.

Looking at German survey data, the hypothesis that juvenile law decreases recidivism seems not to hold. In 2003, the German Ministry of Justice published a report on recidivism. Within four years after their release from prison, 35.7% have again been legally registered. After being sentenced under juvenile law, recidivism rates are almost twice as high (59.3%) as those being sentenced under

[2] It is also possible that young delinquents serve their sentence in a separate part of a criminal prison.

criminal law (32.6%). The recidivism rate of those who spent time in juvenile jail according to §§ 45, 47 JGG amounts to 40.3% and is thus closer to but still exceeding the overall average rate. Does that mean that juvenile law has failed?

Of course, descriptive statistics do not allow for causal interpretation and inference. Additionally, it might not be the sanction type that makes them recidivate, but personal characteristics, since adolescent people commit crime at a higher frequency as old ones (Heinz 2004). This suffices Heinz to conclude that the higher recidivism rate following juvenile law sanctions is "probably due to age rather than sanction". However, there is little empirical evidence in favour or against this educated guess.

In this paper we test the hypothesis that the application of criminal law increases adolescent delinquents' recidivism using a unique micro data set containing 1,150 inmates in 31 German prisons. We identify factors influencing expected recidivism, controlling for the applied type of law. For a simple inclusion of a law type dummy is not appropriate when age influences both law type and recidivism rates, we perform a regression discontinuity analysis. By analyzing only people who in terms of age are very similar to each other we can hope to avoid the age bias and measure the real effect.

While at the age of 21 we cannot identify a significant treatment affect, we identify a drop in recidivism at the age of 18. According to a fuzzy regression design approach, this would be interpreted as an effect of the increased fraction of individuals sentenced under criminal law. However, our analysis also identifies another important point. It is not only the applied law which matters, but also the choice possibilities. When controlling for the applied law type, we find that the drop in recidivism is actually driven by both groups and not only those inmates who are sentenced under juvenile law. For the first group this drop in recidivism can be explained by the specific deterrence effect of criminal punishment. For those being sentenced under juveniles law deterrence cannot explain the change in expected behaviour since the punishment is identical to those who have not turned 18 yet. Our results thus suggest that there is a second, treatment effect coming from the threat of the potential application of criminal law.

This article makes several contributions to the existing literature. We add to the empirical evidence explaining recidivism of inmates. In contrast to Fagan (1996) we look at adolescents instead of minors and in addition find a more differentiated result. Furthermore we contribute to the - recently growing - applied econometric literature on regression discontinuity analyses. Finally, our research uses, as opposed to big parts of mainstream literature, German prison data. This is an interesting field, since German prison conditions are very different compared to those in the Anglo-Saxon world and hence the external validity of findings in these countries cannot be taken as granted.

The remainder of the paper is organized as follows. Section 2 describes the database, section 3 analyzes the data, first using simple regressions and then a regression discontinuity analysis. We perform robustness checks and discuss our findings in section 4. Section 5 concludes.

2 Data

This analysis is based on a prison survey of 1,771 inmates. The survey uses a two-stage approach that combined stratified and random sampling. Firstly, a representative sample of the population of prisons in Germany was created. Secondly, a random draw out of this population completed the sampling.

The survey data was collected in 31 German prisons in 2003 and 2004 using a questionnaire with 123 questions. In order to account for the different nationalities of the inmates, the questionnaire was distributed in either German, Turkish, Serbo-Croatian, Russian, Polish or English language (see Meyer (2007) for more details on the survey). The survey was organized and performed by a team of researchers from Darmstadt University of Technology. It was initiated and first analyzed by Entorf (2008).

A possible weakness of the data is its low response rate: only 13.3% of the 13,340 questionnaires came back answered. Interviews with judicial employees suggest an overrepresentation of the more active group of inmates. However, when comparing the sample characteristics to those of the average prison population in Germany, there is no evidence of a selection bias (Entorf 2008). In addition, we checked the resondants' averge share of their sentence being served, which might be a proxy for activity. On average, respondants had served 56% of their sentence length when the survey was conducted. Hence, even though we cannot rule out a selection bias, we do not find any sign hinting to its existence.

The convicts of the 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 intetersted in the effect of the applied law type, we only use the last two subgroups and drop the inmates in pretrial custody. This leaves us with a subsample of 1,150 inmates. The descriptive statistics can be found in table 1.

Our target variable is expected recidivism. It is constructed from the response to the following survey question:

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

The person was asked to answer this question on a 5 point scale, whereat a 1 stands for "no, never" and 5 corresponds to "absolutely certain". For reasons of small sample size Entorf (2008) bundled categories 1 and 2 as well as categories 3 to 5 to get a binary variable "likely recidivism". From our point of view, this simplification is too strong, since bundling could leave out some interesting information.[3] We will thus use the original variable.

[3] For example, it is difficult to claim that a person answering this question with 3 has the same expectation as somebody who put 5.

Of course, one might raise objections against using expected recidivism as a proxy for real recidivism. There are at least three arguments in favour of our approach. First, there is evidence that expected 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, there is no influence from additional factors that happen after the release from prison and that thus can hardly be controlled for.

Additionally to the variables directly collected in the survey, we construct two further variables. *Percseated* defines the percentage of the sentence length that the individual already had served when surveyed. *Ageoffence* is the age of the inmate when committing the crime and thus determines the applicable law. Since this characteristic did not appear in the survey directly, we constructed it using time and age when surveyed and the time when the crime was committed. Combining these different pieces of infromation, we could identify the approximate age of the inmate at the time he committed the crime. Due to different precision levels of the relevant points in time the calculated age at offence is quite noisy. The age when surveyed is only given in 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 offence offence[4]. The missing precision of this variable might make our regression discontinuity analysis inaccurate, since *ageoffence* is the variable that is crucial for the applied type of law. As a consequence, we dropped contradicting observations, i. e. where *ageoffence* and the applied law type did not match given German legistlation.[5]

Further control variables refer to the gender of the respondent (*female*) and the crimes committed (*crimrec*). Also, the crime type is controlled for, since it is likely determine the probability of recivism. For organized crime and drug dealing for example there is a high chance of relapse due to physical addicition or the influence of the social network. In the case of drug addictions there is also an increased general probability of committing crimes, e. g. under the influence of drugs or for financing drug habits (Entorf and Winker 2008).

Also, variables on social contacts are used to explain expected recidivism: e. g. martial status. Another interesting variable is schooling background which is included based on the expectation that a person who invested in her human capital and engages in crime, will hardly be able to use this education at criminal activities and thus this investment would be a sunk cost. Since a previous criminal record might serve as a negative signal on the job market, this variable is included by corresponding interaction variables. If we look at the interaction of school and crimes we see that less than 10% of the inmates

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

[5] Some seemingly contradictive entries could be explained by § 32 JGG, which states that sentences for several crimes can only be uniformly sentenced according to either juvenile or adult law. Furthermore, also in adult prisons there are potentially delinquents sentenced by juvenile law due to capacity constraints in juvenile jail. We kept all observations where inconsistency might be driven by these explanations and dropped all other inconsistent entries.

with at least high school education are involved in drug dealing, whereas for the whole population this percentage is much higher.

We also include variables measuring job expectations directly. In our sample, only 40 % of the inmates have a job or a job contact at the time of the survey. Looking at the interaction of this variable with various crimes, we see more or less the same percentage throughout all crimes. Further, the descriptive statistics show that roughly 20% of the adolescents were sentenced under criminal law. This share is consistent with the reported quota in Germany and thus our sample once more seems to be representative.

3 Identifying the effects of Juvenile Law on Expected Recidivism

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, we need to identify the corresponding treatment effect. Define ER_i as a measure of expected recidivism and $T_i \in \{0,1\}$ as the treatment indicator of inmate i, we can write:

$$ER_i = (1-T_i)ER_i^0(x) + T_i ER_i^1(x) = \begin{cases} ER_i^0(x) & if \quad T_i = 0 \\ ER_i^1(x) & if \quad T_i = 1 \end{cases}$$

where $ER_i^0(x)$ is expected recidivism when juvenile law has been applied, while $ER_i^1(x)$ is expected recidivism when criminal law has been applied. Both expressions are a function of a list of observables (x). According to German legislation, juvenile law is mandatory to all minors, i. e. to all persons who have not turned 18 yet at the time the criminal act was commited. For adolescent delinquents, i. e. those with at least 18 but less than 21 years of age at offence, the legislator left the decision to the courts whether to apply juvenile or criminal law. Finally delinquents of at least 21 years have to be sentenced under criminal law. Given this legislation, we can specify the treatment assignment function as:

$$T_i(ageoffense) = \begin{cases} 0 & if & ageoffense < 18 \\ g(\cdot) & if & 18 \leq ageoffense < 21 \\ 1 & if & ageoffense > 21 \end{cases} \quad (1)$$

The treatment assignment function $g(\cdot)$ can take values between 0 and 1. In order to identify the treatment effect, the treatment indicator must not be endogenous. In the present case there are two possible sources of an endogeneity problem. First, the treatment assignment rule obviously depends on the age at offense. If *ageoffense* is correlated with ER_i identification is not possible. The same holds true if $g(\cdot)$ is determined by observables that also explain ER_i.

Hence, we first check whether age at offense influences expected recidivism and then try to identify the arguments of $g(\cdot)$.

3.1 Drivers of expected recidivism

Here, we will perform a regression analysis to see if *ageoffense* influences expected recidivism. Since the dependent variable might only take integer values from one to five, a simple OLS regression might not be the best approach. In order to justify OLS we would have to assume that the differences between the categories are the same which seems to be quite strong given the survey question on which expected recidivism is based. The same assumption is implicitly made later when we estimate the model nonparametrically. More appropriate models to estimate this variable are Ordered Probit and Ordered Logit. The weakness of these two models, however, is that the estimated coefficients cannot be interpreted straightforward since one would need to compute the marginal effects for each outcome. Therefore we will estimate both OLS and the ordered discrete-choice models and base the interpretation on the OLS estimates given the results of the different estimation methods are approximately the same.

In order not to underspecify the model, we start out with a very rich model and then test down until only the significant variables remain. Please find the results on Table 2. Most importantly, we find that *ageoffense* has a significant influence on expected recidivism. Hence we have to reject any approach that measures the treatment effect by a simple dummy variable for the application of criminal law. In the remainder of this chapter we will elaborate all other factors that explain expected recidivism in our sample, since we will have to make sure that these variables are controlled for in the subsequent analyses. We should note that Entorf (2008) analyzed the dataset already by bundling the dependent variable. So we can use his results as a benchmark and check whether our analysis replicates his findings.

As Entorf, we find that the propensity to recidivate decreases if the inmate has a job offer or at least job contacts (*job_yes_contact*). A related question of the survey asks the respondents to estimate their individual probability of getting a job (*jobexpgood*). When including the latter it becomes highly significant and *job_yes_contact* loses all its significance. There are two possible ways to deal with this finding. On the one hand, one could stick to *jobexpgood* since it seems to have a higher degree of explanatory power. On the other hand, one could suspect that expected recidivism and job expectation are driven by some (possibly unobserved) factor which has not been included in the regression. Then we should drop job expectation due to an endogeneity problem. Since both variables are individual expectations, a natural canditate is the general mood of the inmates or a measure highly correlated with either general or current mood. However, when including different mood variables, none of them was significant. Hence, we do not see support for the unobserved factor assumption. Therefore, we include *jobexpgood* as the variable with the higher explanatory power.

As in Entorf, gender of the inmate (*female*) has a serious influence in the sense that women seem to be more confident to not return to prison than men. Also confirming Entorf's results, we find drug dealing or consumption (*drugs*) to increase the expected probability of the inmate to reoffend. However, in our sample schooling has no influence. While Entorf finds contact with the partner to be highly significant, we only find that martial status significantly affects expected recidivism. Since our sample does not include inmates in pretrial custody, this discrepancy can be explained by the fact that contact with the partner has a strong influence only in the pretrial custody, but is less influential after the criminal is convicted[6].

Summing up, including additional regressors and using an ordered discrete-choice model our results are consistent with Entorf (2008). In addition to his results, we identified additional variables of significant influence. One is the extent to which inmates have already served their sentence (*percseated*). This variable is highly significant and positive. The positive influence is somewhat surprising, meaning that persons who are just about to leave the prison think that they have a higher probability of returning than somebody who just came. It seems that the effect of the punishment is of a short term nature and decreases as inmates get used to life in prison. This result seems to be consistent with Robinson and Darley (2004) who summarize the psychological literature on punishment, finding that offenders gradually become desensitized to incarceration. A third variable which was not used in Entorf's analysis but is highly significant in our sample is the criminal record of the person (*crimrec*). Somebody with a high criminal record is very likely to commit a crime again. Also here our findings are in line with theory.

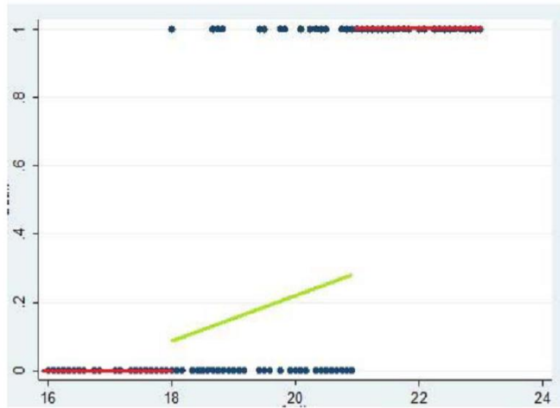
3.2 Drivers of treatment assignment for adolescents

Next, we want to estimate the function $g(\cdot)$ which assigns treatment to the inmates aged between 18 and 21 at the time of offense (N=101). The assumption of random treatment assignment is highly questionable given that judges choose the punishmnet, hence we try to identify characteristics of the inmates that determine the application of juvenile law. An obvious candidate is the age at offense (*ageoffense*), since adolescence seems to serve as a phase of transition from juvenile to criminal law as applicable jurisdiction. In addition, we tested for several further characteristics of the inmate. Estimating a probit and a logit model we found *ageoffense* and high school graduation (*abi*) to be positively and robbery (*raub*) to be negatively correlated with treatment assignment. There is some intuition for the influence of robbery on treatment assignment, since it might be rated a rather soft offense as compared to e. g. grievous bodily harm. A higher probability of criminal law for high school graduates is somewhat surprising. Possibly, high school graduates might be judged to be more mature as other offenders of the same age. This seems to influence judges in their decision, even though such a judgment might not be justified.[7] However, other factors that are known to influence recidivism, like the criminal record or the offender's drug consumption, have no influence on the applied law type. Both findings

[6] We verified this claim with the data from persons in pretrial custody.

[7] Heinke (2009) finds that understanding the consequences of crime is not influenced by school type.

Figure1: *Probability of Criminal Treatment*



Notes: This Figure shows the jump in the assignment probability of criminal treatment depending on the age at offense. If this age is below 18 juvenile legislation applies. If the age is above 21 criminal legislation applies. Between 18 and 21 the criminal can be sanctioned with either

shed some interesting light on the decision schemes of judges in Germany and at the same time raise the question whether this assignment rule is optimal.

For our further analysis it is important to see that there is a significant discontinuity in the treatment assignment function with respect to *ageoffense*, at the beginning of the interval and also at its end (see Figure1). This allows for a regression discontinuity analysis which follows in the next chapter.

3.3 Regression Discontinuity Design

Whenever the treatment assignment is determined at least in parts by the value of an observed variable which also influences the outcome variable, the differences approach is inappropriate and cannot identify the treatment effect (see Van der Klaauw (2008) for a formal derivation). In this case, a Regression Discontinuity (RD) design is an appropriate alternative to randomized experiments. Introduced by Thistlethwaite and Campbell (1960), this approach did not draw too much of the attention in the economic literature until the late 1990s. However, today, there is a growing body of literature on RD applications which was initiated by Angrist and Lavy (1999) and Black (1999) amongst others.

Whereas in experiments with randomized assignment, experimenters can take care of comparability between treatment group and control group, assignment in an RD design is nonrandom and persons receiving treatment differ systematically from those who do not. Therefore, an RD analysis requires some knowledge on the assignment rule $g(\cdot)$ that determines how persons are assigned to treatment. More specifically, it requires some cutoff point (c) where the function that assigns the treatment probability is assumed to have a discontinuity.

Following Imbens and Lemieux (2008) we can estimate the average treatment τ effect by

$$\widehat{\tau_{RD}} = E\left[\beta|(X_i = c)\right] = \frac{\lim_{x\downarrow c} E[ER_i|X_i=x] - \lim_{x\uparrow c} E[ER_i|X_i=x]}{\lim_{x\downarrow c} E[T_i|X_i=x] - \lim_{x\uparrow c} E[T_i|X_i=x]}$$

where X_i is the variable *ageoffense*. The numerator of this ratio is the difference in limits of the value of the dependent variable at the cutoffs (18 and 21) approximated both from the left and the right.The denominator represents the differences in limit at the same cutoffs, however here the values of treatment probability are approximated.

These values can be estimated either by nonparametric estimation or by local linear estimation. In order to get the best estimates at the cutoff points, we try to use only observations close to it. Later we will calculate the optimal bandwith, i. e. how closely we apporach the cutoff points. For now we turn our attention to a bandwith of two years, which gives a balanced picture of sample size and homogenity within the sample. The resulting sample size of the three relevant subgroups is rather small: there are only 48 individuals in the group of 16- and 17-year-old inmates, 101 individuals in the group of 18- to 20-year-old inmates and 59 in the group of 21- and 22-year-old inmates.

As a prerequisite for the regression design approach, we need to assume that to the left and to the right of the cutoff points individuals are identical with respect to all regressors except for treatment assignment. We cannot test the assumption of comparability for all relevant variables, because there still might be some unobservables which we cannot analyze. However, we can compare the observable characteristics of the three groups. Here, we focus on variables which we found to have a significant influence on our output variable. Thus, we need to test whether the 3 groups do not differ with regard to gender (*female*), criminal record (*crimrec*), percentage of duty in jail fulfilled (*percseated*), job expectation (*jobexpectation*), alcohol/drug abuse (*alc_ drug*) and drug dealing as crime (*crimdrug*) and whether or not the individuals are married (*verh*). Analyzing the means of the explanatory variables (Table 4) we find the following differences: In our sample there are no female or married persons below 18. Furthermore drug dealers on average are older than 21 resulting in significant differences of individuals in the two groups with respect to this variable. Furthermore, at the 10 percent significance level, individuals below and above 21 also differ in *percseated* and *crimrec*. We will first estimate our regression ignoring these facts and then estimate the same model a second time with an adjusted sample in order to compensate the sample differences.

3.4 Fuzzy RD design

When the jump in the treatment assignment function is not from 0 to 1, a fuzzy RD design can be applied. We thus can estimate the treatment effect in the way described above:

$$\widehat{\tau_{FRD}} = E\left[\beta|(X_i = c)\right] = \frac{\lim_{x\downarrow c} E[ER_i|X_i=x] - \lim_{x\uparrow c} E[ER_i|X_i=x]}{\lim_{x\downarrow c} E[T_i|X_i=x] - \lim_{x\uparrow c} E[T_i|X_i=x]} = \frac{\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}}{\widehat{\alpha}_{Tr} - \widehat{\alpha}_{Tl}}$$

More intuitively, $\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}$ is the difference in the estimated intercepts, when regressing esimated recidivism on age: $\widehat{\alpha}_{ERr}$ is the intercept when taking into account only observations with an age above the cutoff and $\widehat{\alpha}_{ERl}$ is the intercept when using only those below the cutoff age. The same intuition holds for the regression of the treatment indicator T on age and the estimated intercepts $\widehat{\alpha}_{Tr}$ and $\widehat{\alpha}_{Tl}$. For the estimation of these variables either a nonparametric regression at the boundary or a local linear regression can be used.

Independent of the estimation technique, we need to make sure that the inmates on both sides of the cutoff are identical up to the treatment. Since the above t-test (Table 4) did show a significant difference in the sample for some explanatory variables the analysis will be twofold. On the one hand we will use the whole sample, on the other hand we will adjust our sample for the observed differences.

3.4.1 Nonparametric Estimation

First we will perform a nonparametric regression. The optimal bandwidths (h) of the nonparametric regressions are chosen in a way to minimize the squared residuals. In ranked order they correspond to 1.1 years, 1.3 years and 0.5 years respectively. Please find the results in Table 5. The fuzzy design is defined in a way that the estimated jump is weighted by the change in probability. Since the difference in probability must be smaller than 1, this always increases the measured treatment effect even bigger. In order to ensure that significance does not depend solely on this artificial adjustment we calculate the standard errors both for the jump ($\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}$) and the jump adjusted by the probability ($\widehat{\tau_{FRD}}$).

The first thing to note is that the smaller the bandwith the smaller the sample size and the bigger the effect, but also the bigger the noise. Looking at the non-adjusted sample we see a drop at 18. The magnitude of the drop depends on the bandwith used. For the bandwith of 0.5 decreases by 1.5, while for the other two bandwidths it is around 0.3. However, none of these is significant. This changes when looking at the adjusted sample. Now, the magnitude increases substantially and the reduction is significant for all bandwiths. Deviding this change by the change in probability we get an estimated treatment effect of around 2 for all bandwidths. This means that the expected recidivism drops by two categories, i. e. somebody who was certain to end up in prison again and thus selected 5, expects only a small chance of recidivism and choose 3 if getting the treatment.

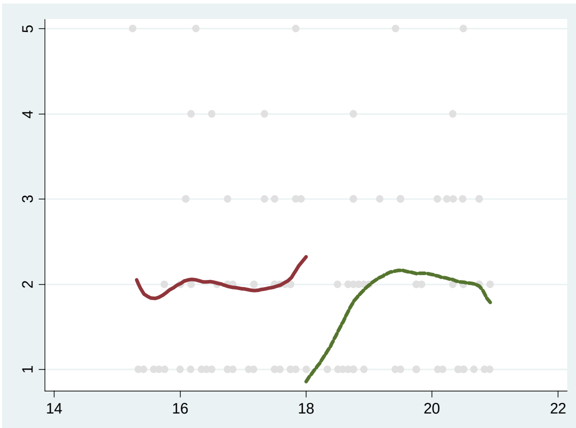
The results at the second cutoff are smaller and less easy to interpret. Using the unadjusted sample we estimate an increase at all bandwiths. The increment is slightly significant, however weighting it by the change in probability and thus adding additional noise the significance vanishes. When adjusting the sample we still observe an upturn of the same magnitude, however the significance goes down even more.

Summing up, the results suggest that at the first cutoff (18) expected recidivism drops. However, at the second cutoff (21) we have the opposite picture

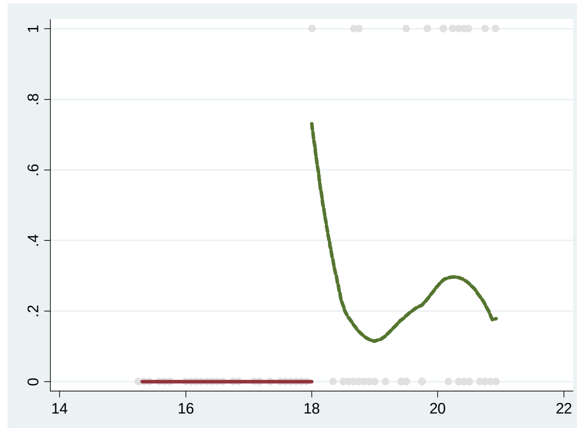
Figure2: *Nonparametric estimation*

Panel A: Cutoff 18

Change of expected recidivism

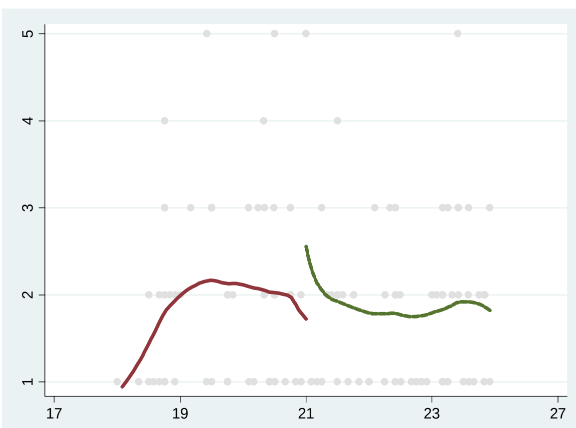


Change in Probability of Treatment

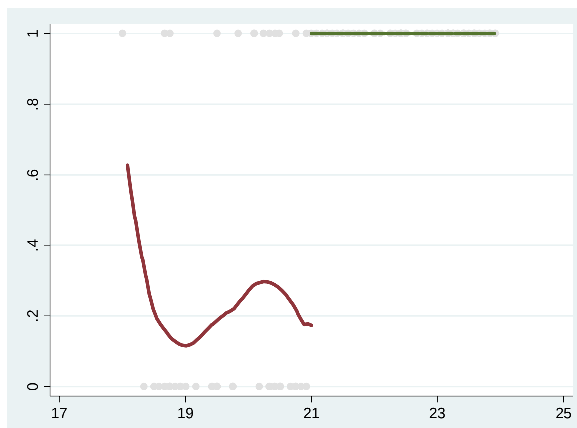


Panel B: Cutoff 21

Change of expected recidivism



Change in Probability of Treatment



Notes: This figures show the estimated Graphs of Expected Recidivism and the corresponding change in assignment of the treatment with age at offense as explanatory variable. A nonparametric jump at the two cutoffs is estimated for the bandwidth of 1.1 years. The analysis of the graph can be found in Table 5 (Bandwidth 1.1 Adjusted Sample). We take into account that samples are not comparable in all respects and thus limit the sample to nonnmarried, nondrugdealing man.

and see a positive influence even if the effect itself is smaller and less significant.

3.4.2 Parametric Estimation

Dropping the assumption of equal differences among categories we estimate a parametric model with the standard techniques for ordered categories. We will thus estimate the same variables using the following equations

$$\begin{aligned}
\widehat{\alpha}_{ERl} &= \arg\min \textstyle\sum_{i:c-h\leq X_i\leq c} \left(ER_i - \alpha_i - \beta_i(X_i - c)\right)^2 \\
\widehat{\alpha}_{ERr} &= \arg\min \textstyle\sum_{i:c\leq X_i\leq c+h} \left(ER_i - \alpha_i - \beta_i(X_i - c)\right)^2 \\
\widehat{\alpha}_{Tl} &= \arg\min \textstyle\sum_{i:c-h\leq X_i\leq c} \left(T_i - \alpha_i - \beta_i(X_i - c)\right)^2 \\
\widehat{\alpha}_{Tr} &= \arg\min \textstyle\sum_{i:c\leq X_i\leq c+h} \left(T_i - \alpha_i - \beta_i(X_i - c)\right)^2
\end{aligned}$$

Again, we analyze both cutoffs separately and estimate two effects $\widehat{\tau}_{FRD}$, for c=18 and for c=21. The lower case h, stands for the bandwith. This is set to 1.1, the optimal bandwith estimated non-parametrically. Please find the results in Table 6. The estimated effects confirm the results from the nonparametric estimations. We find a positive effect at the age of 18, in the sense that recidivism plummets, and a negative effect at 21. The effect at 18 is not significant, when looking at the unadjusted sample. Also the effect at 21 is not significant for two out of three methods and only the Ordered Logit estimation is significant. Significance at c=18 increases, both in magnitude and in significance once we drop noncomparable persons from our sample. For c=21 after adjusting the dataset all estimates become insignificant.

Thus, it seems that certainty about the treatment, such as juvenile law for all delenqents below 18 and criminal law for those aged above 21, is generally discouraging and thus increasing expected recidivism. Uncertainty about the treatment on the other hand seems to decrease expected recidivism.

Summing up we found the following phenomena:

- At the first cutoff, when for the first possibly criminal law is applicable, there is a drop of expected recidivism. The fuzzy RD analysis suggests that this drop is driven by the (few) delinquents who were sentenced under criminal law.
- At the second cutoff we find almost no change.

3.5 Robustness checks

Since the regression discontinuity design does not allow for the inclusion of further regressors, we adjusted our sample to have sufficiently homogeneous inmates in treatment and control group. Nevertheless, one could suspect, that these adjustments did not account for all relevant heterogeneity in inmate characteristics. Hence, it would be interesting to compare the results from the previous chapter to a regression analysis where all significant factors are controlled for. Adopting the idea of the regression discontinuity design and the boarder case analysis (as in Chen and Shapiro (2007)), we try to capture a discontinuity in expected recidivism by adding dummies for individuals in a small neighborhood around the cutoff. Since there are two jumps in treatment probability, we can use both in order to identify the treatment effect on expected recidivism. Hence, we construct three age category dummies: one for those slightly to the left to 18 (*ageoffense18*$^-$), one for all those between the two cutoffs (*ageoffense1821*) and one for those slightly to the right right of 21 (*ageoffense21*$^+$). A significant jump in the estimated value of the dummies identifies a discontinuity in the target variable and hence a treatment effect. By controlling for other factors in the regression we can relax the assumption that the groups to the left and right of the cutoff are similar. However what we need for this analysis is the assumption that recidivism does not depend (or just slightly depend) on age itself. Including the age as a regressor and not including the dummies we get a negative coefficient (Table 2 Equation 1) we get an estimate of age of -0.01 which can be considered as very flat.

We thus estimate the following equation:

$$y_i = \alpha_i + \beta_1 ageoffense18^- + \beta_2 ageoffense1821 + \beta_3 ageoffense21^+ + \gamma_i R_i$$

where R_i are other regressors $ageoffense18^-$ is a dummy which is one when age is smaller (within a small neighborhood) than 18, $ageoffense1821$ equals one for the interval 18-21 in age at offence and $ageoffense21^+$ will equal one for age bigger (within a small neighborhood) than 21. Since the variable $ageoffense$ has - as we saw above - very little influence on expected recidivism, what we are estimating is infact the influence of the assignment function T_i which is defined in equation 1 in Section 3:

$$y_i = \alpha_i + \beta_1(T_i = 0) + \beta_2(T_i = g(\cdot)) + \beta_3(T_i = 1) + \gamma_i R_i$$

where now T_i is the treatment function described above[8]. So we can estimate the effect of the treatment. Weighting the estimated effect by the actual change in assignment probability (the change in T_i) we can approximate what would happen if the treatment was assigned to all candidates and the assignment probability changed form 0 to 1.

In search of the optimal bandwidth for this regression analysis, we start with very big time intervals and then vary the interval size. The bandwidth with the highest level of significance for the dummies is two years, resulting in dummies for inmates at the age of 16 and 17 (*ageoffense1617*), at the age of 18 to 20 (*ageoffense1820*) and those at the age of 21 and 12 (*ageoffense2122*). Even

[8] Note that by estimating this equation we will have no threat of collinearity since we will only use a small neighborhood around the cutoffs for the dummy and not all datapoints. Furthermore we will include one further dummy for the region where the age is below this neighboorhood in order that the constant does not capture both T=0 and T=1.

with this bandwidth, not all dummies reach a reasonable level of significance, which might be due to the small number of observations due to the small age categories.

When comparing neighboring subgroups, i. e. persons to the right and left of the two cutoffs 18 and 21, we try to replicate the fuzzy RD approach used above. ((Here, we also include the group of adolescents, where some got the treatment and others did not.)) In our dataset within this interval 80.7% of the people were treated as juvenile criminals and thus got a milder penalty, while 19.3% got the regular criminal penalty. In order to estimate the effect of the treatment we follow the idea of a fuzzy RD deviding the difference of the age category dummies by the actual change in assignment probability. In doing so we estimate what would happen if the treatment was assigned to all candidates and the assignment probability changed form 0 to 1. Comparing the estimates of *ageoffence1617* to *ageoffence1820* the difference is 0.2. Dividing by the change in the probability, which changes from 0 to 19.3 %, we get an estimate of the treatment effect of approximately -1.0. We can apply the same procedure to the cutoff at 21, where the treatment probability changes from 19.3% to 100%. Here the difference is much smaller and adjusted with the change in probability the estimated treatment effect is -0.09. The same holds true for the analysis with Ordered Logit (2) and Ordered Probit (3). Also here the first jump is much bigger than the second one.

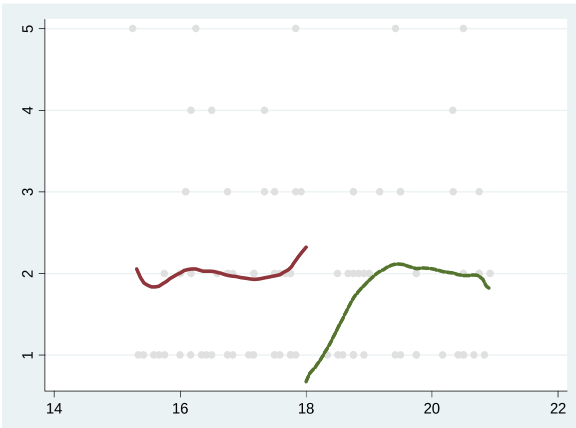
Comparing these results with what we got above we see that there is again a drop in expected recidivism at 18, however probably due to the bandwidth and the additional regressors the jump here is smaller than the one estimated both parametrically and nonparametrically. At the second cutoff point (21) the effect is now even negative. Above we had a small slightly positive effect, having a very low level of significance. Comparing these two results we have to conclude that we cannot really say anything about what happens when offenders turn 21 because the effect is so small.

Summing up, we estimate a big negative effect of criminal law on expected recidivism, when age at offence is around 18 and only a very small treatment effect at the age of 21. However, the estimates are based on dummy differences whose level of significance level is not very high. Nonetheless they point into the same direction as the results from the previous chapter thereby confirming them.

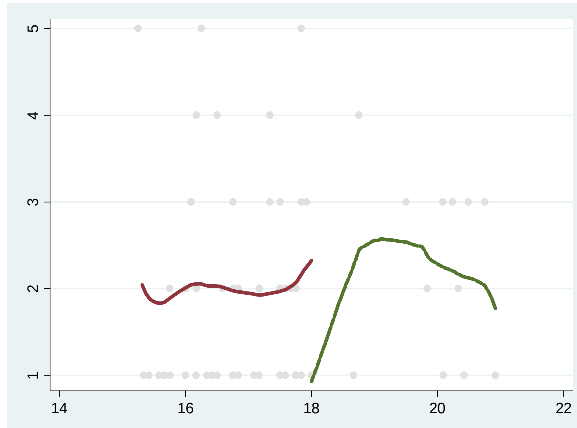
Figure3: *Nonparametric estimation*

Panel A: Cutoff 18 Change of Expected Recidivism

Punished with juvenile legislation

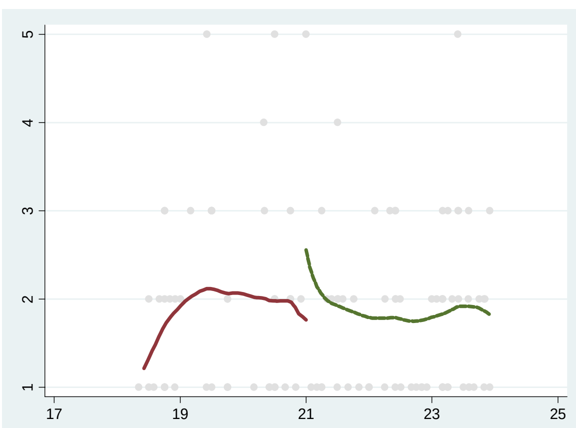


Punished with criminal legislation

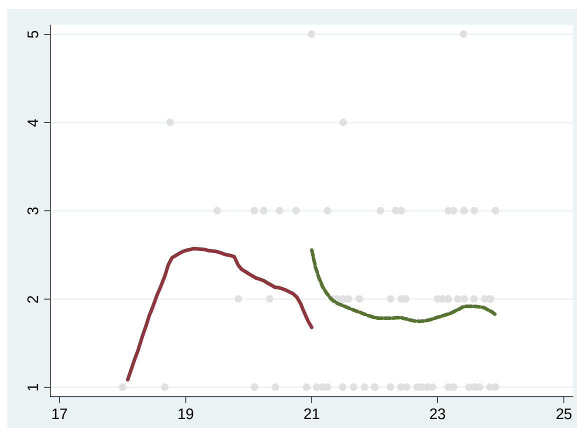


Panel B: Cutoff 21

Punished with juvenile legislation



Punished with criminal legislation



Notes: This figures show the estimated Graphs of Expected Recidivism with age at offense as explanatory variable. It separates the two samples of criminals convicted under juvenile legislation and criminals convicted after crmiminal legislation between the age of 18 and 20 (to the left of 18 juvenile law always applies and to the right criminal law always applies). A nonparametric jump at the two cutoffs is estimated for the bandwidth of 1.1 years. The analysis of the graph can be found in Table 7 (Bandwidth 1.1 Adjusted Sample). We take into account that samples are not comparable in all respects and thus limit the sample to nonnmarried, nondrugdealing man.

3.6 The threat of criminal law

Having found the drop in expected recidivism, we try to further understand what caused this drop. In order to do this, we propose an additional analysis in this chapter. The question we need to ask now is whether the drop at the age of 18 identified in the fuzzy RD is really driven by those sentenced under criminal law as suggested by the standard crude interpretation. In order to answer this question we analyze the offenders treated with juvenile law and criminal law seperately.

Since we found different results at the two cutoff points, we came up with the idea of a possibile second treatment that effects recidivism. We can think of the possibility that solely the threat of criminal law changes the influence of juvenile law on young delinquents. Then, the threat of criminal law would imply an additional (sharp) discontinuity at the age of 18, when the potential appliance of criminal law changes from 0 to 1.

Testing the hypothesis that the lower recidivism rate is driven by the delinquents sent to criminal prisons, we find that both groups show a level of expected recidivism that is lower than in the pooled group left of the cutoff point (see Table 9, Figure 3).

At second glance, this result is not counterintuitive. Adolescents sentenced under juvenile law might feel thankful for not being sentenced by criminal prison and thus strengthen their wish to return to a life in legality. This is in line with the influence of fairness on recidivism (Corrado, Cohen, Glackman, and Odgers 2003).

The inmates sentenced under criminal law, might show specific deterrence effects due to their more severe punishment.

Going further, one might even expect a second discontinuity at the age of 21, since younger delinquents might perceive criminal law as an especially tough minority treatment while it is mandatory for those older than 21. However, the effect at the second cutoff (21) is smaller and less significant.

4 Conclusion

With this paper, we add some empirical evidence to the literature on the influence of social and socioeconomic factors on expected recidivism. We find job expectation, education and social networks to be negatively correlated with expected recidivism.

Furthermore, we have analyzed the impact of sanction type on inmates' expectations on their subsequent criminal behavior. We exploited the fact that in Germany there are two jumps in the probability of being sentenced under criminal law at the age of 18 and 21 respectively. By taking advantage of these discontinuities we attempted to isolate the causal impact of criminal law on expected recidivism. Our findings suggest that the possibility of being sentenced under criminal law encourages young people not to recidivate. Therefore, German legislation has installed a quite interesting mechanism by introducing a chance of being sentenced under criminal law while not making it apply very often.

In terms of policy recommendations, the adjustable form of punishment seems to work well, since there is a decrease in expected recidivism exactly at the point, where the possibility of the application of criminal law is introduced. This drop is observed for both groups and thus the system seems to be efficient in reducing recidivism. Our results suggest that it might be fruitful to decrease the age limit for the potential application of criminal law. However, it is important to remember that the RD approach is only valid at the cutoff point and thus cannot be transferred to the whole population.

Concluding, German legislation and judges seem to do a good job at reducing expected recidivism for young offenders. Nonetheless, there should be further attempts to study the behavior of adolescent delinquents in Germany and to identify similar effects in other countries.

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

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Table 1 *Descriptive Statistics*

Variable	Obs	Mean	Std. Dev.	Min	Max
recid Expected Recidivism 1=never 5=definitely	1150	1.812174	1.017441	1	5
Ageoffense Age when commiting the crime	1005	32.07993	11.0112	12.24794	65.50274
yinJail Time already spent in jail	976	2.400814	3.209057	0.082192	23.43014
ytotal Length of sentence	1050	4.141532	3.905201	0.083333	41
percseated yinJail/ytotal	976	0.596088	0.321802	0.017933	1
Crim Dummy: Sentenced based on criminal law	1150	0.846957	0.360186	0	1
female Dummy: Female	1150	0.108696	0.311392	0	1
Married Dummy: Married	1150	0.169565	0.375414	0	1
crimrec How many times convicted already	1150	4.26913	5.199665	0	34
alc_drug Dummy: Alcohol or drug addiction	1150	0.285217	0.451714	0	1
jobexpgood Dummy: Good Job expectation	1150	0.535652	0.498944	0	1
Crimdrug Dummy: Crime=Drug dealing	1150	0.175652	0.38069	0	1
raub Dummy: Crime=Robbery	1150	0.145217	0.352473	0	1
abi Dummy: Has completed highschool 12-13y of schooling	1150	0.075652	0.264556	0	1

Notes: This table shows the different descriptive statistics. Entries are validated if the entry of expected recidivism shows an entry. Dummy variables are set equal to 1 if expressions are true, 0 otherwise. Zeros might also containg missing values. Interaction variables are a multiplication of already existing variables.

Table 2 *Drivers of expected recidvism*

	OLS (1)	Ordered Logit (2)	Ordered Probit (3)
Ageoffense *Age at offense*	-0.01*** (0.00)	-0.02*** (0.01)	-0.01*** (0.00)
Female *Dummy Female*	-0.30*** (0.09)	-0.84*** (0.22)	-0.50*** (0.13)
Crimrec *Criminal Record*	0.04*** (0.01)	0.08*** (0.01)	0.05*** (0.01)
Percseated *% of time in jail*	0.30*** (0.09)	0.69*** (0.20)	0.41*** (0.12)
Jobexpgood *Good job exp.*	-0.46*** (0.06)	-0.88*** (0.13)	-0.54*** (0.08)
alc_drug *Alk/drug dep.*	0.30*** (0.07)	0.66*** (0.14)	0.38*** (0.08)
Crimdrug *Crime=Drug*	0.27*** (0.08)	0.70*** (0.16)	0.37*** (0.10)
Married *Dummy married*	-0.11 (0.08)	-0.32* (0.18)	-0.19* (0.10)
_cons	1.97*** (0.13)		
cut1		-0.38 (0.27)	-0.26 (0.16)
cut2		1.11*** (0.27)	0.62*** (0.16)
cut3		2.92*** (0.30)	1.60*** (0.17)
cut4		3.39*** (0.32)	1.82*** (0.17)
N	993	993	993
adj. R^2	0.18		

Standard errors in parentheses * p<.10, ** p<.05, *** p<.01

Note: This table shows the factors that influence Expected Recidivism. The dependent variable is categorical with classes from 1(no recidivism) to 5(sure recidivism). Because of the nature of the dependent variable the model is estimated with OLS (1), Ordered Logit (2) and Ordered Probit (3).

Table 3 *What influences criminal legislation treatment*

Dep. Variable: Criminal Dummy: Sentenced under criminal law	OLS (1)	Logit (2)	Probit (3)
ageoffense Age when commiting the crime	0.08* (0.05)	0.55* (0.31)	0.28 (0.17)
abi Dummy: Has completed highschool 12-13y of schooling	0.38** (0.18)	1.83* (1.05)	1.10* (0.64)
raub Dummy: Crime=Robbery	-0.16* (0.08)	-1.24* (0.68)	-0.63* (0.35)
_cons	-1.38 (0.90)		
cut1		11.92* (6.24)	6.14* (3.43)
N	101	101	101
adj. R^2	0.09		

Standard errors in parentheses * p<.10, ** p<.05, *** p<.01

Notes: In this table the factors that influence the probability of criminal legislation treatment are shown.

Table 4 *Comparison of the 3 different groups*

Group	1	2	3	1vs3			1vs2			2vs3		
Sample	**16-17**	**18-20**	**21-22**	*Diff (D)*	St. Error	Pval D=0	*Diff (D)*	St. Error	Pval D=0	*Diff (D)*	St. Error	Pval D=0
Observations	**48**	**101**	**63**									
Female	**0**	**0.108911**	**0.190476**	*0.190476*	0.057196	*0.0012*	*0.108911*	0.04527	*0.0174*	*0.081565*	0.055723	0.1452
Crimrec	**3.447917**	**4.346535**	**3.015873**	*0.432044*	0.554672	0.4377	*0.898618*	0.792704	0.2588	*1.330662*	0.725229	*0.0684*
Percseated	**0.558574**	**0.540771**	**0.627436**	*0.068862*	0.05977	0.2518	*0.017804*	0.052008	0.7326	*0.086666*	0.049965	*0.0847*
jobexpgood	**0.541667**	**0.633663**	**0.52381**	*0.017857*	0.096462	0.8535	*0.091997*	0.085985	0.2864	*0.109854*	0.078932	0.1659
alc_drug	**0.291667**	**0.316832**	**0.269841**	*0.021825*	0.086715	0.8018	*0.025165*	0.081511	0.758	*0.04699*	0.073844	0.5254
Crimdrug	**0.083333**	**0.277228**	**0.190476**	*0.107143*	0.067128	0.1134	*0.193894*	0.070695	*0.0069*	*0.086752*	0.069031	0.2107
married	**0**	**0.089109**	**0.174603**	*0.174603*	0.055295	*0.0021*	*0.089109*	0.041401	*0.033*	*0.085494*	0.052429	0.1049

Notes: This table shows a sample T-Test which compares the means of the 3 different subgroups and analyzes theter or not the differences (in absolute value) are statisstically significant. It tests the assumption of mean independence for the dependent observable variables that showed to have influence on expected recidivism. Those are the gender, the criminal record the percentage of punsihment endured, the jobexpectation, their drug addiction, wheter or not their crime was related to drugs and wheter or not the candidates were married. As we can see nobody in the first group is either married or female. Whereas there are some individuals for the second and third group. Looking at a 5 percent significance only the only variable showing problems beside gender and marriage is drug as crime.

Table 5 *Nonparametric Estimates*

Panel A: Cutoff 18

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwid	20	29	43	64	51	73
Ageoffense<18 (α_{ERr})	3.689	3.689	2.323	2.268	2.282	2.282
Ageoffense>18 (α_{ERl})	1.000	2.108	0.860	1.934	0.837	1.941
Difference	-2.689**	-1.581	-1.463**	-0.334	-1.445**	-0.341
	(1.243)	(1.145)	(0.700)	(0.521)	(0.651)	(0.473)
Prob. Of treatment<18 (α_{Tr})	0.000	0.000	0.000	0.000	0.000	0.000
Prob. Of treatment>18 (α_{Tl})	1.000	0.257	0.731	0.137	0.694	0.136
FRD effect (τ)	-2.589*	-5.762	-2.001**	-2.437	-2.081**	-2.503
	(1.688)	(5.885)	(0.981)	(4.377)	(0.925)	(4.029)

Panel B: Cutoff 21

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwid	23	34	44	74	52	87
Ageoffense<21 (α_{ERr})	1.246	1.173	1.720	1.591	1.826	1.666
Ageoffense>21 (α_{ERl})	3.448	3.306	2.555	2.565	2.446	2.482
Difference	2.202	2.133**	0.835	0.974*	0.62	0.816*
	(1.407)	(0.995)	(0.732)	(0.549)	(0.627)	(0.485)
Prob. Of treatment<21 (α_{Tr})	0.563	0.672	0.173	0.340	0.162	0.338
Prob. Of treatment>21 (α_{Tl})	1.000	1.000	1.000	1.000	1.000	1.000
FRD effect (τ)	4.810	6.414	1.009	1.476	0.736	1.230
	(5.152)	(7.322)	(0.900)	(0.917)	(0.752)	(0.777)

Standard errors in parentheses * p<.1, ** p<.05, *** p<0.01

Notes: This table represents the estimated effects of the treatment "criminal legislation" on recidivism. A nonparametric jump at the two cutoffs is estimated for different bandwidths. Applying a Fuzzy like design we weight the estimated jump by the jump in probability of treatment. The Adj. takes into account that samples are not comparable in all respects and thus limits the sample to nonnmarried, nondrugdealing man.

Table 6 P*arametric Estimates*

Panel A: Cutoff 18

Dependent Variable Expected Recidivism	OLS		Ordered Logit		Ordered Probit	
	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size (Bandwidth 1.1)	43	64	43	64	43	64
Ageoffense<18 (α_{ERr})	2.263	2.298	1.150	0.992	0.620	0.554
Ageoffense>18 (α_{ERl})	0.893	2.024	4.289	1.063	2.629	0.597
Difference	1.37** (0.685)	-0.274 (0.526)	-3.139** (1.318)	-0.071 (0.698)	-2.009** (0.927)	-0.043 (0.863)
Prob. Of treatment<18 (α_{Tr})	0.000	0.000	0.000	0.000	0.000	0.000
Prob. Of treatment>18 (α_{Tl})	0.615	0.128	0.615	0.128	0.615	0.128
FRD effect (τ)	-2.229* (1.180)	-2.141 (4.582)	-5.1034*** (1.656)	-0.556 (5.470)	-3.267** (1.588)	-0.339 (6.743)

Panel B: Cutoff 21

Dependent Variable Expected Recidivism	OLS		Ordered Logit		Ordered Probit	
	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size (Bandwidth 1.1)	44	74	44	74	44	74
Ageoffense<21 (α_{ERr})	1.825	1.728	1.753	2.090	1.011	1.180
Ageoffense>21 (α_{ERl})	2.283	2.362	1.123	0.600	0.598	0.391
Difference	0.458 (0.689)	0.634 (0.521)	0.630 (0.749)	1.49** (0.661)	0.413 (0.968)	0.789 (0.740)
Prob. Of treatment<21 (α_{Tr})	0.455	0.340	0.455	0.340	0.455	0.340
Prob. Of treatment>21 (α_{Tl})	1.000	1.000	1.000	1.000	1.000	1.000
FRD effect (τ)	0.480 (0.719)	0.961 (0.831)	0.660 (0.818)	2.257** (1.115)	0.435 (1.046)	1.195 (1.142)

Standard errors in parentheses * p<.1, ** p<.05, *** p<0.01

Notes: This table represents the estimated effects of the treatment "criminal legislation". The probabilities are calculated by regressing a regression on the dummy youth (criminal=a+b*(Age-Cutoff), where cutoffs are 18 and 21). The coefficients represent the constant estimating recidivism=a+b*(Age-Cutoff). In the ordered probit(OPROB) and ordered logit(OLOG) the constant is the average distance among the cuts*. Applying a Fuzzy like design we weight the estimated jump by the jump in probability of treatment. The Adj. takes into account that samples are not comparable in all respects and thus limits the sample to nonnmarried, nondrugdealing man.

* In order to avoid confusion we denote as cutoffs the ages of 18 and 21, where the probability of being convicted with adult law changes. We further denote as cuts the 4 cuts calculated by stata in the Ordered Probit and Logit models.

Table 7 *Estimating the effects of criminal legislation treatment*

Dep. Variable: Expected Recidivism *5 class categorical variable*	OLS (1)	Ordered Probit (2)	Ordered Logit (3)
Ageoffense15 *Age dummy 15 or less y*	0.23 (0.25)	0.21 (0.31)	0.16 (0.59)
Ageoffense1617 *Age dummy 16 to 17 y*	0.36** (0.14)	0.41* (0.17)	0.73** (0.28)
Ageoffense1820 *Age dummy 18 to 20y*	0.16 (0.10)	0.21 (0.12)	0.34 (0.20)
Ageoffense2122 *Age dummy 21 to 22y*	0.08 (0.13)	0.13 (0.15)	0.23 (0.26)
Ageoffense *Age at offense*			
Female *Dummy Female*	-0.30*** (0.09)	-0.49*** (0.12)	-0.84*** (0.21)
Crimrec *Criminal Record*	0.04*** (0.01)	0.05*** (0.01)	0.08*** (0.01)
Percseated *% of time in jail*	0.27** (0.09)	0.36*** (0.11)	0.59** (0.18)
Jobexpgood *Good job exp.*	-0.46*** (0.06)	-0.52*** (0.07)	-0.83*** (0.12)
alc_drug *Alk/drug dep.*	0.31*** (0.06)	0.38*** (0.08)	0.66*** (0.13)
Crimdrug *Crime=Drug*	0.28*** (0.07)	0.39*** (0.09)	0.72*** (0.15)
Married *Dummy married*	-0.12 (0.08)	-0.21* (0.10)	-0.35* (0.17)
_cons	1.61*** (0.08)		
cut1		0.26** (0.10)	0.47** (0.17)
cut2		1.14*** (0.10)	1.95*** (0.18)
cut3		2.09*** (0.12)	3.72*** (0.22)
cut4		2.30*** (0.12)	4.17*** (0.24)
N	1139	1139	1139
adj. R^2	0.17		

Note: There are represented 3 regressions on the dependent variable expected recidivism taking on integer values from 1 to 5. In order to see the effects of the treatment we include 3 dummies, one for 16 and 17 year old people and thus juvenile legislation, one for the group in between 18 and 21, where both juvenile and criminal legislation applies and one for the group of 21 and 22, where only criminal legislation applies. We estimate this model with OLS(1), Ordered Probit (2) and Ordered Logit (3). The difference of the dummies is estimated and the corresponding p value is displayed.

Table 8 *Average Treatment effect*

Panel A *Estimated Dummies*

	OLS (1)	Ordered Logit (2)	Ordered Probit (3)
ageoffense1617	0.36	0.73	0.41
Prob. Of Treatment	0	0	0
ageoffense1820	0.16	0.34	0.21
Prob. Of Treatment	0.192983	0.192983	0.192983
ageoffense2122	0.08	0.23	0.13
Prob. Of Treatment	1	1	1

Panel B *Effect of Treatment*

Differences and corresponding p values			
ageoffense1617-ageoffense1820	0.20	0.39	0.20
Difference weighted by prob. Change (Treatment effect)	1.04	2.02	1.04
P - Value	0.2178	0.2411	0.2947
ageoffense1820-ageoffense2122	0.08	0.11	0.08
Difference weighted by prob. Change (Treatment effect)	0.10	0.14	0.10
P - Value	0.6019	0.7247	0.6937
ageoffense1617-ageoffense2122	0.28	0.50	0.28
Difference weighted by prob. Change (Treatment effect)	0.28	0.50	0.28
P - Value	0.1212	0.1767	0.204

Notes: This table represents the estimated effects of the treatment "adult legislation". Panel A reports the values of the dummies estimated in Table 2 and the corresponding probability that the treatment of adult law applies. I . For people younger than 18, when they committed a crime, juvenile law is applied for certain, while for people above 21 criminal legislation is applied for certain. In between some persons receive a juvenile penalty, while others receive an criminal penalty. In Panel B the effect of the treatment is calculated.

Table 9 *Separated Estimates*

Panel A: Cutoff 18, Part a) Juvenile Punishment

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwidth	-	28	40	59	48	68
Ageoffense<18 (α_{ERr})	-	3.585	2.323	2.268	2.282	2.282
Ageoffense>18 (α_{ERl})	-	2.480	0.677	2.162	0.629	2.146
Difference	-	-1.105	-1.646**	-0.106	-1.653**	-0.136
	-	(1.199)	(0.717)	(0.546)	(0.683)	(0.491)

Panel A: Cutoff 18, Part b) Criminal Punishment

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwidth	-	-	27	32	34	40
Ageoffense<18 (α_{ERr})	-	-	2.323	2.268	2.282	2.282
Ageoffense>18 (α_{ERl})	-	-	0.932	0.981	0.915	0.983
Difference	-	-	-1.391	-1.287	-1.367	-1.299
	-	-	(1.132)	0.924	(1.001)	(0.809)

Notes: This table represents the estimated effects of the treatment "criminal legislation" on recidivism. A nonparametric jump at the two cutoffs is estimated for different bandwidths. It separates the two samples of criminals convicted under juvenile legislation and criminals convicted after crmiminal legislation between the age of 18 and 20 (to the left of 18 juvenile law always applies and to the right criminal law always applies). The sample is now separa The Adj. takes into account that samples are not comparable in all respects and thus limits the sample to nonnmarried, nondrugdealing man. Where no vaules are displayed sample size is to small.

Panel B: Cutoff 21, Part a) Juvenile Punishment

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwidth	21	29	36	61	43	72
Ageoffense<21 (α_{ERr})	1.832	1.765	1.762	1.837	1.889	1.925
Ageoffense>21 (α_{ERl})	3.431	3.293	2.554	2.564	2.445	2.481
Difference	1.599	1.528	0.792	0.727	0.556	0.556
	(1.411)	1.029	(0.822)	(0.606)	(0.700)	(0.536)

Panel B: Cutoff 2, Part b) Criminal Punishment

Dependent Variable	Bandwidth 0.5		Bandwidth 1.1		Bandwidth 1.3	
Expected Recidivism	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample	Adj. Sample	Unadj. Sample
Sample Size within Bandwidth	11	18	27	46	30	52
Ageoffense<21 (α_{ERr})	0.017	0.141	1.679	1.178	1.762	1.201
Ageoffense>21 (α_{ERl})	3.431	3.293	2.554	2.564	2.445	2.481
Difference	3.414	3.152**	0.875	1.386**	0.683	1.28*
	(2.554)	(1.491)	(0.902)	(0.706)	(0.801)	(0.663)

Standard errors in parentheses * p<.1, ** p<.05, *** p<0.01

Notes: This table represents the estimated effects of the treatment "criminal legislation" on recidivism. A nonparametric jump at the two cutoffs is estimated for different bandwidths. It separates the two samples of criminals convicted under juvenile legislation and criminals convicted after crmiminal legislation between the age of 18 and 20 (to the left of 18 juvenile law always applies and to the right criminal law always applies). The sample is now separa The Adj. takes into account that samples are not comparable in all respects and thus limits the sample to nonnmarried, nondrugdealing man. Where no vaules are displayed sample size is to small.