The impact of formal control on delinquency during adolescence

Do formal control interventions prevent, promote, or have no effect on delinquent development?

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No man is an island, entire of itself; every man is a piece of the continent, a part of the main.

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[1] The remaining errors are of course fully my responsibility. I hope that I have not forgotten anyone else who provided more direct support to this dissertation. If I did, I am sorry and hope that you can still appreciate my indirect thanks.

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1 | SYNTHESIS

1.1 | Introduction

Within the first two decades of the twenty-first century, German criminal justice agents recorded an average of 240,000 juvenile (14 to 17-years-old) crime suspects per year (PKS Bundeskriminalamt, 2020).[2] Most of their offenses were relatively minor in nature (e.g., shoplifting, vandalism, or drug offenses). Confronted with such offenses, police typically only recorded suspects' personal details, and prosecutors informed them by mail that their case had been dismissed, sometimes on the condition of informal intervention (for example, by parents). In the case of more serious offenses (e.g., robbery or assault) or repeated arrests, the juvenile justice system could respond more severely. Sentences for these crimes included non-stationary educational or disciplinary measures (e.g., community service), up to four weeks of juvenile detention, or, in the most serious cases, juvenile prison. Since the German system is based on the principles of education and minimum intervention, few young offenders were generally sentenced to juvenile prison, and diversion of cases out of the formal system was the most common response. In the last twenty years, 70 or more percent of juvenile criminal cases have been diverted out of the formal system, and just 5 percent or less of all cases have ended in juvenile prison sentences (Dünkel & Heinz, 2017).

Germany appears to be in good company in its lenient treatment of young offenders. Experts suggest that over the last two decades, many if not most juvenile justice systems in (Western) Europe have tried to avoid stigmatization and adverse effects of formal processing by intervening minimally and addressing criminogenic educational deficits in young offenders (Dünkel, 2015, 2018; Junger-Tas, 2008; but see Goldson & Muncie, 2012). These experts note that youth justice has moved toward a more diversion- and education-oriented approach that reflects internationally established standards. Besides informal diversionary practices, this approach includes alternative educational and restorative justice measures such as cognitive-behavioral therapy, social training courses, and victim-offender mediation. Imprisonment, in contrast, was used less uniformly in the European systems. While some countries (e.g., England and Wales, France, and the Netherlands; see Goldson & Muncie, 2012; Muncie, 2008) have (temporarily) broadened the legal scope for imposition of juvenile prison sentences, most others have used imprisonment only as a last resort. Compared to the majority of European countries, the United States responds to juveniles offenses

[2] Consistent with the international crime drop (Farrell et al., 2014), the number of juvenile crime suspects has declined since 2000 in Germany (see Oberwittler, 2021; PKS Bundeskriminalamt, 2019).

more punitively overall, reflected, for example, by its higher incarceration rates relative to the European systems (Muncie, 2008; Nowak, 2019).[3]

Although formal responses to juvenile delinquency undeniably vary between countries and jurisdictions, all of the aforementioned justice systems have a primary goal in common: that of preventing young offenders from committing further crimes.[4] Because it is, however, by no means given that any of the various intervention measures or regimes prevent or reduce future delinquency, research on the impact of formal control is necessary. My dissertation contributes to this research by studying whether the interventions of the German juvenile justice system are efficient in preventing or reducing future crimes of apprehended offenders and why this may or may not be the case. Before summarizing my dissertation and its key findings, I will briefly review the state of the research on sanctioning effects.

1.2 | State of the research on sanctioning effects

The establishment of panel designs in criminology in the 1930s and 1940s (e.g., Glueck & Glueck, 1950) laid the groundwork for studying the effects of criminal justice interventions. Sanctioning research, finally, sparked in the late 1960s and 1970s, fueled by the debate between labeling proponents and deterrence proponents on whether formal control interventions promote or prevent future crimes (Klemke, 1978; C. W. Thomas & Bishop, 1984; Tittle, 1975). In one of the first studies of its kind, Gold and Williams (1969) reported the results from two separate samples. Both found that U.S. juveniles reported more self-reported delinquency after apprehension than matched counterparts who were not apprehended.

1.2.1 | Sanctioning effects on delinquency

Since this emergence of empirical research on sanctioning, numerous studies have analyzed offenders' delinquent behavior following criminal justice intervention. The reviews by Barrick

[3] Unfortunately, the juvenile justice data available internationally are of low quality, impeding robust statistical comparison of different systems (Campistol & Aebi, 2018). The findings outlined above should therefore be read with caution—they present general orientations and trends rather than factual similarities or differences in youth justice practices.

[4] It is important to note that the impact of formal control may extend beyond the apprehended offender. Although not the subject of this dissertation, *general crime prevention* (i.e., preventing potential offenders from committing crimes) must be considered alongside individual crime prevention when evaluating the overall impact of criminal justice and criminal policies (Andenaes, 1966). Furthermore, formal interventions cannot be justified solely on the basis of crime prevention but also on the premise of, for example, restoration or retribution (Canton & Padfield, 2019; Hoskins & Duff, 2021).

(2014) and Kleck and Sever (2017) reveal a vast body of empirical research on sanctioning effects, with a dramatic increase in studies on this topic since the 1990s. For these reviews, the authors screened the literature for English-language studies investigating the impact of criminal justice intervention[5] (compared to no or a less severe intervention) on the future delinquency of apprehended offenders. Their summaries of the research findings suggest that criminal justice interventions often lack the intended crime-preventive impact (see Table 1.1). Most empirical studies indeed report no statistically significant effects on future delinquency (Barrick: 46.7%; Kleck and Sever: 54.5%) or even crime-promoting effects (Barrick: 30.5%; Kleck and Sever: 28.8%).[6]

Table 1.1: Reviews of research findings on criminal justice intervention effects on crime

	# of	Percent of findings						
Type of analysis	**findings**	**– sig**	**– p = ?**	**– ns**	**? ns**	**+ ns**	**+ p = ?**	**+ sig**
Kleck and Sever (2017, pp. 144, 148, 150, 154)*:*								
All	659	14.6	0.6	22.5	4.4	27.6	2.1	28.2
Juveniles	104	9.6	1.9	14.4	1.0	28.8	1.0	43.3
Outside USA/Canada	104	14.4	1.0	14.4	8.7	15.4	3.8	42.3
Recidivism: Self-report of crime	95	17.9	0.0	23.2	5.3	26.3	1.1	26.3
Recidivism: Arrest	263	16.0	1.1	22.4	3.8	29.3	1.9	25.5
Recidivism: Conviction	99	8.1	1.0	19.2	3.0	23.2	4.0	41.4
Recidivism: Incarceration	32	21.9	0.0	9.4	12.5	18.8	9.4	28.1
Barrick (2014, pp. 96, 100)*:*								
All	167	13.2	2.4	19.8	--	26.9	7.2	30.5
Juveniles	24	4.2	0.0	12.5	--	8.3	16.7	58.3
Recidivism: Self-report of crime	15	0.0	0.0	0.0	--	6.7	20.0	73.3
Recidivism: Arrest/contact/report	52	15.4	1.9	30.8	--	28.8	1.9	21.2
Recidivism: Conviction	15	20.0	0.0	13.3	--	33.3	6.7	26.7
Recidivism: Incarceration	2	50.0	0.0	0.0	--	50.0	0.0	0.0

Notes: – sig: association negative and statistically significant; – p = ?: association negative, but significance level not reported; – ns: association negative but not statistically significant; ? ns: association not statistically significant and its sign was not reported; + ns: association positive but not statistically significant; + p = ?: association positive but significance level not reported; + sig: association positive and statistically significant.

However, the reviews also indicate that previous sanctioning research is limited in at least four crucial respects. First, most studies have relied on adult samples to study the effects of formal control interventions on future criminal activity. The impact of such interventions on *juveniles*, in contrast, has been studied much less (Barrick: 24 out of 167 findings; Kleck & Sever: 104 out of

[5] In this dissertation, criminal justice intervention is used synonymously with the term criminal justice contact. It thus encompasses all types of measures taken by the justice system, ranging from police contact and arrest to incarceration.
[6] Some other reviews have reported similar findings: Huizinga and Henry (2008) conclude in their review of prospective longitudinal studies based on general population probability samples that formal intervention mostly either had no measurable effect on crime or even a crime-promoting one. Loeffler and Nagin (2022) report that in most of their reviewed quasi-experimental studies, incarceration had either no significant impact or even crime-promoting effects. Finally, the meta-analysis of 29 experimental studies by Petrosino et al. (2014) suggests that traditional justice processing slightly increases delinquency relative to diversion.

659 findings). This is unfortunate because adolescence[7] is a life period in which humans are considered to be more susceptible to external sociocultural signals than in adulthood (Blakemore & Mills, 2014; Dahl et al., 2018). The findings in Barrick (2014) and Kleck and Sever (2017) seem to reflect this increased susceptibility, as a higher share of studies with youth samples (relative to adult samples) reported statistically significant (crime-promoting) effects. Furthermore, especially in mid- to late adolescence, people are more likely to commit street delinquency (e.g., assault, shoplifting, drugs, or vandalism; Boers, 2019; Day & Wiesner, 2019; Piquero, 2008). Thus, criminal justice contact is more common (although accompanied by less severe sanctions) in this age range than later in life (Dünkel & Heinz, 2017; Sampson & Laub, 1997).

Second, with only 104 out of the 659 findings reviewed by Kleck and Sever (2017), relatively little English-language sanctioning research is from *outside North America* (U.S. and Canada). For Germany, a comprehensive review of sanctioning research concluded that (adequately designed) German studies on the impact of formal control interventions are scarce to non-existent (Heinz, 2019). However, focusing on only one or a few criminal justice systems may come at a price. Legal systems differ in the intervention measures they have at their disposal, and formal control interventions may have varying effects on delinquency in different jurisdictions. The findings for the United States and Canada therefore probably do not apply to other countries. Kleck and Sever's (2017) findings indicate such differential effects, with statistically significant crime-promoting effects reported more often in studies outside of North America (Outside the United States and Canada: 42.3%; All: 28.2%).[8] Some differential effects were also found in a rare cross-national study that compared intervention effects in the U.S. and German juvenile justice systems (Huizinga et al., 2003).

Third, most previous studies measured criminal involvement (or recidivism) after a formal intervention using only official (e.g., police or court) data, and therefore typically measured whether an offender was re-recorded (e.g., rearrested; see Table 1.1) for committing a crime after a formal intervention. However, relying (only) on official data may be misleading when studying whether sanctions prevent crimes, since these data reflect a mixture of individual delinquency *and* formal reactions to that delinquency. Official measures can therefore be regarded as ill-defined recidivism proxies that cannot separate actual changes in offenders' criminal behavior from changes

[7] Blakemore and Mills (2014, p. 188) note that "[a]dolescence is often defined as the period between the onset of puberty and the achievement of relative self-sufficiency." The German juvenile justice system defines juveniles as those from age 14 to 17 (Dünkel & Heinz, 2017). In this dissertation, the terms juveniles and adolescents are used interchangeably (and refer specifically to individuals between the ages of 14 and 17 in later sections).

[8] While 20.9 to 33.3 percent of the findings were statistically significant and crime-promoting in the different U.S. regions, the same was true for only 9.3 percent of findings in Canada.

in the reactions of the justice system (Beardslee et al., 2019; Huizinga & Henry, 2008; Kleck & Sever, 2017; Liberman et al., 2014). Despite these problems with official data, relatively little research to date has investigated changes in *self-reported delinquency* (Barrick: 15 out of 167; Kleck & Sever: 95 out of 659). This is unfortunate, as self-reports provide pure measures of individual behavior and thus seem a more valid source for estimating how formal control interventions affect delinquency.[9] The importance of differentiating between the two aforementioned data types was recently highlighted by Liberman et al. (2014). They found that a first-time arrest moderately increased self-reported delinquency but amplified the official rearrest risk much more substantially. A handful of other (mainly U.S.-based) studies found similar effect differences (e.g., Beardslee et al., 2019; Klein, 1986),[10] highlighting that sanctioning research should not rely (solely) on official data to measure changes in delinquent behavior (see Farrington & Murray, 2014; Huizinga & Henry, 2008).

Finally, many studies falsely suggest that average crime-promoting or crime-preventing effects can be interpreted as support for labeling theory or deterrence theory, respectively (e.g., Berk et al., 1992; Liberman et al., 2014; Morris & Piquero, 2013; Motz et al., 2020; J. T. Ward et al., 2014).[11] However, these theories (like all other sanctioning theories) specify that formal interventions affect criminal behavior only if they trigger specific *processes* (see next section). Since different processes specified in vastly different theories may lead to similar (e.g., crime-promoting) sanctioning effects, a simple estimate of an intervention effect on delinquency is insufficient to validate the respective theory (see Bernburg, 2019; Huizinga & Henry, 2008; C. W. Thomas & Bishop, 1984). Instead, a proper validation approach should directly analyze the intervening processes and factors.

1.2.2 | Sanctioning effects on intervening factors

Unfortunately, research on potential intervening factors and processes is relatively scarce (see Bernburg, 2019; Huizinga & Henry, 2008; Krohn et al., 2014). This scarcity may result partly from the fact that studying the intervening factors and processes places high demands on data. The data

[9] However, self-reports are not in themselves a panacea. Juveniles tend to underreport their delinquent behavior, and there is evidence that this underreporting differs between groups (e.g., minorities underreport to a larger extent). Overall, however, self-reports of crimes and detections (arrests) are deemed reasonably reliable and valid (Köllisch & Oberwittler, 2004; Thornberry & Krohn, 2000).

[10] For some contrasting evidence, see Crasmöller (1996), who studied the effect of diversion versus traditional justice processing and found insignificant effects on both official and self-reported crime data.

[11] Some of these studies note that their estimates do not necessarily provide support for a particular theory (e.g., Morris & Piquero, 2013; J. T. Ward et al., 2014).

must not only be collected from the same individual over multiple time points but must also include self-report measures to operationalize the intervening factors. Consequently, to appropriately study the intervening processes, sanctioning research requires a panel design with self-report and ideally also official data (see Farrington & Murray, 2014; Huizinga & Henry, 2008; C. W. Thomas & Bishop, 1984). Such study designs are expensive and therefore relatively rare.[12] As a result, comparatively little research has been done on the intervening mechanisms, with a particular shortage of research on contexts outside the United States (for exceptions, see Murray et al., 2014; Schulte, 2019; Schulz, 2014; Zhang & Messner, 1994).

The existing research is guided mainly by two classic sanctioning theories: deterrence theory and labeling theory. Both theories specify intervening processes that formal control interventions must trigger to affect delinquency. In doing so, they delineate the specific intervening factors to be studied. *Deterrence theory* typically assumes that formal intervention may, in a first step, increase apprehended offenders' perceptions of sanction threat, including their perceptions of the certainty, severity, and celerity of punishment (Hirtenlehner, 2020; Paternoster, 2018; Pogarsky et al., 2004).[13] These altered sanction threat perceptions (i.e., the intervening factors) should then, in a second step, prevent further crimes by increasing the offenders' fear or worry about adverse legal consequences. Currently available evidence on these two deterrence processes or steps is mixed. With regard to the first step, most studies indicate that (increasing the certainty of) formal intervention increases the perceived certainty of apprehension (e.g., Anwar & Loughran, 2011; Horney & Marshall, 1992; Matsueda et al., 2006).[14] Little research exists, however, on how official interventions influence the perceived severity and celerity of punishment (for an exception, see Apospori & Alpert, 1993). Regarding the second step, the research suggests that only the perceived certainty of punishment has a small to modest deterrent effect. The findings on the impact of the perceived severity and celerity of punishment on delinquent behavior are, in contrast, much less convincing (Paternoster, 2018; Pratt et al., 2006; Pratt & Turanovic, 2018; Wikström, 2008).

Labeling theory assumes that formal interventions may stabilize or increase future delinquency through three central processes (Bernburg, 2019; see also Paternoster & Iovanni, 1989). First, they may facilitate the development or "hardening" of a deviant self-concept. Second, formal

[12] Boers (2019) estimates that more than 30 such criminological panel studies exist worldwide.
[13] Deterrence theory suggests that formal intervention increases sanction threat perceptions and delinquency only if it is applied with high enough certainty, severity, and celerity (see already Beccaria, 1764/1872). If this assumption is taken seriously, rare and non-severe formal interventions may also increase delinquency (see Paper I: Boers et al., 2022).
[14] However, some studies find more mixed or contradictory evidence (e.g., Pogarsky et al., 2005; Schulte, 2019; Schulz, 2014).

interventions may lead to the social exclusion of apprehended offenders, for example, by reducing or dissolving their conventional bonds and life chances. Third, formal interventions may, partly as a result of the social exclusion, facilitate associations with deviant peers. Labeling theory posits that in a second step, these three (altered) intervening factors—a deviant self-concept, social exclusion, and deviant peer associations—amplify future delinquency.[15] The empirical evidence on the impact of formal control on these three processes is as follows: On the one hand, changes in deviant identity have rarely been studied, and the existing findings are rather discouraging for labeling theory (e.g., Gibbs, 1974; C. W. Thomas & Bishop, 1984). On the other hand, there is more research on the other two intervening factors, suggesting that formal interventions amplify delinquency by decreasing conventional bonds and life chances or by increasing associations with deviant peers (e.g., Bernburg et al., 2006; Johnson et al., 2004; Kirk & Sampson, 2013; Lopes et al., 2012; Schulte, 2019; Wiley et al., 2013; Zhang & Messner, 1994).

Although the research mentioned above undoubtedly provides valuable insights into some of the processes that may be triggered by formal control, it is limited in a crucial way. As it typically relies on *either* deterrence theory or labeling theory, it is divided between two separate strands of literature (see Bernburg et al., 2006; Lochner, 2007; Matsueda et al., 2006; Wiley et al., 2013).[16] Although having multiple theoretical perspectives may often be useful for gaining knowledge, clashing viewpoints can also hinder a more integrative and comprehensive theoretical perspective on sanctioning effects. Such a perspective could acknowledge that intervening processes specified by theories as diverse as labeling and deterrence theory may operate simultaneously or in a person-specific manner. Thereby, it could challenge (oversimplifying) assumptions that one classic sanctioning theory may be more valid or appropriate than another, which could impede rather than facilitate the understanding of sanctioning effects (see Paternoster & Iovanni, 1989; Tittle, 1975). In this vein, Piquero et al. (2011, p. 338) highlighted that "the effect of sanctions on compliance is not one size fits all" and proposed that "it may be more profitable to think of a general theory of sanctions rather than deterrence, labeling, or defiance theory. A general theory of sanctions would simply argue that sanctions and sanction threats are important components of compliance, leaving it to be determined the conditions under which we could expect the direction of that effect to be positive, negative, or null."

[15] However, labeling theory acknowledges that formal interventions may, under some circumstances, also diminish these factors, which may then decrease future offending (see Paternoster & Iovanni, 1989; Thorsell & Klemke, 1972). For example, if formal control is accompanied by reintegrative reactions of informal others, it is suspected to potentially decrease future criminal activity (Jackson & Hay, 2013).

[16] Schulte (2019) is one of the few to have studied processes from both theoretical perspectives simultaneously in his extended deterrence model (see also C. W. Thomas & Bishop, 1984; D. A. Ward & Tittle, 1993).

1.3 | Current dissertation

In light of the previous research and its limitations, my aim in this dissertation was to answer two central research questions:

(RQ_1) *How do formal control interventions influence the future delinquency of young apprehended offenders in the German juvenile justice system? Do they prevent, promote, or have no effect on delinquency?*

(RQ_2) *Why do formal control interventions prevent, promote, or have no effect on delinquency?*

I analyzed these research questions in five individual articles. The current chapter explains how each of the articles addresses my research questions and contributes to the previous sanctioning research. In the following, I provide a brief overview of the data sources used in this dissertation, summarize the main results of each article, and discuss my dissertation's key findings.

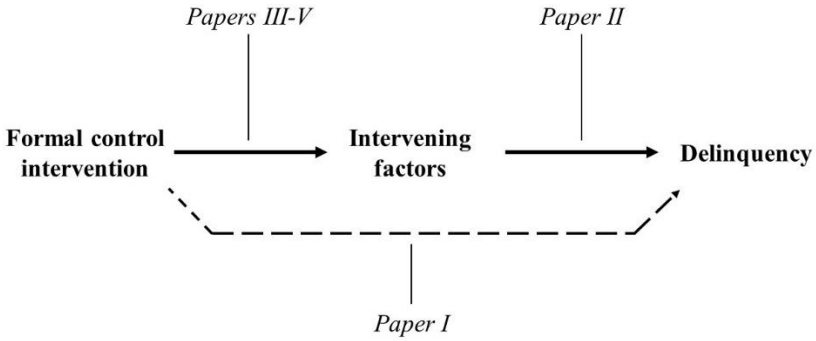

Figure 1.1: Scheme of the causal process in sanctioning research

The articles that make up this dissertation are presented in three parts (see Figure 1.1; Table 1.2). In Part I, the first paper (Boers et al., 2022) addresses research question RQ_1 by exploring how formal control interventions affect future delinquency. In Part II, the second paper (Kaiser, 2021) applies Situational Action Theory (SAT) to identify some of the crucial intervening factors and study how these interactively explain delinquent behavior. Part III, finally, consists of three articles (Kaiser, 2022; Kaiser et al., 2021, 2022) that contribute to research question RQ_2 by investigating how formal control influences some of these intervening factors.

Table 1.2: The individual papers of the dissertation project

Part	Chapter	Paper (Reference)	Aim of study	Data	Subsample	Independent variable(s)	Dependent variable(s)	Analytical methods
I	2	Paper I (Boers et al., 2022)	To explore how formal control affects the risk of future delinquency (and system contact)	CrimoC PADS+	Juveniles with official contact at age 15 and matched counterparts	Official contact with the juvenile justice system	Self-reported delinquency; official contact with the juvenile justice system	Propensity score matching
II	3	Paper II (Kaiser, 2021)	To identify the causes of crime (i.e., intervening factors) and study how they interact to produce delinquency	CrimoC	Juveniles who regularly participated in panel waves 2 to 5 (age 14 to 17)	Differential peer associations, personal morals, risky peer group activities	Self-reported delinquency	Multilevel Bayesian negative binomial regression
III	4	Paper III (Kaiser, 2022)	To explore how formal control affects some of the causes of crime (i.e., intervening factors) specified by SAT	CrimoC PADS+	Juveniles with official contact at age 15 and matched counterparts	Official contact with the juvenile justice system	Key intervening factors (including peer associations, personal morals, risk perceptions)	Propensity score matching
III	5	Paper IV (Kaiser et al., 2021)	To analyze the experiential argument, including its assumption that the risk of police detection is low	CrimoC	Juveniles who regularly participated in panel waves 2 to 5 (age 14 to 17)	Criminal offending (combining self-reported data on behavior in the past year and before)	Risk perceptions	Fixed-effects regressions
III	6	Paper V (Kaiser et al., 2022)	To analyze whether people with different morals update their risk perceptions differentially due to their experienced detection certainty	CrimoC	Juveniles who regularly participated in panel waves 2 to 5 (age 14 to 17) and committed a crime in the given wave (offender-only sample)	Detection-crime ratio (i.e., the ratio of the number of self-reported police detections to the number of self-reported crimes)	Risk perceptions	Fixed-effects regressions

1.3.1 | Data and study context

The data used in the current dissertation project stem mainly from the panel study *Crime in the modern City* (CrimoC; Boers et al., 2010). CrimoC started in 2002 by sampling two entire student cohorts in Duisburg, an industrial city in the west of Germany. The current dissertation considers only data from the younger cohort, consisting of all juveniles who were in seventh grade (average age 13) in 2002. CrimoC's sampling design included all 57 Duisburg schools. After 40 schools agreed to participate, self-administered questionnaires were distributed in their classrooms. In these questionnaires, the students reported on various topics, including delinquency, friends, family, leisure-time activities, moral attitudes, and risk perceptions. Valid data were gathered from 3,411 individuals (61% of the entire population of seventh graders) in this first sweep. These students were then interviewed repeatedly (first annually and later biannually), initially in the classroom and later by mail. Overall participation rates were satisfactory, with more than 3,200 individuals taking part in the first six waves (age 13 to 18; Bentrup, 2019). Along with the self-reported data, CrimoC also asked participants for their consent to collect official data from the *Erziehungsregister* and the *Bundeszentralregister* on any formal control interventions they had experienced (see Schulte, 2014). Official sanction data were available on 2,964 individuals from 2002 to 2009 (average age: 13 to 20 years).[17]

Two of the papers in this dissertation (Paper I and III) also rely on data from the *Peterborough Adolescent and Young Adult Development Study* (PADS+; Wikström et al., 2012) from England in addition to the CrimoC data. PADS+ proceeded similarly to CrimoC in collecting information on topics such as delinquency, routine activities, and moral attitudes from a juvenile population over multiple years (but in one-on-one interviews). However, PADS+ did not sample its participants within the school setting. It instead drew a random sample of an entire age cohort (13 years old on average when the self-report survey started). With 710 juveniles ultimately participating in the first (self-report) panel wave in 2004, PADS+ successfully achieved its goal of surveying about one third of these adolescents.[18] Participation rates were exceptionally high in the follow-up years, with interviews conducted annually at first and later at two- and three-year intervals. PADS+ also asked juveniles for permission to collect their official crime and sanction data from the Police National Computer system used across the United Kingdom, which 700 participants granted.

[17] For more information on the CrimoC study, see www.crimoc.org.
[18] Beyond the self-report questionnaire, PADS+ included a variety of other methods of collecting information about the lives and development of its young participants. Among these methods were parental interviews, a community survey, and a space-time budget (for more information, see https://www.cac.crim.cam.ac.uk/research/padspres).

Overall, the design of PADS+ resembles that of CrimoC and thus serves as a valuable point of comparison for the study of sanctioning effects (although measures were not all well-aligned; Kaiser et al., 2018). Having access to both data sources allows comparison of how the German and English juvenile justice systems differ in their formal reactions to juvenile delinquency and in their impacts on the apprehended juvenile offenders. Rarely has such comparative approach been used in sanctioning research, despite its potential to provide deeper insights into why formal control regimes may be more or less efficient in preventing crimes (for an exception, see Huizinga et al., 2003).

To provide some background for the cross-national comparison, it is necessary to highlight some fundamental differences that existed between the German and English juvenile justice systems in the mid-2000s (i.e., the study period; see Paper I: Boers et al., 2022). As a result of a punitive turn in criminal policy in the 1990s, England was characterized by a stricter diversion scheme and, in practice, diverted a smaller percentage of (14 to 17-years-old) juveniles out of formal proceedings than Germany (51 versus 77 percent). It also sentenced slightly more juveniles to long-term imprisonment (i.e., more than six months; 3 versus 1 percent). However, the differences in diversion were less pronounced in the German and English samples. The diversion rates of 73 (PADS+) and 81 (CrimoC) percent were higher and much more similar in the two samples than the rates on the country level.[19] The two systems thus differed in regard to this dissertation less in the approach chosen to handle young offenders and more in how police acted and in how they implemented diversion. In both regards, the English system was overall more interventive than the German one. During the study period, English police were urged to actively search for crime, whereas the German police usually only responded to crimes that were reported. Additionally, when young offenders in England were arrested for a minor crime, a possible diversion was exercised at the police station. If the offender confessed the crime under investigation, they received a formal warning in the presence of a parent or guardian. In Germany, in contrast, the diversion process was less intrusive: Offenders were typically only asked for their personal details and were later informed by mail that their case had been dismissed (sometimes after some form of informal intervention).

1.3.2 | Key findings of the individual papers

In the following subsections, I summarize the key results of the individual articles and show how they contribute to the main research questions. In doing so, I highlight how the results of the

[19] The percentages of long-term prison sentences resembled those at the country level.

articles fit together to provide a fuller picture of the crime-relevant impact formal control had on the juveniles under investigation.

1.3.2.1 | A study of the formal control impact on reoffending and future control

In Paper I of this dissertation (Boers et al., 2022), my coauthors and I explored how formal control intervention affected young offenders' future self-reported delinquency and their risk of official (re-)contact. We relied on data from three panel waves of both the CrimoC and PADS+ studies, in which the participants were 14, 15, and 16 years old on average. The three panel waves made it possible to specify a proper causal time ordering of the variables in a quasi-experimental design (see Liberman et al., 2014; Wiley & Esbensen, 2016): (1) pre-treatment, including the covariates at time point T1 (average age: 14); (2) treatment, the juvenile justice contact at time point T2 (average age: 15); and (3) post-treatment, including the outcome variables, self-reported delinquency and system contact at time point T3 (average age: 16). Only juveniles who had participated in all three waves and for whom official data were available were included in the analysis samples. The latter consisted of 2,117 and 690 juveniles in CrimoC and PADS+, respectively, of whom 88 (4.2 percent) and 37 (5.3 percent) experienced a system contact in T2.

For analysis, Paper I used propensity score matching, a state-of-the-art analytical technique to calculate causal sanctioning effects (e.g., Liberman et al., 2014; Wiley & Esbensen, 2016). Propensity score matching aims at mimicking a randomized experiment by (post hoc) constructing treatment and control groups that are ideally equal in their covariate distributions (Apel & Sweeten, 2010; Stuart, 2010). For this purpose, the covariates were first used as predictors in regression models to estimate each individual's probability of having system contact in T2. The estimated probabilities, also called propensity scores, were then used to find, for each individual with system contact in T2, a counterpart (or counterparts) with a similar propensity score but no system contact in T2. Regression models were finally computed to estimate treatment effects for the treated individuals and their matched counterparts.

The propensity score analysis produced two key findings. First, system contact had no statistically significant effects on self-reported delinquency in either the German or the English sample. Estimates indicate that the prevalence of various offense types (property offending, vandalism, violence) changed typically about five percentage points or less due to system contact (compared to having none). The versatility of offending, used as an indicator of the intensity of offending, was also not significantly affected by official intervention. Although delinquent behavior was therefore

estimated to be influenced little by formal control, the second finding of Paper I suggests that system contact substantially increased the risk of future official registrations. The estimates indicate that the likelihood of future system contact increased by about 15 percentage points in the German sample and 23 percentage points in the English one.

However, the estimates in Paper I "only" capture the (lack of an) average impact of official control interventions on future criminal behavior (and official contact). The findings offer no insight into why no or only small effects on self-reported delinquency were observed. However, understanding the reasons behind the lack of a formal control effect is crucial to the design of criminal justice policies that make the legal system more efficient in preventing recidivism. These reasons can only be captured by studying the intervening processes triggered by formal control intervention. As noted above, different theories (among them deterrence and labeling theory) have proposed a variety of intervening processes and factors.

1.3.2.2 | Testing interactional implications of SAT's action model

To identify the most relevant of these intervening factors and to provide a starting point for a more general theory of sanctions, I used Situational Action Theory in this dissertation (Wikström, 2019a; Wikström et al., 2012). I chose SAT as a theoretical background for three reasons. First, the theory aims at overcoming the problem that "everything seems to matter" in criminology by precisely identifying the most important causes of crime (Wikström, 2011). These causes should also be the critical intervening factors in sanctioning research since only they can explain why someone either offends or abstains from offending after some criminal justice intervention (Wikström & Treiber, 2016a). Second, SAT's causes of crime align with some of the intervening factors suggested by labeling and deterrence theory (e.g., deviant peer associations and risk perceptions), making it appear feasible to integrate some of the processes specified by these theories into SAT. Third, SAT contains a well-specified action model that incorporates modern insights from other relevant behavioral sciences, such as the dual-process nature of decision making. It thus addresses what Wikström (2006, 2010) identified as a lack of an adequate action model in earlier theories of crime (see also Wikström & Treiber, 2016b).

SAT's action model highlights the importance of the person-environment interaction and specifies a perception-choice process for explaining criminal behavior at the situational level. Within this

process, personal morals,[20] self-control capabilities, and sanction threat perceptions[21] are the person-level causes of crime that contribute to an individual's crime propensity. Environmental-level causes of crime are features of the places (e.g., if they are monitored) and persons (e.g., deviant peers) that individuals encounter, determining their criminogenic exposure. According to SAT, the personal and environmental causes of crime interact in each moment to influence the perception of crime as an action alternative and the choice of whether or not to engage in such rule-breaking behavior (for a more detailed description, see Wikström et al., 2012; see also Papers II and III: Kaiser, 2021, 2022).

SAT can only serve as a starting point for a general theory of sanctions if its action model holds what it promises. I therefore tested some of its implications in Paper II of this dissertation (Kaiser, 2021). This paper explored how associations with crime-prone versus crime-averse peers (environmental cause of crime) influenced a person's criminal involvement. Using the data from 3,290 CrimoC participants aged 14 to 17 (panel wave 2 to 5), the investigation replicated some results previously produced with the PADS+ sample (Beier, 2018; Wikström et al., 2012). It analyzed a complex interactional implication derived from SAT, namely that criminogenic peer influence varies depending on the combination of unstructured socializing[22] and personal morals. SAT's action model implies that peers exercise an unimpeded criminogenic influence only when people with weak moral opposition to delinquency spend time with their peers in unstructured and unmonitored activities. If either a person's morals or the environmental features of an activity hinder the perception and choice of crime as an action alternative, peer influence on delinquency should diminish substantially.

Paper II modeled this complex three-way interaction between differential peer associations, unstructured socializing, and personal morals by computing marginal effects from a multilevel Bayesian negative binomial regression. This regression model included the frequency of crimes as the dependent variable and measures of differential peer associations, unstructured socializing, and personal morals as independent variables (as well as their product terms). The results support the implications of SAT's action model by suggesting that peer influence varies depending on the

[20] According to SAT (Wikström, 2020, p. 193), personal morals reflect a person's "value-based and emotionally grounded views about what is the right or wrong thing to do or not to do in particular circumstances."

[21] Wikström generally only refers to personal morals and self-control abilities as building blocks of an individual's crime propensity. However, one of his articles implies that deterrence sensitivities (or sanction threat perceptions) are another critical cause of crime, playing an essential role in SAT's deterrence process (Wikström, 2011; see also Pauwels et al., 2018).

[22] Unstructured socializing is a term borrowed from routine activity theory (Osgood et al., 1996) and refers to time spend with *peers* in activities without specific agenda (*unstructured*) and without guardians (e.g., parents) who may enforce legal rules (*unmonitored*).

combination of personal morals and unstructured socializing. The criminogenic impact of (crime-prone) peer associations was largest when individuals had relatively weak moral opposition to delinquency *and* spent fairly large amounts of time socializing with their peers in an unstructured manner. On the other hand, criminogenic peer influence decreased markedly when a person's morals were strong *or* when they either spent little time socializing with peers or when they socialized with peers in a relatively structured manner.

These findings supplement the growing research on SAT, suggesting that the theory is indeed a promising approach to explaining criminal behavior (for a review, see Pauwels et al., 2018). SAT thus seems well-positioned to serve as a foundation for a general theory of sanctions. As its causes of crime (and their interplay) have fared well in empirical tests so far, they may constitute the critical intervening factors that must be influenced through formal control interventions to change future delinquent behavior.

1.3.2.3 | A study of the formal control impact on intervening factors

The third paper in this dissertation (Paper III; Kaiser, 2022) thus analyzed how some of SAT's causes of crime were influenced by the formal interventions studied in the first paper. In doing so, it explored why formal control had relatively little influence on the future delinquency of the young offenders studied (research question RQ_2). Paper III used the same data, design, and analytical procedures as the first article but analyzed different outcome variables: Instead of self-reported delinquency and system contact, the dependent variables included some crucial intervening factors at T3. These intervening factors, identified with the help of SAT, were personal morals, deviant peer associations, and risk perceptions (i.e., the perception of detection risk).[23]

The study of these factors as dependent variables indicated that formal control had, on average, little effect on most of the intervening factors. In the CrimoC sample, neither of the intervening factors selected through SAT was significantly affected by juvenile justice contact. In PADS+, most outcome variables were also not substantially influenced by system contact. The few statistically significant estimates suggest that individuals with official contact subsequently reported increased deviant peer associations and slightly reduced feelings of guilt (compared to their matched counterparts). However, some robustness analyses challenged the significant effect on

[23] These three factors were selected from the causes of crime specified in SAT, as they were the only ones for which similar measures were available in CrimoC and PADS+.

deviant peer associations. Against this backdrop, the paper concluded that system contact may often did little to prevent crime, as it had no or only weak effects on SAT's causes of crime.

In the discussion, Paper III proposed some explanations for why no or only small formal control effects were found. The first possible explanation is that formal intervention has a greater impact the more likely it is and produced only small effects in this dissertation because most of the young offenders studied were detected with low certainty (likelihood) when they committed their crimes (*certainty argument*). Deterrence theory suggests that rare arrests (interventions) should have little effect on offenders' risk perceptions if they already have learned through many previous successful offenses that the detection risk is low (Anwar & Loughran, 2011; Pogarsky et al., 2004, 2005). Similarly, labeling theory suggests that a formal intervention is less likely to trigger labeling processes (e.g., social exclusion) when these were already initiated before, for example, by previous informal reactions to officially unsanctioned crimes (Bernburg, 2019; Paternoster & Iovanni, 1989; Zhang, 1997). In this vein, both deterrence and labeling processes may, due to the low likelihood of formal intervention, have already run their course and solidified some of the intervening factors before juveniles' first official contact with the criminal justice system. A second explanation may be that formal intervention has a greater impact the more severe it is and effects may thus be small in this dissertation because most young offenders received lenient punishments (*severity argument*). According to deterrence theory, lenient formal reactions should, other than severe sanctions, be relatively ineffective in heightening offenders' perceptions of sanction severity (Apospori & Alpert, 1993). On the other hand, labeling research typically assumes that more severe formal interventions represent more severe labels. The latter should have a greater criminogenic impact (compared to less severe interventions and labels) because they are more likely to trigger (e.g., exclusionary) labeling processes (Paternoster & Iovanni, 1989; see also Wiley et al., 2013).

Paper III provided, however, no tests of the proposed certainty and severity arguments. It reports that most young offenders were handled relatively leniently by being diverted out of the formal juvenile justice system, but otherwise gives little insight into (1) whether the juveniles were rarely detected when committing crimes[24] and (2) whether a higher detection certainty or more severe sanctions would have produced more substantial effects (be they crime-preventing or crime-promoting).

[24] But see online supplementary material S1 to Paper III (Kaiser, 2022) for evidence of low detection risk.

1.3.2.4 | A study of the low risk of formal intervention (and the experiential effect)

Paper IV (Kaiser et al., 2021) provide some of these insights as a byproduct of studying the validity of the so-called "experiential argument." This argument suggests that people who commit (their first) crimes will typically learn that they are less likely to be caught than they initially thought, and will reduce their perceived risk of detection accordingly (see Paternoster et al., 1985). One underlying assumption behind the experiential argument is that people are rarely detected when they commit crimes. This assumption aligns with the certainty argument that formal control interventions may have little impact because they are *rare events*.

Following this line of thought, the fourth paper analyzed, amongst others, whether being caught by police is indeed a rare experience when people commit a crime. The analysis was based exclusively on self-reports of 3,259 juveniles from panel waves 2 (age 14) to 5 (age 17) of the German CrimoC study. It was restricted to this sample because most aspects of the experiential argument had been studied with the English data (Hirtenlehner & Wikström, 2017; Wikström et al., 2012), but fewer with the German data (Seddig et al., 2017). The paper computed offense-specific and total detection rates to study the detection risk. These rates were calculated for the whole sample by dividing the absolute (self-reported) number of crimes the police were aware of by the absolute (self-reported) number of crimes committed.[25]

The calculated detection rates support the assumption of the experiential argument that individuals are rarely caught when committing crimes. The juveniles in the study reported that the police detected only about two percent of all offenses they had committed. Additionally, all calculated offense-specific detection rates were well below 10 percent. These results thus indicate that juveniles offended successfully (i.e., without detection) most of the time and that system contact was indeed a rare event in comparison to the many criminal offenses that went undetected. The findings, hence, strengthen the assumption of the certainty argument that formal control intervention was highly unlikely.

However, this assumption of low detection risk is only one part of the certainty argument proposed above. Additional evidence is needed for the second assumption underlying the argument, which

[25] Paper IV and V were based on self-report detection data instead of official sanction (arrest) data for two reasons. First, the self-reported detection data perfectly match the self-reported delinquency categories. Such matching is much more difficult if not impossible, with the crime categories used in official sanction data. It therefore seems more reliable to compute detection rates with self-report data only. Second, it should be more critical for the formation and change of risk perceptions that individuals remember (self-report) being detected by police than that they have a criminal record in an official database.

is that the lower the certainty (likelihood) of formal control interventions, the less impact they have.

1.3.2.5 | A study of the differential effect of the experienced detection certainty

Paper V (Kaiser et al., 2022) provided this requested evidence (in regard to the deterrence process) by analyzing how the certainty of detection experienced by offenders related to their risk perceptions. The paper studied these perceptional updating processes again exclusively using the German data because only CrimoC includes self-reported *frequency* information on police detections. This frequency information is necessary to measure the *experienced detection certainty* of an individual offender. This certainty was operationalized by dividing the number of police detections by the number of criminal offenses committed by an offender (see Matsueda et al., 2006). This detection-crime ratio makes it possible to directly investigate the certainty argument that a higher (smaller) certainty of intervention is linked to a greater (smaller) formal control impact. It does so by studying whether a higher experienced detection certainty (i.e., ratio value) was related to a subsequently increased detection risk perception (which should then, according to deterrence theory, deter individuals from committing crimes). This relationship was analyzed with fixed effects regression models that allow estimation of intraindividual changes over time that cannot be confounded by time-stable characteristics (Allison, 2009).

The fixed effects estimates, which were calculated on the basis of data from 1,385 young CrimoC offenders aged 14 to 17 (panel wave 2 to 5), further bolster the certainty argument.[26] They suggest that offenders only increase their perceived detection risk substantially if they experience a substantial increase in their certainty of detection. Combined with the finding of the previous paper that juveniles were rarely caught when they committed crimes, these findings indicate that formal control interventions may indeed have had little impact on risk perceptions because they occurred so rarely. To put it bluntly: If apprehension appears as one adverse event among numerous successful (i.e., undetected) experiences with criminal offending, it may make little impression on young offenders (risk perceptions).

Beyond providing support for the certainty argument, Paper V also studied an additional explanation for the low impact of formal control: that more substantial effects may be masked by the heterogeneity of the impact. More precisely, it investigated whether people update their risk

[26] The sample consist solely of *offenders* since only for them an individual detection-crime ratio can be calculated (the number of offenses is in the denominator of the ratio value).

perceptions differently depending on their personal morals. The results indicate that an increased experienced certainty of detection had little effect on the risk perceptions of individuals with medium to strong moral opposition to delinquency. Individuals with weaker moral opposition to delinquency, in contrast, seemed to learn from their experiences. They assessed their detection risk substantially higher after experiencing an increased detection certainty. This differential learning could contribute to small effects of formal control if we assume that a substantial portion of the apprehended juveniles held medium to strong morals. This assumption is not entirely implausible, considering that most adolescents (including those who periodically break some rules) have internalized the legal norms of society (Hirtenlehner & Reinecke, 2018b; Kroneberg & Schulz, 2018). If this is indeed true, then substantial deterrence effects of formal control are, on average, unlikely because only the relatively small portion of individuals with weak morals will learn substantially from their arrest experiences.

1.4 | Discussion and Outlook

The current dissertation investigated the influence of formal control interventions on the future delinquency of young offenders in the German juvenile justice system. To do so, it relied primarily on data from CrimoC, a panel study collecting data from Duisburg adolescents. Some analyses also employed a comparative approach and produced similar results with a sample of English juveniles, indicating that the results may transfer to other national contexts and jurisdictions.

1.4.1 | The key findings

Overall, the dissertation consists of five papers (which can be found in full length in the subsequent chapters). These papers produced four main sets of findings concerning the dissertation's two central research questions. The *first finding* is that contact with the justice system had little effect on the delinquent behavior of the young offenders studied. Juveniles with system contact were characterized by a similar prevalence and intensity of subsequent delinquency as matched and thus similar juveniles without contact. These results answer research question RQ_1 by indicating that the German juvenile justice system was on average, at least for the sample under investigation, relatively ineffective in reducing the future criminal behavior of young offenders. However, with this ineffectiveness, the German system fares quite well considering that previous (mainly U.S.-based) research more often reported that criminal justice interventions were to promote than to prevent crime (see Table 1.1; Barrick, 2014; Kleck & Sever, 2017).

The *second finding* of the dissertation addresses the question of why juvenile justice interventions had such a small effect on future delinquency (RQ_2). The results presented here show that none of the relevant intervening factors (personal morals, risk perceptions, and deviant peer associations) selected through SAT were significantly affected by system contact in the CrimoC sample. These results indicate that the juvenile justice system had almost no influence on offenders' future delinquency because it produced, on average, no substantial effects on the proximate causes of crime (i.e., the intervening factors). The results do not support another possible explanation for the small impact, namely that formal control triggered substantial intervening processes, which, however, canceled each other out by operating in opposite directions (i.e., preventing crime versus promoting crime). This lack of impact on the intervening factors contradicts the more substantial (significant) effects found in a small body of previous U.S.-based research (e.g., Anwar & Loughran, 2011; Bernburg et al., 2006; Matsueda et al., 2006; Wiley et al., 2013). Confronted with these results, Paper III speculated that the effects reported in this dissertation might be somewhat smaller because the German juvenile justice system and to some extent also the English system reacted to juvenile delinquency with somewhat lower certainty and severity than the U.S. system. However, these speculations about a potentially differential certainty and severity effect in the different systems could not be directly tested with the given data as no comparable U.S. data were available.[27]

However, these issues were partly (i.e., for deterrence theory) addressed in the *third set of findings*, which show that the *certainty* of formal intervention may be an essential component of its effectiveness in preventing delinquency. The results suggest that the (deterrence-related) intervention effects in this dissertation might have been small because (1) the juveniles studied were rarely detected when they committed crimes, and (2) a low detection certainty is accompanied by smaller effects on individual risk perceptions (one of the causes of crime). These two latter findings corroborate the certainty argument proposed in Paper III (regarding deterrence processes) that the impact of formal control depends on how likely intervention is and thus give further insights into research question RQ_2. The importance of the certainty component for deterrence processes—which was highlighted centuries ago by Beccaria (1764/1872)—has also been emphasized by a growing number of studies and was labeled as the "certainty principle" (Apel, 2013). This research implies (1) that the detection or arrest risk is generally low (e.g., Enzmann, 2012; Lochner, 2007; Wikström et al., 2012), (2) that a higher experienced arrest or detection certainty is related to greater risk perceptions (e.g., Anwar & Loughran, 2011; Horney & Marshall, 1992; Matsueda et

[27] To my knowledge, there is also no other analysis that could provide any evidence of whether and why the (strength of the) *intervening processes* differ between the German and U.S. system (Huizinga et al., 2003, only report findings on the impact on future delinquency and subsequent work status in their cross-national analysis).

al., 2006), and (3) that increased risk perceptions weakly to moderately decrease the likelihood of committing crimes (see Paternoster, 2018; Pratt et al., 2006).

The *fourth set of findings* transcend the two research questions posed in this dissertation and highlight an aspect of growing importance in criminology: differential effects (or differential impact). For sanctioning research, differential effects mean that the impact of formal interventions may apply only to a subset of individuals or may depend on what sanction is applied when and how (see Huizinga & Henry, 2008; Loughran et al., 2010; Tittle, 1975). The desideratum of such a differential perspective was formulated most concisely by Sherman (1993, p. 445) in his call for research on the question: "[U]nder what conditions does each type of criminal sanction reduce, increase or have no effect on future crimes?" This dissertation focused on differential sanctioning effects in the last paper, which showed that the impact of formal control varied by personal characteristics in the CrimoC sample. An increased detection certainty was related to increased risk perceptions only among individuals with weak moral opposition to delinquency, while the relationship vanished for individuals with stronger morals. Combined with findings that sanction threat perceptions deter crimes particularly among individuals with weak morals (e.g., Hirtenlehner & Reinecke, 2018a; Kroneberg et al., 2010; Svensson, 2015), this finding implies that the deterrence process may be mainly or only relevant to a subset of people. This result, and the growing number of research findings on differential sanctioning effects (for reviews, see Bernburg, 2019; Loughran et al., 2018), indicate that SAT has a point in claiming that explaining crime (including its prevention) "is all about interactions" (Wikström, 2020, p. 193). The theory may therefore prove pivotal in guiding the study of differential sanctioning effects that many argue should be embraced in future research (e.g., Barrick, 2014; Bernburg, 2019; Kirk & Wakefield, 2018; Loughran et al., 2018).[28]

1.4.2 | Limitations and perspectives

Although this dissertation offers key insights into the impact of sanctioning, it resembles other studies in raising more questions than it answers. One of these questions is whether the overall low *severity* of intervention in the sample studied is indeed part of the explanation for the observed small impact of formal control. This severity argument, proposed in the third paper, was not tested in this dissertation. However, in a previous dissertation using CrimoC data, Schulte (2019)

[28] However, it is important to note that to date, SAT has concentrated mainly on its action model, and the developmental parts of the theory have been introduced only more recently (see Wikström & Treiber, 2016a, 2018) and will need some more refinement to allow the derivation of precise testable implications.

analyzed the impact of the severity of intervention by measuring formal control as an ordinal index ranging from non-intervention to conviction. His analyses indicate that more severe sanctions are related to increased subsequent violent delinquency by way of their impact on peer associations and personal morals. A handful of other previous studies with quasi-experimental designs produced similar findings, indicating that more severe interventions seem to be related to increased reoffending (e.g., Beardslee et al., 2019; McAra & McVie, 2007; Wiley et al., 2013). Furthermore, a meta-analysis of 29 experimental studies shows that "traditional" system processing amplifies delinquency compared to diversionary practices (Petrosino et al., 2014). Overall, other research therefore suggests that the small impact of formal control in this dissertation may indeed be partly attributable to the lenient nature of the interventions (primarily diversions). However, it must be noted that this body of research is relatively small and often ignores the intervening processes by only investigating the impact of intervention severity on reoffending. This lack of research on the intervening processes is especially acute with regard to deterrence, with very few studies exploring the impact of the experienced severity of formal control intervention on perceived sanction severity (e.g., Apospori & Alpert, 1993; Wood, 2007).

A second question is whether the *certainty* of formal control intervention plays a role in other mechanisms than those specified by deterrence. Most research to date has studied the effects of this component from a deterrence perspective (Kreager & Matsueda, 2014; Paternoster, 2018), probably because it was already deemed central by the pioneers of this framework (Beccaria, 1764/1872; Bentham, 1789/2000).[29] This dissertation is no exception: It only explored the assumption of deterrence theory that a higher certainty of intervention increases individual risk perceptions (which, in turn, should reduce future delinquency). In contrast, sanctioning research on the relationship between the certainty of formal intervention and other processes is entirely missing. This is unfortunate because implications can also be derived from labeling theory and SAT's developmental model that the effects of formal control interventions may be contingent on their certainty. Labeling theory, as noted above, suggests that processes of social exclusion or association with deviant peers might more likely to be triggered when (labeling) reactions occur early in a series of offenses (Bernburg, 2019; Paternoster & Iovanni, 1989; Tittle, 1975). However, since formal intervention is unlikely, many informal reactions are likely to have already shaped the intervening factors of an offender so that an additional justice intervention may have little additional impact. On a related topic, SAT's developmental theory suggests that moral education (i.e.,

[29] Beccaria introduced celerity as another important component of punishment. This component is, however, much less researched and has produced discouraging evidence if studied (for a review, see Pratt & Turanovic, 2018).

learning personal morals in social interactions) is contingent on the consistency of (formal and informal) reactions and a person's prior experiences and existing personal morals (Wikström & Treiber, 2016a, 2018). If we again make the reasonable assumption that the morals of offenders are likely to have already been shaped and solidified by many (relatively consistent) informal control reactions to their crimes, then one rare formal apprehension may have little additional impact. Future research should devote particular attention to the implications of both of the aforementioned theories in regard to the certainty component of formal control.

A final and similar question worth discussing is whether the *differential effects* of formal control extend beyond deterrence processes. This dissertation focused entirely on perceptual updating (specified in deterrence theory) when studying whether people learn differently from formal control experiences. However, previous theoretical work has suggested that differential sanctioning effects might also apply to other processes. Labeling theory, for example, assumes that formal interventions may or may not lead to social exclusion and associations with deviant peers, depending on how the sanction is applied and on how informal others react to it, among other factors (Bernburg, 2019; Paternoster & Iovanni, 1989). The developmental model of SAT also implies that interventions may influence personal morals differently depending, for instance, on people's prior moral education experiences and cognitive abilities (Wikström & Treiber, 2016a, 2018). So far, however, most differential sanctioning research has focused on deterrence processes (for reviews, see Hirtenlehner, 2020; Loughran et al., 2018; Piquero et al., 2011). Only a few studies have researched differential labeling processes, concentrating mainly on socio-demographic moderators (e.g., minority status or social disadvantage; see Bernburg, 2019). Research on differential moral education processes is so far—at least to my knowledge—entirely absent from the literature. This gap should be addressed in future research. Some panel studies (including CrimoC) already provide an excellent data source for such an investigation as they include relevant measures of social exclusion, deviant peer associations, and personal morals.

1.4.3 | Policy implications

At first glance, the finding that formal control interventions in the German sample were relatively inefficient in preventing recidivism of the young offenders may seem alarming—especially since preventing recidivism is one of the main goals of criminal justice interventions. Under the assumption that the small impact of formal interventions is due to their low certainty and severity in the German juvenile justice system, one could argue that formal control agencies should sharpen their

teeth (i.e., tighten their sanctions and enforce them more likely and more strictly). Proponents of 'neoliberal' or 'neo-correctional' crime policies who demand that the system respond to juvenile delinquency more proactively and more invasively would presumably welcome such a more punitive approach (see Case & Haines, 2018; Dünkel, 2015; Goldson, 2002; Muncie, 2008).

However, when considering the empirical state of the research, the results presented in this dissertation appear in a different light. On the one hand, empirical studies find more often crime-promoting effects of formal control interventions than crime-preventing effects (Barrick, 2014; Kleck & Sever, 2017). On the other hand, several studies have suggested that, compared to lenient interventions, more severe interventions promote future delinquency (e.g., Beardslee et al., 2019; McAra & McVie, 2007; Petrosino et al., 2014; Schulte, 2019; Wiley et al., 2013). With this information at hand, increasing the certainty and severity of criminal justice intervention would likely increase the impact of intervention by triggering crime-relevant processes, but at what cost? This increased impact may not necessarily operate, as hoped, in a crime-preventative direction (e.g., through a deterrence process) but holds the danger of unintentionally promoting crime (e.g., through processes of social exclusion).

Against this backdrop, the German juvenile justice system's current approach of diverting most young offenders out of the traditional justice process may be doing more good than harm. Indeed, an anonymous reviewer of one of the papers pointed out that diversion might be seen as a tool that diminishes the effects of formal control by design. Diversion may theoretically be expected to produce fewer deterrence effects than more severe sanctions, but it may also mitigate detrimental labeling effects. This latter hope is underscored by the meta-analysis of Petrosino et al. (2014), who produced evidence that diversion may be accompanied by less recidivism than traditional sanctioning. Confronted with this and other findings that place diversionary practices in a favorable light (e.g., Beardslee et al., 2019; Schwalbe et al., 2012; D. B. Wilson et al., 2018; H. A. Wilson & Hoge, 2013), it may be advisable to follow the path taken by the German juvenile justice system over the last decades and deal with most offenders in a relatively lenient way (Dünkel & Heinz, 2017; Heinz, 2019).[30] As empirical sanctioning research on many other topics is unfortunately particularly in Germany scarce, it is reasonable to wait before making any more firm policy recommendations. These can only be based on further high-quality research that produces deeper

[30] However, the heterogeneity of the underlying findings in the meta-analysis indicate that diversion cannot be seen as a magic bullet but that it may also produce detrimental effects (compared to other forms of interventions) if applied to particular people in particular circumstances. It is up to future research to study this heterogeneity in more detail.

insights into how particular formal control interventions affect particular people (for existing research on various intervention programs, see Heinz, 2019; Pappas & Dent, 2021).

Finally, it should be highlighted that, even if diversion may attenuate crime-promoting effects, it may still lead to an increased risk of future formal control (compared to non-intervention). This is at least what Paper I and other studies imply with their findings that justice interventions have a greater impact on the risk of future system contact than on future self-reported delinquency (e.g., Beardslee et al., 2019; Boers et al., 2022; Klein, 1986; Liberman et al., 2014). Liberman et al. (2014, p. 363) speculated that this finding might result from an "increased scrutiny [and reduced tolerance] of the individual's future behavior, by police as well as other actors such as teachers and school staff." Similarly, Paper I (Boers et al., 2022) notes that this "auto-dynamic effect" might reflect the tendency of "[a] social control system [to reproduce] itself by referring to its own prior control decisions, filed in the institutional memory of police and court registers." So far, however, no study has investigated the precise processes that lead to this self-referencing aspect of formal control. Future research should therefore pay particular attention to why formal control agents may be more likely to respond to the criminal behavior of someone who was previously registered for a crime (compared to someone who was not). This research is critical since repeated formal interventions increase the risk of more severe sanctions, which, in turn, appear to have a criminogenic impact. Criminal justice policy would therefore do well to think not only about how to prevent recidivism but also about how to decrease the likelihood of self-referential intervention and sanctioning decisions.

PART I.

FORMAL CONTROL AND DELINQUENCY

2 | PAPER I

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The impact of formal controls:

A comparative study between an English and a German city

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ABSTRACT

The study of sanctioning effects has a rich history in deterrence and labeling theory. Most analyses have only used official data to study these effects. Yet, some more recent studies indicate that it is necessary to investigate self-reported as well as official data since it appears that sanctioning has differential effects on self-reported delinquency and formal control interventions. The current study contributes to this small body of research by using Propensity Score Matching to analyze panel data from an ongoing English (Peterborough Adolescent and Young Adult Developmental Study) and a German (Crime in the modern City) study. We estimated average treatment effects of system contacts on both reoffending and subsequent contacts for juveniles living in Peterborough (ENG) and Duisburg (GER). Our findings are that (1) although official contacts have no substantial effects on the prevalence or versatility of reoffending, (2) they substantially increase the risk of a future formal contact. These results were almost identical at both sites, which may indicate a more general finding on the effects of formal control interventions.

KEYWORDS

cross-national research, effects of formal control interventions, propensity score matching, secondary sanctioning

2.1 | Introduction

In criminology, the impact of interventions and sanctions by the formal social control agencies police and criminal courts is assumed to have two directions: They may prevent offenders from further offending, or they may reinforce subsequent delinquency.

The idea that penal sanctions shall have a crime-preventive impact is mainly a heritage of the Age of Enlightenment. In order to restrict the excessive retributive punishments in feudal regimes, two concepts evolved. First, the concept of treatment: if offenders are treated through a strict working regime—in lieu of corporal or capital punishment—they will become honest and will rehabilitate (Howard, 1777).[31] Second, the concept of deterrence: Philosophers like Beccaria (1764/1872) or Bentham (1789/2000) proposed that a humane penal harm (which excluded corporal or capital punishment) should be determined in such a way that it should merely deter the offender from further offending by a sanction proportionate to the offense-induced harm (see Bruinsma, 2018).

It took some time until these preventive ideas arrived in the law books and in penal practice. In England and the United States preventive programs became relevant already in the 19th century, while in Germany (with the exception of juvenile penal law)[32] they became influential only in the late 1960s, two decades after the Nazi-Regime had been defeated.

Today, rehabilitative treatment and deterrence form next to retribution (as offense-proportionate and insofar restricted and just punishment) the basic legitimacy of a modern criminal law. The preventive turn also resulted in a further innovation in terms of modern rationality. From then on, the effectiveness of the criminal justice system became an object of empirical investigation. Next to the black letter lawyer, the social and behavioral scientist entered the stage of penal sciences. Under a sole retributive doctrine, a metaphysical construct, the impact of penal sanctions cannot be an object of empirical testing: Punishment finds its reason in itself (just compensating the harm caused by the offense) and not in the crime prevention by treating or deterring an offender (for a modern version, see von Hirsch, 2017).

However, against the backdrop of empirical observations, penal sanctions appeared to be much less promising in preventing further offending than the rather optimistic modern reformers had expected (for a more recent review, see Sherman et al., 1997). This was apparently one reason for

[31] Amsterdam working houses (tuchthuis; Krause, 1999).
[32] In 1923, a special, education-centered law for dealing with juvenile offenses was enacted as *Reichsjugendgerichtsgesetz* (RJGG). The basic architecture of this law is still in force in the current *Jugendgerichtsgesetz* (JGG).

the broad attention given to the alternative theoretical perspective of labeling, which assumes that penal interventions do not prevent but reinforce or even initiate delinquent behavior.

With the methodological progress of panel studies in developmental and life-course criminology, in particular during the last three decades, scholars received the appropriate tools to analyze the causal impact of penal sanctions using quasi-experimental designs. Nevertheless, these sophisticated studies did not result in strong evidence on preventive or promoting effects of penal sanctions either. In general, direct preventive as well as promoting effects are at best weak. However, today, the different theories on sanctioning effects assume mainly a mediated causal process (see Bernburg, 2019; Krohn et al., 2014; Paternoster, 2018): Penal sanctions may lead to an at maximum moderate increase in subjectively perceived detection and sanctioning risks (deterrence); or may support programs which combine the (re-)construction of social bonds with the promotion of cognitive agency (rehabilitative treatment); or may disturb prosocial structural resources and support a delinquent self-concept (labeling). Empirical results appear to support these contradicting assumptions about a mediated impact in one way or another, lending somewhat more evidence to delinquency promoting than preventing mechanisms (Barrick, 2014; Huizinga & Henry, 2008).

While these studies focused on the impact of formal controls on delinquent behavior, a few other studies also found a different impact of formal controls: Controls may also increase the risk of further formal controls, independent of changes in delinquent behavior ("secondary sanctioning"; Liberman et al., 2014).

The current study uses, for the first time in a comparative framework, data from two panel studies that have been conducted in England and Germany: the Peterborough Adolescent and Young Adult Developmental Study (PADS+) and the study Crime in the modern City (CrimoC), carried out in Duisburg. The goal is to analyze whether the impact of formal control interventions vary in different juvenile justice regimes. In particular, we want to broaden the knowledge on processes of secondary sanctioning and will therefore not analyze mediated effects here (for this, see Kaiser, 2022).

After discussing the theoretical framework in the light of prior research, emphasis will be placed on the relevant differences between the English and German juvenile justice system. Against this background, the impact of the formal control interventions during adolescence will be analyzed with the panel data from Peterborough and Duisburg.

2.2 | Theoretical framework and previous research

In this study we will investigate whether a criminal justice intervention is associated with changes in young people's future offending (Figure 2.1). We will also explore whether a criminal justice intervention amplifies the risk for a future criminal justice intervention, and we will compare these two kinds of outcomes (future offending and criminal justice intervention).

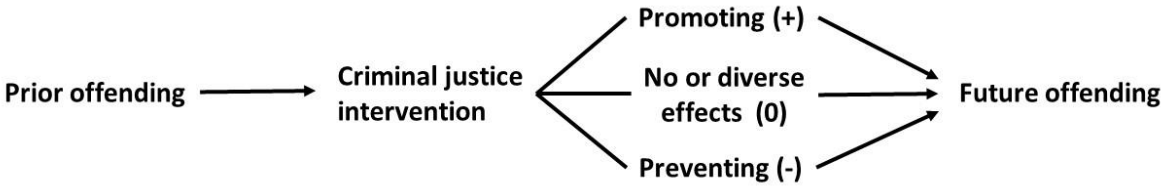

Figure 2.1: Criminal justice interventions and potential impacts on offenders' future offending

There are two major theories of why criminal justice interventions may affect people's future offending: deterrence theory and labeling theory. In the literature studying the association between criminal justice interventions and future offending, it is common to assume (and sometimes conclude) that increases in future offending are indicative of a *labeling process* and that decreases in future offending are indicative of a *deterrence process*. However, establishing whether young people's future offending is amplified or reduced (or unaffected) by a criminal justice intervention does not *in itself* answer the question *why* this happens: "neither increases nor decreases in levels of delinquent involvement following the imposition of sanctions provides unequivocal evidence for either the labeling or deterrence paradigms" (C. W. Thomas & Bishop, 1984, p. 1229). Few studies have attempted to and effectively explored the proposed mechanisms theorized as responsible for an association between criminal justice interventions and future offending (i.e., they are typically assumed rather than demonstrated; see Huizinga & Henry, 2008). Although one can argue that if there are no strong changes (increases or decreases) in future offending, there is no evidence of (no room for) strong *unidirectional* deterrence or labeling influences. However, this does not exclude the possibility of the existence of *diverse influences* that may more or less cancel each other out and, hence, may hide the existence of important multidirectional influences, i.e., that criminal justice interventions may have differential effects on future offending for different people, e.g., depending on their receptivity to the kind and circumstances of the intervention and its aftermath: "effects of interventions are characterized not by homogeneity but by heterogeneity" (Cullen & Jonson, 2014, p. 65). In line with this, a review of research shows that the most frequent finding is the *absence of a statistically significant association* between a criminal justice intervention and future offending, although it is somewhat more common with a finding of increased rather

than decreased offending among the results that are statistically significant (Barrick, 2014; Kleck & Sever, 2017)[33]. A result that does not lend much support to the existence of a strong universal unidirectional labeling *or* deterrent effect (especially since statistical significance in these studies typically does not equal strong effects; ibid.; Huizinga & Henry, 2008; Pratt & Turanovic, 2018) but leaves room for the possibility of important multidirectional influences (e.g., depending on personal characteristics, life-circumstances, stage in a criminal career, and the kind and execution of the intervention).

2.2.1 | Deterrence experiences

Deterrence may be regarded as a *situational* concept, influencing action-choices that make potential offenders refrain from an act of crime they consider. It is commonly assumed that effective deterrence is the outcome of a rational choice, a calculation that the benefit resulting from committing a crime is less than its costs (i.e., that the perceived risk of getting caught and its anticipated consequences outweigh the expected gain). Hence, it is often assumed that providing sufficiently certain, severe, and swift punishment (costs) through the intervention of the criminal justice system will help prevent criminal behavior (see G. S. Becker, 1968; Nagin, 2013; Paternoster, 2018). However, deterrence does not need to involve any cost-benefit calculation to be effective. It is enough that the would-be offender refrains from a contemplated crime due to fear or worry about its consequences. Deterrence is only a causative factor for people who consider, and when they consider, breaking a rule of law (e.g., Nagin, 2013; Pogarsky, 2007; Wikström et al., 2012). For people who do not see a crime as an action alternative (which is likely to apply to most people most of the time), deterrence plays no role for their action-choices (Wikström, 2019b). The extent to which would-be-offenders' action-choices are influenced by deterrent cues may vary depending on factors like their personality (e.g., impulsivity), strength of their social bonds to conventional society and temporary psychophysiological states such as intoxication or high levels of emotion or stress.

In the analysis of the impact of criminal justice interventions on future offending and the role of deterrence, it is central to distinguish between deterrence and deterrence experiences (Wikström, 2008). *Deterrence experiences* is a *developmental* concept. It refers to how (cumulative) past

[33] "A quantitative assessment of studies examining the impact of arrest, conviction, juvenile justice intervention, and incarceration on recidivism provides modest support for the hypothesis that official sanctions, *in certain situations*, *may increase* subsequent deviance" (Barrick, 2014, p. 110; italics added).

criminal justice interventions may influence concurrent deterrence sensitivities[34] (e.g., current assessments of the risk of getting caught and its consequences). Research suggests that people's deterrability may vary depending on personal factors and life-circumstances and their stage in a criminal career (see, e.g., Schulz, 2014; K. J. Thomas et al., 2013; for an overview, Apel, 2022), i.e., some offenders, at some stages and circumstances of life, may be more easily influenced by criminal justice interventions than others.

Deterrence experiences may either increase or decrease a person's sensitivity to deterrence cues (or stay irrelevant) by altering risk calculations or, more generally, affecting levels of worry and fear of consequences. While deterrence experiences are mostly discussed in terms of their efficacy to prevent crime, it is not implausible that they in some circumstances also may promote future offending. Some research suggests that those inexperienced in committing crime tend to "have unrealistically high expectations of sanction risks" (Nagin, 1998, p. 13). It is, therefore, also possible that some (or repeated) deterrence experiences may reduce people's deterrence sensitivity if the consequences are less than expected. This means that official contacts with the criminal justice system may increase their future crime propensity and, hence, their likelihood to commit crime. The point being that increased offending after a criminal justice contact may not only, or exclusively, be a question of any 'labeling' effect but may also be due to so-called experiential effects. We know from previous studies of the PADS+ and CrimoC cohorts that the assessment of the risk of getting caught decreases with the amount of previous self-reported offending (Hirtenlehner & Wikström, 2017; Kaiser et al., 2021; Seddig et al., 2017).[35] Therefore, it may not be implausible that experiences of the consequences of criminal justice interventions can (further) reduce young people's deterrence sensitivity if the repercussions of a criminal justice intervention are experienced as milder and less consequential than anticipated. Without measurements tapping into whether primarily labeling or experiential processes are in operation, it may be difficult to conclusively argue, as is commonly done, that an increase in offending after a criminal justice intervention is primarily due to labeling influences.

From the point of view of criminal policy, criminal justice interventions are not only a question of the efficacy of specific (individual) deterrence but also of *general deterrence*, an aspect that is difficult to study but needs to be taken into consideration when assessing the overall preventive function of criminal justice responses (Andenaes, 1974). It is fully possible that criminal justice interventions may have no or little impact on the future crime involvement for the individual

[34] In the literature, this is commonly referred to as "sanction threat perception," e.g., Paternoster (2010).
[35] This is also a finding commonly reported in other studies, e.g., Carmichael et al. (2005).

subjected to an intervention, while still having a generalized impact on the crime propensity and crime involvement of others, i.e., observers of criminal justice interventions and their consequences (von Hirsch et al., 1999). However, this is nothing we will explore in this study.

2.2.2 | Labeling experiences

Labeling theory is based on the premise that "being labelled as a criminal offender may trigger processes that reinforce or stabilize deviant behavior, net of the behavioral pattern and the social and psychological conditions that existed prior to labeling" (Bernburg, 2019, p. 179). The common idea how this works is "that official labeling affects delinquency indirectly through social exclusion/attenuated bonds, delinquent identity, and involvement with deviant peers" (Wiley et al., 2013, p. 940).

A common classic labeling idea appears to be that all young people commit acts of crime (delinquency), but some are unlucky (or more likely to be targeted) and get caught, and this results in them being officially labeled as criminals (delinquents). This may set them off on (or amplify) a criminal career, where a main suggested reason for why this happens is that the labeling becomes a self-fulfilling prophecy causing them to become more crime-prone by taking on (or strengthening) an identity (self-concept) as a 'criminal' or 'delinquent' (see, e.g., H. S. Becker, 1963; Lemert, 1967; Tannenbaum, 1938). However, the empirical evidence for this suggested process is limited. Few studies have aimed to directly study changes in self-concept (e.g., Huizinga et al., 2003; C. W. Thomas & Bishop, 1984), while most studies that have attempted to address this mediating process have typically investigated, as a self-concept proxy, changes in offenders' attitudes to crime and delinquency following a criminal justice intervention. Findings of a more positive attitude to crime and delinquency is commonly interpreted as an indicator of the development or reinforcement of a 'criminal' identity (e.g., Schulte, 2019; Wiley, 2015; Wiley et al., 2013). However, attitudinal changes[36] are not a straightforward proof of the existence and importance of a labeling process. Changes in people's attitudes towards crime and delinquency may not only be affected by criminal justice intervention experiences but may, more broadly, be a result of changes in other influences on the personal and environmental factors that impact a person's crime propensity (e.g., Wikström et al., 2022). Even in studies where the researcher controls for pre-existing (pre-intervention) personal and environmental factors (e.g., through propensity score matching), these factors—including attitudes—may substantially change over the study period (unrelated to

[36] Which may also include various measures such as those referring to guilt feelings and neutralizations.

any impact from the intervention) and hence affect the outcome (future offending). The measurement of pre-intervention controls and outcomes (future offending) is typically many years apart (e.g., Wiley et al., 2013), something that may be particularly problematic if the study covers developmentally intense periods, such as adolescence, where a lot takes place in young people's lives and many important crime propensity relevant changes in their personal development and life-circumstances (unrelated to any effects from criminal justice interventions) may take place. This is obviously a problem not restricted to analyses of labeling effects but also relevant in studies on deterrence effects.

There are reasons to believe that young people's risk of getting caught when offending is primarily a rational rather than random (or targeted) process. Previous research from the PADS+ study (Wikström et. al., 2012) shows that although most (70%) young people report having committed a crime, most reported no or only an occasional crime involvement over a period of 5 years (66% reported no [30%], or one or two [36%] mostly minor crimes during ages 13-17), and the bulk of the cohort's crime was committed by a small group (3.8% were responsible for 47% of the cohort's crime during ages 13-17). Moreover, and importantly, the findings also show that the more crimes a young person self-reported the more likely it is that she or he also had been subjected to a criminal justice intervention (Pearson's r = 0.60), and that the two self-reported crime categories that predicted having an official contact the strongest (caution, conviction) were more serious crimes of theft (i.e., thefts of and from cars and residential burglary). In other words, it is primarily those who are frequent and more serious offenders that are at the greatest risk for a criminal justice intervention. This is further illustrated by the fact that the PADS+ participants who had experienced an official intervention by the criminal justice system (caution or conviction) during ages 13-17, on average, reported 45 crimes in the years before the year of their first official contact. Therefore, many young people who encounter a first criminal justice contact have already embarked on a 'criminal career' (or, at least, a life of repeated crime involvement). However, these facts do not exclude the possibility that a criminal justice intervention may influence young people's crime propensity but show that it is not 'normal' (common) for them to engage in crime and that many of those experiencing a first criminal justice intervention already have started to perpetrate a criminal career.[37]

Another more recent idea how labeling might work (often referred to as structural labeling) is the idea that becoming officially identified as a 'criminal' has a negative impact on the offender's

[37] Acts of crime and delinquency before the age of 13 are not included in this calculation, so, if anything, it is an underestimation of pre-intervention involvement in crime and delinquency.

integration into mainstream society: “official labeling in particular, is seen as a transitional event that can substantially alter the life course by reducing opportunities for a conventional life” (Bernburg & Krohn, 2003, p. 1288). It seems a reasonable hypothesis that becoming officially known for crime, especially for frequent and more serious offending, may impact a young person’s conventional life-chances (e.g., affect, legally and otherwise, educational and employment possibilities, and weaken existing, and impede or discourage the development of new attachments to conventional others) and, therefore, also make desistence more difficult. There are plenty of studies suggesting the possibility of such mediation effects (e.g., Bernburg et al., 2006; Bernburg & Krohn, 2003; Krohn et al., 2014; Laub & Sampson, 2003; Lopes et al., 2012; Sampson & Laub, 1993, 1997; Schulte, 2019; Wiley et al., 2013).

2.2.3 | Diverse intervention effects are to be expected

Given what we know about the impact of criminal justice interventions on future offending and the causes of crime, it is reasonable to expect that criminal justice interventions are likely to have diverse effects. People are different and their reactions to criminal justice interventions may vary as do the kind and content of the criminal justice interventions they encounter and react to. Some people may be more easily deterred and others more defiant when experiencing particular criminal justice interventions, meaning that the direction of influence may go both ways (either promoting or preventing further offending). Some people may have more to lose than others and, therefore, may be more easily deterred, and some may be less affected by (additional) labeling (e.g., because they are already well-known as offenders and already live outside ‘conventional’ society). All this, and other relevant differences, may result in an overall lack of effect, or in an only small unidirectional effect, obscuring that the influence on future offending by criminal justice contacts may have important effects in different directions for different people (e.g., depending on their personality, amount of previous criminal justice contacts, the content of the intervention and its social context). It is therefore easy to agree with Sherman (1993, p. 445) when he argues that the question whether criminal justice interventions promote or prevent future offending is wrongly put: “Widely varying results across a range of sanction studies suggest a far more useful question: under what conditions does each type of criminal sanction reduce, increase or have no effect on future crimes.”

2.3 | Juvenile justice systems in England and Germany

A starting point of our study was the observation that the English juvenile justice system appeared to be more interventive in its reaction to youth crime than the German system. The following discussion—based on the law in effect in 2006—will focus on the two key differences between both systems that likely had the most impact on our study samples: diversion and police practice.

Diversion. In both systems, the most common reaction to a juvenile's offense is to divert the case from formal proceedings. While the diversion rates in England as well as in Cambridgeshire were remarkably lower than in Germany and Duisburg, respectively (cf. Table 2.1), they were, for reasons unknown, quite similar in the study samples (PADS+: 73%; CrimoC: 80.7%).

Table 2.1: Reaction of the JJS in England, Germany, Cambridgeshire, Duisburg, PADS+, CrimoC

Reaction	Nation		Country		Study sample	
	ENG	GER	CAM	DU	PADS+	CrimoC
Diversion	51.4%	76.9%	51.9%	74.7%	73.0%	80.7%
Conviction	48.6%	23.1%	48.1%	25.3%	27.0%	19.3%
Non-custodial measures	93.6%	76.2%	93.2%	75.8%	90.0%	82.4%
Shortterm juvenile detention	--	20.1%	--	21.6%	--	17.6%
Juvenile imprisonment	6.4%	3.7%	6.8%	2.6%	10.0%	--

Notes: n(PADS+) = 37; n(CrimoC) = 87 (missing data for one participant);
Source: Ministry of Justice 2018a; 2018b; IT.NRW 2017; Statistisches Bundesamt 2009, partly own calculations.

The lower rates in England, however, cannot be explained by differences in the severity of criminal offending (see online supplementary material S1), but partly by different diversion schemes. In England, a formalized, strict two-tiered system of *reprimands* and *(final) warnings* was established in 1998 (Bottoms & Dignan, 2004). If admitting guilt, a first-time offender having committed a minor offense could be given a formal reprimand instead of being prosecuted. In the case of a more serious offense or a previous reprimand, the offender could be given *one* (final) warning, combined with the voluntary participation in a rehabilitation program. The number of diversions was hence severely limited, so that many juveniles, who otherwise might have been suitable diversion candidates, were drawn into the formal court system. The decision to divert the case was made at the police level at the discretion of the officer handling the case, who also administered the formal reprimand or warning, usually in uniform at the police station and in the presence of the juvenile's parents. The juvenile was thus inevitably exposed to the institutional police setting and experienced—even for an informal reaction—a rather intensive system contact (§ 65(1)-(4), § 66(1)-(3) CDA 1998; Dignan, 2010; Evans & Puech, 2001; Goldson, 2000; Home Office & Youth Justice Board, 2002).

In Germany, a less strict three-tiered system of diversion gives the youth prosecutor broad discretion to divert a case. Of all diverted cases, 94% are closed with only minimal system contact.

Proceedings against a first-time offender with a petty offense are usually dismissed without any intervention at all. In case of more serious or repeated offending, dismissing the case requires some form of educational intervention by the youth's parents, school, or employer. Diversion is possible even for a medium offense but conditioned on the youth admitting guilt and performing some educational measures ordered by the juvenile court judge (e.g., unpaid community work, § 45(1)-(3) JGG; Dünkel & Heinz, 2017).

Police practice. The differences between English and German diversion regimes are further exacerbated by a divergent style of policing in England and Germany. While German police mostly react to reported (youth) crime (Eisenberg & Kölbel, 2017), English police acted much more proactive between 2002 and 2007. This only short-lived change in English policing was due to the adoption of the *Offenses Brought to Justice Target* in 2002, which required the justice system to increase the number of offenders receiving a formal reaction by 20% within the next five years (Bateman, 2015; Office for Criminal Justice Reform, 2004). To meet this target, police focused primarily on crimes committed by young people as they are usually easier to detect. Between 2002 and 2006 the rate of first-time juvenile entrants into the justice system thus increased in Cambridgeshire by 53% (Peterborough: 57.8%, nationwide: 29.2%), the arrest rate by 48.2% (nationwide: 31.4%). The resulting criminalization of a large number of youths resulting from this change in policing style hit young first-time low level offenders disproportionately hard, since a substantial amount of their cases had previously been dealt with informally outside the court system, e.g., a stern talking-to by the police officer on the spot (Bateman, 2015, 2017; Flanagan, 2007; Ministry of Justice, 2010, 2017; R. Morgan, 2007; R. Morgan & Newburn, 2012; Newburn, 2011).

Sanctions & Sanctioning. The sanctions available to the juvenile court were quite similar in England and Germany, ranging from a wide variety of educational orders (e.g., social training, community work, fine, victim-offender reconciliation, attending school, curfew) to confinement. The English juvenile courts made almost exclusive use of non-custodial measures (93.6% nationwide, 90% in the PADS+-sample), while German courts also resorted to the only here available short-term custody ("Arrest," 20.1% nationwide, 17.6% in the CrimoC-sample) (Albrecht, 2002; Bottoms & Dignan, 2004; Dignan, 2010; Dünkel & Heinz, 2017).

In sum, the English juvenile justice system was based more on early and intensive intervention than the German system, especially regarding diversion and the policing style. With the English police looking proactively for youth crime, many young first-time offenders were drawn into the formal justice system. Mainly due to the more rigid and formal diversion scheme, the exposure to the institutional police setting was also more intensive than in Germany. Due to the also severely

limited possibility of a repeated diversion decision in England, many juveniles were driven deeper into the formal court system. As a consequence, young offenders in Peterborough were more likely to be inexperienced and to having had more intensive system contacts even in case of diversion compared to those in Duisburg.

2.4 | Hypotheses

Following the current state of research, (1) the findings on the overall not strong impact of formal controls on subsequent delinquent behavior are quite mixed (see Barrick, 2014; Huizinga & Henry, 2008; Motz et al., 2020). There is somewhat more support for delinquency-promoting rather than delinquency-preventing effects, while there are also many insignificant findings. (2) Comparing the English and the German juvenile justice system, official responses towards delinquent behavior were—at least in the 2000s—somewhat less lenient in England. (3) Regarding an institutional impact, formal controls may increase the risk of subsequent controls.

Research has two central shortcomings: First, only few empirical studies on formal control effects have been conducted in Europe (for exceptions, see Huizinga et al., 2003; McAra & McVie, 2007; Murray et al., 2014). Second, most studies are solely based on official police or court data as behavioral proxies of reoffending (Barrick, 2014), i.e., official registrations are taken as individual behavior. To address these shortcomings, the current study is—as far as known—the first comparative investigation of secondary deviance and secondary control effects within two different European jurisdictions. Against this background, we will investigate the following hypotheses:

Hypothesis 1: Formal controls are more likely to increase rather than decrease later delinquency.

Hypothesis 2: Since the English Juvenile Justice System reacts somewhat less lenient towards delinquent behavior, delinquency-promoting effects of formal controls are more likely among the Peterborough juveniles.

Hypothesis 3: Formal controls increase the risk of subsequent formal controls.

2.5 | Formal control effects in Peterborough and Duisburg

2.5.1 | Samples

Our analyses are based on data from the *Peterborough Adolescent and Young Adult Development Study* (PADS+; Wikström et al., 2012) and the *Crime in the modern City study* (CrimoC; Boers et al., 2010). Both are panel studies that started data collection with 13-year-old school students in Peterborough and Duisburg. Participants were asked to complete standardized questionnaires. In addition, researchers collected the participants' police and court records.

The target population of PADS+ covered all 11-year-old school students who lived in Peterborough and entered year seven in 2002. After sampling randomly, 710 juveniles (approximately one third of the population) finally participated in the first wave in 2004. In the follow-up waves—that were conducted annually until age 17 and in two and three-year intervals thereafter—PADS+ achieved retention rates of more than 95% (707 in wave 2, 703 in waves 3 and 4, and 693 in wave 5). *Police National Computer* (PNC) records were collected for 700 students.[38]

In CrimoC, researchers tried to survey all 7th-graders in Duisburg in 2002. Out of 56 schools, 40 (71%) agreed to participate, resulting in 3,411 completed questionnaires at wave one (approximately two thirds of all 7th-graders). The follow-up waves were conducted annually until age 20 and then biennially until age 30. The difference in design resulted in somewhat more unit-non-responses in CrimoC compared to PADS+, although participation was also high (3,392 in wave 2, 3,339 in wave 3, 3,405 in wave 4, and 4,548 in wave 5).[39] Official records were available for 2,964 respondents (87%).[40]

To establish proper time order for causal inference, three time periods were defined (see Liberman et al., 2014; Wiley & Esbensen, 2016): pretreatment (T1; covariates), treatment (T2, i.e., official contact), and post-treatment (T3; outcomes: self-reported delinquency and official contact). Table 2.2 shows how the PADS+ and CrimoC data were aligned with these three periods.

[38] For more information on PADS+, see https://www.cac.crim.cam.ac.uk/research/padspres or Wikström et al. (2012).
[39] Wave 5 included also students from vocational schools who participated for the first time.
[40] Official records (received from the *Bundeszentralregister* (BZR) and *Erziehungsregister* (ER)) are based on court and prosecution data and comprise all decisions made after opening an official investigation by police: from dismissal in case of lacking evidence up to convictions. – Number of respondent refers to wave 4 when official records were collected; for more information on CrimoC, see https://www.crimoc.org or Boers et al. (2010).

Table 2.2: Time order

		Time period measures refer to in		
Phase	**Ø-age**	**Pads+**	**CrimoC**	**Measures**
T1	14	01/ - 12/2005	01/2003 - 02/2004	Covariates
T2	15	01/ - 12/2006	03/2004 - 12/2004	Official contact
T3	16	01/ - 12/2007	01/2005 - 02/2006	SRD, Official contact

Notes: CrimoC's treatment period (T2) is shorter to take into account that some covariates (e.g., self-reported delinquency, SRD, in T1) refer to the time period from January 2003 to January/February 2004, whereas comparable measures in PADS+ refer only to whole years (e.g., whole year 2005).

In order to be included in the final analyses, participants from both studies had to meet two conditions: (1) participation in waves 3, 4, and 5, as well as (2) access to their official records. All in all, 690 juveniles in PADS+ (97% of 710), and 2,117 in CrimoC (62% of 3,405)[41] matched these criteria. In CrimoC, the resulting sample consists of somewhat less 'high-risk youth' than the original sample.[42]

2.5.2 | Measures

Our measurement descriptions follow the division into the three (quasi-)experimental time-periods: treatment, outcomes, and covariates.

The *treatment* variable is official control, a binary variable distinguishing between those with "no-official contact" (= 0) and those with "official contact" (= 1). In PADS+, 37 of 690 (5.4%) and in CrimoC 88 of 2,117 (4.2%) juveniles had been officially registered for an offense within T2. In both samples, official intervention was generally not very intensive. Usually, juveniles were diverted out of the system or received some form of educational measures (see Table 2.1).

Outcome variables are self-reported delinquency (SRD) indices and official contact measures (PADS+: PNC; CrimoC: BZR, ER). The pool of SRD indicators consists of *nine* (PADS+) or *thirteen* (CrimoC) offenses, respectively, committed *in the last year* (PADS+) or *since the start of the last year* (CrimoC). Although the number of offenses varies between PADS+ and CrimoC, they cover the same categories of delinquent behavior. On the one hand, SRD indicators were used to calculate prevalence rates of general, violent, and property offenses as well as vandalism.[43] On

[41] The sample size of wave 4 was selected because in this wave respondents were asked to consent to a collection of their official data.

[42] Among those not included in the final CrimoC sample, the reported level of self-reported delinquency, police-related problems, deviant peer group activities, and school performance problems was somewhat higher; for more information, see online supplementary material S2b.

[43] In PADS+, offending categories consist of the following offenses: (1) violence = assault, robbery, (2) property offending = shoplifting, theft from car, theft of car, theft from person, residential burglary, non-residential burglary,

the other hand, in order to measure offending intensity, versatility scores were computed (with a maximum of 9 or 13 different offenses in PADS+ or CrimoC, respectively).[44] In addition, official control (0 = no contact; 1 = contact) within T3 was also considered as an outcome variable in order to analyze effects of "secondary sanctioning" (Liberman et al., 2014).

Covariates. For each study site, the selection of more than 50 covariates was guided by theoretical considerations and empirical evidence. Consequently, they cover a wide range of variables known to be related to offending or an official contact: deviant and delinquent behavior, previous formal controls, individual characteristics, peer, family and school bonding, parental education, neighborhood and demographics. Including multiple indicators is regarded as a promising strategy to tackle selection bias threats effectively (Steiner et al., 2010). SRD and official control measures in T1 are also included as covariates because matching on them assures that the treatment and the control group are balanced on the focal variables of the current study at baseline.[45]

2.5.3 | Analytical procedure

Methodologically, the crucial point in analyzing formal control interventions is to avoid selection bias: To make sure that post-intervention differences between an intervention and a control group are based on the intervention only, both groups should not differ on other characteristics (so called covariates), following ideally the ceteris paribus-rule. This can best be achieved by an experimental research design based on a random selection of both groups. However, for legal reasons, police, prosecutors or judges are not allowed to decide randomly whether to intervene or not to intervene in delinquent behavior. Therefore, formal control interventions can typically only be investigated within a quasi-experimental setting. Here, one tries to minimize selection bias by controlling statistically for confounding covariates (see S. L. Morgan & Winship, 2015; Shadish et al., 2002). It was common practice to rely on multiple regression analysis to address threats of selection bias (see Nagin et al., 2009). After it turned out, however, that multiple regression is not efficient enough in controlling for confounding effects of covariates (Smith & Paternoster, 1990), Propensity Score Matching (PSM) has been applied as a more appropriate tool of accounting for

(3) vandalism; in CrimoC: (1) violence = assault without weapon, assault with weapon, robbery, bag snatching (2) property offending = shoplifting, theft from car, theft of car, bicycle theft, theft from person, burglary (3) vandalism = property damages, graffiti spraying, scratching.

[44] Another common way to measure intensity is to compute frequency rates (number of offenses per offender; Blumstein et al., 1986). We used versatility scores because they have better statistical properties (Sweeten, 2012), and produced more precise estimates than frequency rates with the current data.

[45] For a complete list of covariates, see online supplementary material S2a and S2b.

selection effects (McAra & McVie, 2007; Morris & Piquero, 2013; J. T. Ward et al., 2014; Wiley et al., 2013; Wiley & Esbensen, 2016).

To explore how a contact with the juvenile justice system affects the risk of reoffending and further official contact, we use PSM to estimate the Average Treatment Effect on the Treated (ATT) as our causal estimate of interest. Derived from the potential outcome model (see S. L. Morgan & Winship, 2015; Rubin, 1974), the ATT is computed in the following way:

$$ATT = E[\delta \mid Tr = 1] = E[Y_i^1 - Y_i^0 \mid Tr = 1].$$

The ATT refers to officially treated individuals only (Tr = 1). It is defined as the average (E[])[46] difference (Y_i^1-Y_i^0 = δ) between their observed reoffending (Y_i^1) and "their" hypothetical reoffending, i.e., under the assumption that they would not have been treated (Y_i^0). In reality, a treated individual experienced only the treatment condition (official contact) and not the control condition (no contact). Hence, only one (Y_i^1) of the two potential outcomes (Y_i^1, Y_i^0) can be observed. Consequently, causal effects cannot be computed from the observed values of the treated individuals alone. This missingness of the counterfactual outcome value (Y_i^0 as the value not realized in reality) is termed the 'fundamental problem of causal inference' (Holland, 1986).

To overcome this problem and estimate ATTs, we applied PSM. Matching (including weighting) procedures generally mimic a randomized experiment by balancing the treatment and control group on an array of covariates selected for matching (S. L. Morgan & Winship, 2015; Stuart, 2010). They do so by finding and matching control units that are equal (exact matching) or at least most similar to treated units on all selected pretreatment measures. Individuals from the control group that are too dissimilar to the treated individuals are excluded from analyses. Included individuals from the control group are finally used to infer the counterfactual outcome value, allowing for an ATT estimation. Unlike a randomized experiment, matching, however, does not automatically balance unobserved characteristics of treated and untreated individuals. Furthermore, classical matching procedures were based on exact matchings, i.e., finding individuals for the control and treatment group with exactly the same values. However, the higher the number of covariates the less likely it is to meet such a requirement ("curse of dimensionality," Apel & Sweeten, 2010, p. 544). To face this problem, Rosenbaum and Rubin (1983) introduced the so-called propensity score. It refers to the probability that an individual received the treatment. For this study, the propensity score describes the probability that a juvenile was officially recorded for an offense in T2. A great advantage of this single score is that matching on it (i.e., finding individuals with most

[46] E[] is the probability theory's expectation operator.

similar propensity scores among treated and untreated respondents) may be sufficient to balance the treatment and control group on all pretreatment covariates (Kainz et al., 2017).

Our matching procedure followed four steps (Stuart, 2010): First, we estimated propensity scores for each PADS+ and CrimoC sample member with the help of three different estimation procedures: Bayesian Logistic Regression (BLR; McElreath, 2016), Bayesian Additive Regression Trees (BART; Chipman et al., 2010), and the Covariate Balancing Propensity Score (CBPS; Imai & Ratkovic, 2014).[47] Second, these three propensity scores were applied in 12 different matching (or weighting) algorithms to find the combination that leads to the best distributional balance of all covariates between the treatment and control groups.[48] The application of different combinations of propensity score and matching algorithms is recommended to ensure that selection threats induced by pretreatment differences in observed covariates are minimized (e.g., Kainz et al., 2017; S. L. Morgan & Winship, 2015). Third, we selected the best PSM procedure for each sample by assessing the covariate balance achieved by the different method combinations using recommended balance statistics (Kainz et al., 2017; see section Covariate Balance Assessment). Fourth, we applied regression models (R's Zelig package; Imai et al., 2008) to the best-matched samples to estimate ATTs and simulate their uncertainty. While binary SRD prevalence and official contact outcomes were modeled by logistic regression, SRD versatility indices were predicted by Poisson models.[49]

Because the investigated samples suffered from item-non-response, all analytical steps were applied to multiple imputed data sets. Multiple imputation embraces the estimation uncertainty emerging due to missing information in the data set (van Buuren, 2018). It generates multiple data sets by making multiple predictions for the missing values using observed information from other variables. As recommended by Penning de Vries and Groenwold (2017), we conducted matching, the generation of weights and also the outcome analyses for each imputed data set. The imputed

[47] For estimation of the propensity score, we included (a subset of) the covariates (36 in PADS+, and all 52 in CrimoC) as predictors in each modeling procedure. All computations were conducted in R (version 3.5.2; R Core Team, 2018). A list of all R packages used for the analysis is provided in online supplementary material S5.

[48] The 12 different matching (weighting) algorithms were (Stuart 2010): (1-5) nearest neighbor matching with replacement, a caliper of 0.25, and ratios of 1:1 to 1:5, (6-9) optimal matching with ratios 1:1 to 1:3, (10-11) genetic matching with replacement and ratios 1:1 to 1:2, and (12) weighting by the odds.

[49] As predictors, the models included the treatment variable (official contact in T2), the lagged outcome and their interaction term: $\text{Outcome}_{T3} = \alpha + \beta 1\ \text{Treatment}_{T2} + \beta 2\ \text{Outcome}_{T1} + \beta 3\ (\text{Treatment}_{T2} \cdot \text{Outcome}_{T1})$. Zelig applies the following formula to compute the ATTs from the regression models: $\frac{1}{\sum_{i=1}^{n} T_i} \sum_{i:T_i=1}^{n} \{Y_i^1 - E[Y_i^0]\}$. Within this formula, only the counterfactual values (i.e., Y_i^0) are estimated with the help of the regression models because Y_i^1 is observed for all treated individuals and can, therefore, be directly filled into the equation. To check for the robustness of the outcome analyses (see online supplementary material S4), we conducted not only the aforementioned regression specification but also estimated mean differences (regression models with only the treatment as predictor), and weighted regressions (including a fuller set of predictors).

information was finally combined by merging the ATT simulations of all imputed data sets together.[50]

In addition, we also conducted robustness analyses to check how sensitive the ATTs were in relation to different missing data, propensity score estimation, matching, and outcome modeling procedures (Young & Holsteen, 2016). We restricted our sensitivity checks to those propensity score and matching procedure combinations that were relatively successful in establishing covariate balance between treated and untreated individuals.

2.6 | Results

In this section, we first report how the best-working matching methods balanced the covariate distributions before presenting the ATT estimates and robustness checks.

2.6.1 | Covariate balance assessment

In the following, we assess the covariate balance of the best-balancing matching procedures using standardized bias (SB) and variance ratio (VR) statistics (Kainz et al., 2017). SB is the difference in covariate means between the treated and untreated group divided by the standard deviation of the treated group. VRs, in contrast, inform about the variance differences in continuous covariates across the treated and untreated groups. SB thresholds of larger than 0.1 and VRs larger than 2 or smaller than 0.5 indicate imbalance (Harder et al., 2010; Kainz et al., 2017).[51]

2.6.1.1 | PADS+

In PADS+, treated individuals differed from untreated ones on an array of pretreatment characteristics. The majority of covariates (44 of 57) was imbalanced before matching, showing SB statistics larger than 0.1; for 34 covariates the bias was larger than 0.2 (see Table 2.3 for a selection of focal covariates).[52] Across all covariates, the average absolute SB difference was 0.18 (median:

[50] The CrimoC sample is more strongly affected by missing values than that of PADS+. Thus, we produced only 12 imputed data sets for PADS+ but 70 for CrimoC. We applied predictive mean matching within a fully conditional specification (van Buuren, 2018) and additionally also other imputation procedures (e.g., random forests). These sensitivity checks showed that our results are quite robust to the imputation technique applied (see online supplementary material S2b and S4).

[51] $SB = (Mean_{Treated}\text{-}Mean_{Control})/SD_{Treated}$; $VR = SD^2_{Treated}/SD^2_{Control}$; experts have not yet settled on a SB threshold and some recommend a less strict 0.2-threshold (Harder et al., 2010; Kainz et al., 2017).

[52] For balance statistics of all covariates, see online supplementary material S3.

0.12) and the maximum bias was 1.02. In addition, the average of the VRs of the 19 continuous covariates was 1.75 (median: 1.49). Only 3 of the 20 continuous covariates exceeded the VR threshold of 2, including the SRD versatility index (2.98).

Table 2.3: Covariate balance statistics for PADS+

PADS+	Unadjusted Sample		Adjusted Sample	
	SB	**VR**	**SB**	**VR**
COVARIATES – LAGGED (T1) OUTCOMES				
SRD violence prevalence	.24		- .07	
SRD property prevalence	.33		.00	
SRD vandalism prevalence	.37		.04	
SRD general prevalence	.42		.06	
SRD versatility	.78	2.98	.15	1.25
Official contact prevalence	.13		.03	
GLOBAL COVARIATE BALANCE STATISTICS				
Mean (absolute)	.18	1.75	.05	1.47
Median (absolute)	.12	1.49	.05	1.28
Maximum (absolute)	1.02	4.98	.29	3.03

Notes: SB = Standardized bias; VR = Variance ratio; VRs are standardized in a way that they are always larger than 1, so that only ratios above 2 indicate balance problems; because prevalence covariates are binary, we report raw percentage differences and no VR statistics for them (Kainz et al., 2017).

For PADS+, optimal matching[53] with a ratio of 1:3 without replacement on the linear propensity score estimated via BART resulted in the best covariate balance. This procedure led to adjusted groups of 37 treated and 111 control cases. For this adjusted sample, mean differences and VRs of the covariates declined strongly. The mean and median of the SB statistics decreased to 0.05 and only 16 covariates exceeded the threshold of 0.1 (only one variable had a bias larger than 0.2). VRs declined to 1.47 on average (median: 1.28) and three covariates had a ratio larger than 2. According to the most stringent thresholds, remaining imbalances indicate that in the adjusted sample treated individuals showed still a slightly different delinquency pattern, were slightly more involved with the legal system (antisocial behavior order, ASBO; youth offending teams, YOT), perceived the risk of consequences when caught somewhat lower, reported less deviant peers, a less supporting family environment, and more informal social control in their neighborhood (see online supplementary material S3). Overall, however, the matching procedure decreased the likelihood that differences in pretreatment characteristics confound the ATT estimates.

[53] The procedure matches treated and untreated individuals by minimizing a global distance measure (Hansen, 2004). A ratio of 1:3 indicates that three individuals without official contact (i.e., from the control group) were matched to one treated individual.

2.6.1.2 | CrimoC

In CrimoC's unadjusted sample, covariate differences between treated and untreated individuals were much less pronounced, though still remarkable. The mean of the SB statistics across covariates was already quite low (0.07; median: 0.04) and only 28 of the 57 covariates had a bias greater than 0.1 (only 8 covariates exceeded a threshold of 0.2); the maximum standardized mean difference was 0.35 (see Table 2.4 for a selection of focal covariates).[54] VRs were with few exceptions within an acceptable threshold.

Table 2.4: Covariate balance statistics for CrimoC

CrimoC	Unadjusted Sample		Adjusted Sample	
	SB	**VR**	**SB**	**VR**
COVARIATES – LAGGED (T1) OUTCOMES				
SRD violence prevalence	.10		- .05	
SRD property prevalence	.14		.06	
SRD vandalism prevalence	.07		.01	
SRD general prevalence	.14		- .01	
SRD versatility	.24	1.08	.01	1.12
Official contact prevalence	.10		.00	
GLOBAL COVARIATE BALANCE STATISTICS				
Mean (absolute)	.07	1.70	.00	1.19
Median (absolute)	.04	1.25	.00	1.10
Maximum (absolute)	.35	7.35	.06	1.89

Notes: SB = Standardized bias; VR = Variance ratio; VRs are standardized in a way that they are always larger than 1, so that only ratios above 2 indicate balance problems; because prevalence covariates are binary, we report raw percentage differences and no VR statistics for them (Kainz et al., 2017).

Weighting by the odds[55] on the covariate balancing propensity score (CBPS) resulted in the best-balanced distribution of covariates across the treatment and control group. After weighting, the CrimoC sample included an adjusted number of 205.8 control and 88 treated units. For this adjusted sample imbalances in covariates diminished completely. SB statistics of all variables were below 0.1. Mean and median bias was essentially zero (< 0.01). Additionally, VRs of the 23 continuous variables were also all below a value of 2 and their mean (1.19; median: 1.10) was pleasingly low, too. For CrimoC, we can actually assume that it is very likely that our weighting procedure is capable of preventing potential selection bias due to observed covariates.

[54] For balance statistics of all covariates, see online supplementary material S3.
[55] The procedure weighs the control group up to the treatment group by giving more weight to control individuals who are more similar to treated individuals on the propensity score and less weight to those more dissimilar (Harder et al., 2010).

2.6.2 | Average Treatment Effects on the Treated

ATT estimates for the Peterborough and Duisburg samples tell a quite similar story. Most estimates are statistically insignificant, suggesting that a contact with the juvenile justice system had at best weak effects on the prevalence and versatility of reoffending (for prevalence ATT estimates, see black points and lines and gray shaded area in Figure 2.2). According to the ATT point estimates, the *prevalence* of reoffending typically would have changed by less than 5 percentage points (pp.) had offenders with a system contact instead had no contact (see section *Analytical procedure* for a definition of the ATT).

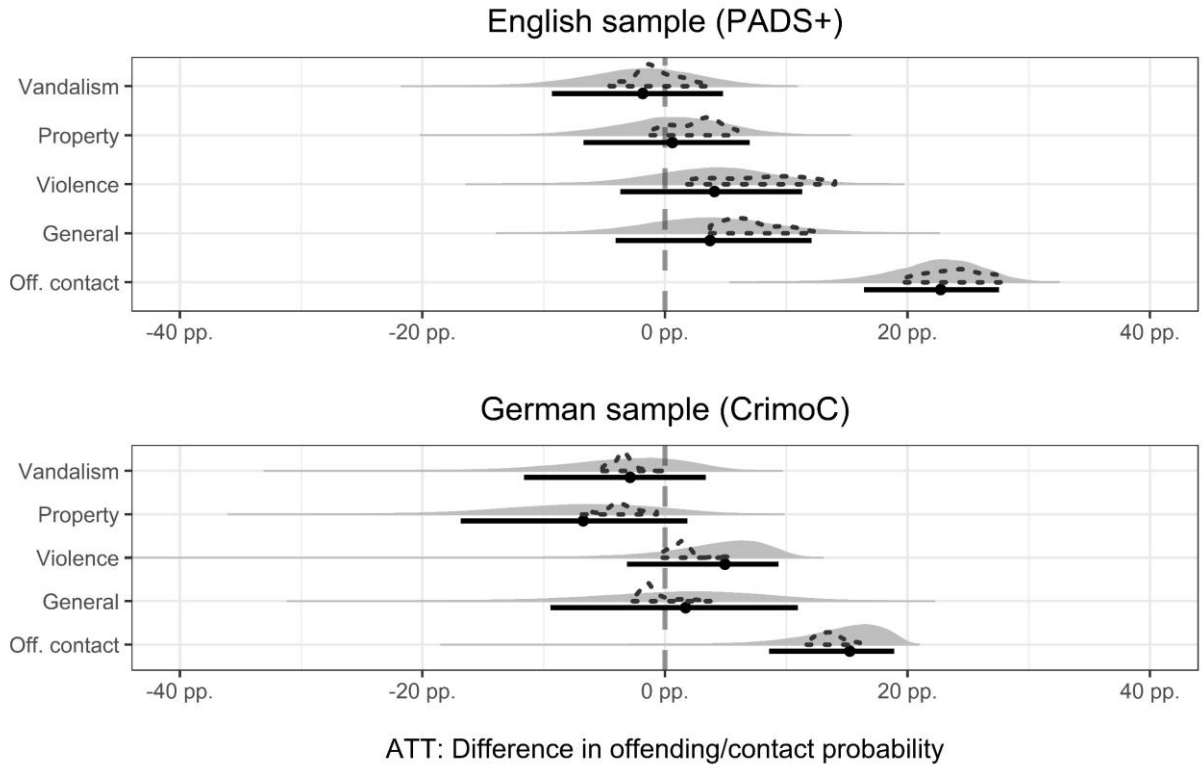

Figure 2.2: Average Treatment Effects on the Treated (ATTs), prevalence rates, Peterborough (PADS+) and Duisburg (CrimoC)

Notes:

Grey shaded area	*= Distribution of ATT simulations of best-balancing model;*
Black dots	*= Medians of ATT simulations of best-balancing model;*
Black lines	*= 89% confidence intervals of best-balancing model;*
Dotted black lines	*= Distribution of medians of the ATT simulations of all candidate models (for sensitivity checks).*

For example, among PADS+ juveniles, an official contact decreased the prevalence of committing vandalism in T3 on average about 2 pp. (ATT = -1.8 pp. [89%-CI:[56] -9.3 pp. 4.8 pp.]), whereas the

[56] Because the typical p-value threshold of 0.05 and confidence interval width of 95% is chosen arbitrarily as a cut-off point in declaring certainty/uncertainty in estimation (McElreath, 2016), we decided against their use. We instead show the full estimation uncertainty by displaying density plots of the ATT simulations (Figure 2.2, gray shaded areas), supplementing them with 89% confidence intervals (Figure 2.2, black lines) to avoid the use of the typical cut-off points. The simulations, hereby, approximate the ATTs full probability distributions (King et al., 2000).

reduction was estimated to be about 3 pp. (ATT = -2.9 pp. [-11.6 pp. 3.4 pp.]) among the Duisburg youths. The probability of property offending decreased slightly but insignificantly in the German sample (ATT = -6.7 pp. [-16.9 pp. 1.9 pp.]), whereas the effect of a system contact on property offending was estimated to be close to null in the English sample (ATT = 0.6 pp. [-6.7 pp. 7.0 pp.]). The likelihood of violent and general offending was somewhat—but again insignificantly—increased due to a system contact in both samples ($ATT_{PADS+.Violence}$ = 4.1 pp. [-3.7 pp. 11.3 pp.]; $ATT_{PADS+.General}$ = 3.7 pp. [-4.1 pp. 12.1 pp.]; $ATT_{CrimoC.Violence}$ = 4.9 pp. [-3.1 pp. 9.4 pp.]; $ATT_{CrimoC.General}$ = 1.7% [-9.5 pp. 10.9 pp.]). The versatility of offending, finally, was barely affected by an intervention of the juvenile justice system. The insignificant ATT estimates indicate that an official contact had probably negligible or only relatively weak effects on the offending variety of adolescents in Peterborough (ATT = 0.04 [-0.24 0.28]) and Duisburg (ATT = -0.09 [-0.38 0.08]).

Despite these at best rather weak control effects on subsequent delinquency, the ATT estimates suggest that an official contact increased the prevalence of a renewed system contact substantially in the follow-up year. While in PADS+ the prevalence of a repeated contact rose by some 23 pp. (ATT = 22.7 pp. [16.4 pp. 27.6 pp.]) due to a prior official contact, the increase was still about 15 pp. in CrimoC (ATT = 15.2 pp. [8.6 pp. 18.9 pp.]).

2.6.3 | Sensitivity of ATT estimates to modeling approach

To compute the ATTs, we applied not only the reported methods (that best balanced the covariates) but tried several different method combinations (varying in the imputation, propensity score, matching, and/or regression procedure). Among these combinations, only those were selected for ATT robustness checks that balanced the covariate distributions well. For each outcome and each of these 36 (PADS+) or 60 (CrimoC) 'candidate' method combinations, we computed ATT point estimates. The distribution of all point estimates was then plotted in density plots (see dotted lines in Figure 2.2). Overall, the density plots suggest that the ATT estimates are relatively robust to changes in the analytical procedure. However, ATT estimates are somewhat more model sensitive in the English than the German sample, probably because of PADS+'s smaller sample size and stronger imbalance before matching. This is especially true for the general and violent offending prevalences as well as for the SRD versatility index. For these three outcome variables, most

alternative method combinations produced ATT estimates that indicated somewhat more substantial (and in some cases significant) system contact effects than those reported above.[57]

2.7 | Conclusion

Do criminal justice interventions promote or prevent young offenders' future offending? This is the main question addressed in this research with significant implications for crime prevention policy and practice. Although it is commonly assumed that increases in young people's offending after criminal justice contacts is evidence of some form of labeling and that decreases in their offending after such contacts is evidence of deterrent effects, the interpretation of these relationships are clearly not as simple as that (see *Theoretical Framework and Previous Research*).

What is studied here are short term effects of (previous year) criminal justice interventions (mainly diversion measures like cautions, community work; some convictions) on future (next year) offending and criminal justice interventions, controlling for selected key background factors through propensity score matching (including previous frequency of criminality). Most initial criminal justice contacts are first-time criminal justice interventions. The study does not explore (and therefore does not exclude) whether *repeated* official criminal justice contacts (or the extent of such contacts) tend to gradually promote (amplify) or prevent (reduce) an offender's future offending. The key results of the study are summarized in the three key points below:

1. The findings do not support any stronger effect of criminal justice contacts on future (next year) offending and, hence, do not support any consistent (unidirectional) labeling (amplification) or deterrent (preventive) effect by criminal justice contacts on the future level of young people's offending.
2. The findings support an increased likelihood of future police contacts for those who already have a (past) police contact, although the reasons for this are unexplained.
3. The findings are remarkably similar in the studied UK and German cities (Peterborough and Duisburg).

The fact that there is no consistent unidirectional association between a criminal justice contact and future offending does not exclude the possibility that this finding may mask the existence of deterrent *and* labeling effects cancelling each other out (i.e., for some people, criminal justice contacts may promote, and for others, reduce their future offending). What the findings indicate

[57] For more detailed information about how robust the ATTs are, see online supplementary material S4.

though, is that there is no evidence of (or room for) any strong consistent unidirectional impact of deterrence *or* labeling on the participants' future offending. If there are any effects of criminal justice interventions on future offending among our study populations, they must be differential and, if so, may depend on things such as individual differences in how people react to a specific intervention, for example, based on their personality, their experience of previous criminal justice contacts, or the content of the intervention in itself and its social context. Exploring any potential duality of effects (i.e., the existence of labeling, *and* deterrent effects) of criminal justice interventions and, if so, what determines *which effect appears for whom in what context* should be a priority for future studies into the effects of criminal justice interventions.

The fact that for those with a previous criminal justice contact, (self-reported) offending does not show a (statistically significant) increase, but that the risk of a future criminal justice contact does, is highly interesting and possibly consequential for the interpretation of research findings in this area of study. It is similar to the finding of Liberman et al. (2014, p. 363): "we find a considerably larger effect on arrest than on SRO [self-reported offending]." One possible explanation is that those already known to the police are more likely to be apprehended for future crimes (because they are on the police radar). Liberman et al. (2014) call the process that leads to an increased probability of being arrested after having previously been arrested "secondary sanctioning." They speculate that this may be due to "increased scrutiny of the individual's future behavior, by police as well as other actors such as teachers and school staff, as well as from reduced tolerance by police and other actors of an arrestee's future transgressions" (Liberman et al., 2014, p. 363).

Based on the presumption that control interventions are usually initiated by a specific delinquent behavior, one can conclude from our findings that such secondary control effects are *auto-dynamic* effects: The posterior event, the second control intervention, is generated by an essentially same anterior event, the first control intervention. Such an institutional-decision-on-institutional-decision impact is different from a *causal* institutional-decision-on-individual-behavior-effect as stated in labeling theory. The latter one is a causal (and not auto-dynamic) effect because here the posterior event (the individual's delinquent behavior) is generated by an essentially different (i.e., extrinsic)[58] anterior event (the control intervention). Overall, it may appear that such an auto-dynamic effect might best be understood in the light of an assumption of self-reference as suggested

[58] Following Bunge (1959, p. 197), "extrinsic determination" marks the crucial difference between a causal and an auto-dynamic effect.

in systems theory (see Luhmann, 1995): A social control system reproduces itself by referring to its own prior control decisions, filed in the institutional memory of police and court registers.

However, since most juvenile crimes are of a less grave nature, and unlikely to engage investigative resources of the police, an increased detection-by-investigation risk may be a less plausible reason (with the exception of drug-related and traffic crimes, for example, police activity is rarely a main source of detection of crime and identification of offenders). Another possible explanation is that there is some unmeasured qualitative difference in the general seriousness of the crimes committed between those who already have an official contact and other offenders (i.e., those apprehended and processed by the police may generally commit more serious crimes). Our data do not differentiate between the seriousness (harmful consequences) of the crimes of the same kind. For example, some assaults could involve quite minor harms, while others could involve more severe injuries and, therefore, are taken more seriously by victims and bystanders (witnesses) and the police, increasing the risk of the crime being reported and that identified offenders are being formally processed. Crimes that become known to the police are overwhelmingly reported by the general public, as is the identification of possible suspects (it is only for the most serious crimes—where the police may devote some investigative resources to clear the crime—that offenders may be identified through investigative activities by the police).

The fact that the findings are almost identical in the studied UK and German cities, despite some significant differences in the social fabric of the countries and their criminal justice systems, indicate that they may have some generality. They also tally well with other research in Western countries, showing that the general impact of criminal justice interventions on future offending in comparable studies are typically found to be small or non-existent, while its effect on future criminal justice contacts is larger (e.g., Liberman et al., 2014).

When it comes to implications for policy and prevention, we would caution against making firm general policy recommendations as a result of our findings. There are no strong directional and clear-cut findings as to potential labeling or deterrent effects from criminal justice interventions. Our results rather suggest that if there are such effects, they may operate in different directions (i.e., both promote and prevent future offending), potentially being dependent on the people involved, their life-circumstances, stages in a criminal career and the kind of intervention and its execution.

PART II.

CAUSES OF CRIME AND DELINQUENCY

3 | PAPER II

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The influence of differential peer associations on delinquency varies depending on the combination of unstructured socializing and personal morals

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ABSTRACT

The current study explores implications of Situational Action Theory suggesting that the effect of crime-prone peer associates on delinquency is contingent on the combination of unstructured socializing and personal morals. I analyze this three-way interaction with data from a German adolescent sample, using predictions and (average) marginal effects that were calculated from a multilevel Bayesian negative binomial regression. In line with the implications of Situational Action Theory, the results indicate that criminogenic peer influence depends on unstructured socializing and personal morals. Peer effects on criminal behavior were marginal among individuals who held strong personal morals against delinquency and among individuals who spent relatively little time in unstructured socializing. Peer effects were greatest among individuals who held weaker morals against delinquency and spent a relatively large amount of time in unstructured socializing. The results underline the importance of studying the contingencies of criminogenic peer effects on personal and environmental factors.

KEYWORDS

average marginal effects, differential peer associations, juvenile delinquency, peer influence, person-environment-interaction, personal morals, Situational Action Theory, unstructured socializing

3.1 | Introduction

By introducing the concept of differential associations (i.e., the relative exposure to criminal and noncriminal patterns), Differential Association Theory (Sutherland, 1939) and its successor Social Learning Theory (Akers, 1973) triggered a surge of empirical research. This literature highlighted the crucial role of differential associations with peers[59] in explaining delinquency and produced a number of key findings: First, individuals who are associated with (more) delinquent peers commit substantially more crimes than those who have no (or fewer) delinquent peer associations (Hoeben et al., 2016). Second, the differential behavior, attitudes, and reactions of peers are more predictive of criminal involvement than those of parents or other people, and have effects that are at least comparable in size to other well-known predictors of crime (Pratt et al., 2010). Third, individuals who are exposed to deviant peer modeling are more likely to behave in a deviant manner than individuals who are not exposed to such peer modeling in experimental settings (e.g., Gallupe et al., 2016; Mercer et al., 2018; Paternoster et al., 2013). To produce this evidence, the research relied on observational and (quasi-)experimental designs and self-report and network data, and in many cases controlled for an array of potential confounders. This laid a solid foundation for the claim that peer associates have a causal impact on individuals' delinquent behavior (McGloin & Thomas, 2019).

Researchers studying peer influence have nevertheless urged the use of approaches that go beyond the monocausal analysis of peer effects. They have advocated for investigation of more complex questions, such as when (i.e., under what circumstances) peers lead to increased criminal involvement, and what individuals are particularly affected by peer influence (McGloin & Thomas, 2019; see also Agnew, 1991; Miller, 2010). This critique of monocausal approaches was sparked in part by previous interactional research indicating that the effect of peer associations on delinquency is not one-size-fits-all, but that it depends on the circumstances under which an individual spends (peer-oriented) time and on the individual's personal characteristics. One of the circumstances that appears to moderate the impact of differential peer associations on delinquency is that of *unstructured socializing*. Unstructured socializing refers to peer-oriented time spent on activities that are unstructured (i.e., with no particular agenda) and unmonitored (i.e., with no parents or guardians present) (Osgood et al., 1996; Hoeben et al., 2016). Research has found that differential peer associations have substantial effects on delinquency among individuals who spend (large amounts

[59] As in the majority of criminological peer influence research (McGloin & Thomas, 2019), the current study refers specifically to friends when speaking of peers. This contrasts with social scientists, who refer to peers more broadly as "associates of the same age" (Warr, 2002, p. 11).

of) time in unstructured socializing, but no or significantly weaker effects among individuals who spend no (or less) time in unstructured socializing (e.g., Beier, 2018; Bernburg & Thorlindsson, 2001; Sentse et al., 2010; Svensson & Oberwittler, 2010; Wikström et al., 2012; for more mixed results, see Haynie & Osgood, 2005; McNeeley & Hoeben, 2017). A personal characteristic that appears to moderate the impact of differential peer associations on delinquency is that of *personal morals*. Personal morals comprise the attitudes and emotions that indicate how strongly a person has internalized a particular rule of conduct (see Hirtenlehner & Wikström, 2017; Kroneberg et al., 2010; Tyler, 2006). Empirical evidence consistently suggests that crime-prone peer associates have a crime-promoting influence only or especially among individuals with weaker personal morals against delinquency (e.g., Bruinsma et al., 2015; Hannon et al., 2001; Mears et al., 1998; Wikström & Svensson, 2008). Individuals with strong morals are, in contrast, barely affected by criminogenic peer influences.

Having shown that peer effects are contingent on individual characteristics and the circumstances in which the individual spends time, the previous research provides evidence that a monocausal approach to the study of peer influence on delinquency is not sufficient. Furthermore, a handful of recent studies suggest that separate investigations of environmental and personal characteristics also do not go far enough, as peer influence depends on the *combination* of both. Beier (2018) found that peers' alcohol misuse was most predictive of alcohol consumption in unstructured and unsupervised settings among respondents who held weak morals against alcohol consumption. Individuals with strong moral convictions against alcohol consumption, in contrast, were generally unsusceptible to peer influence and generally committed few crimes. Results reported by Wikström et al. (2012), furthermore, suggest that crime-prone peers are especially criminogenic when individuals with a high crime propensity (i.e., weak morals and weak self-control) spend time in risky environments. Individuals with a low crime propensity were again barely affected by crime-prone peers and committed almost no crimes. Apart from these two studies, that are both based on data from the Peterborough Adolescent and Young Adult Developmental Study, no other study to date has investigated how personal morals and unstructured socializing in combination moderate the influence of differential peer associations on delinquency.[60]

To address this research gap, the current study explores whether the effect of differential peer associations on criminal behavior is contingent on the combination of unstructured socializing and

[60] A few other scholars have, however, used unstructured socializing (instead of peer associations) as a focal variable when investigating the three-way interaction (Bernburg & Thorlindsson, 2001; Gerstner & Oberwittler, 2018).

personal morals.[61] It derives implications from Situational Action Theory (SAT; Wikström et al., 2012) to describe how this three-way interaction occurs. Although SAT does not use the term differential peer associations, it acknowledges the criminogenic relevance of a person's exposure to crime-prone (versus crime-averse) peers. In particular, the theory implies that crime-prone peers, unstructured socializing, and weak personal morals against delinquency interact to explain criminal behavior. The study examines SAT's implications using a sample of German adolescents. I chose adolescents as the population of interest as they spend a relatively large amount of time with their peers (e.g., Lam et al., 2014; Larson & Verma, 1999; Warr, 1993) and seem to be more susceptible to peer influences than individuals in other developmental phases (e.g., Berndt, 1979; Chein et al., 2011; Gardner & Steinberg, 2005; Warr, 1993).

3.2 | Peer influence in SAT

This section first introduces SAT's situational model by focusing on its key action mechanism: the perception-choice process. It then discusses how differential peer associations can (in interplay with unstructured socializing and personal morals) account for criminal behavior through this mechanism.

3.2.1 | Situational Action Theory's perception-choice process

Situational Action Theory's situational model assumes that human beings decide on moral action (including delinquent action) through the processes of perception and choice (e.g., Wikström, 2014, 2019a; Wikström et al., 2012; Wikström & Treiber, 2016b).

The *perception* of action alternatives is guided by the so-called moral filter, which consists of the interplay between personal morals and the moral norms that an individual perceives apply in a given setting (i.e., their immediate environment). Personal morals reflect a person's view of whether a particular action is right or wrong under a specific circumstance and how much she or he cares about engaging in the right or wrong behavior.[62] The perceived moral norms embody what kind of action people think or feel is expected of them in a given circumstance. According to the idea of the moral filter, people are likely to perceive crime as a potent action alternative if both

[61] I decided to study personal morals as a moderator instead of the composite crime propensity, as composites are criticized for potentially masking effect variation across its constituent elements (e.g., Pogarsky, 2007; Schulz, 2014).
[62] Composed of personal *moral rules* (a person's rules of conduct) and *moral emotions* (e.g., shame and guilt), a person's morals are essentially "internalized social norms" (Hirtenlehner & Wikström, 2017, p. 498).

personal morals and the perceived moral norms of a setting encourage such behavior. People are unlikely to perceive crime as a potent action alternative if both personal morals and the perceived moral norms of a setting discourage such behavior. People face a moral conflict over a particular crime if their morals encourage the behavior, but the setting's norms discourage it, or vice versa. Under these moral conflict conditions, people typically perceive criminal behavior as a potent action alternative, but they have usually also factored in other options.

Situational Action Theory suggests that individuals make *choices* finally only among those action alternatives that they perceive as potent options. When people see only one action alternative, they automatically (or habitually) "choose" this alternative. However, when people perceive multiple possible action alternatives, they typically *deliberate* before deciding.[63] Only when people deliberate over their choice of action alternatives is it possible for control processes to influence their ultimate behavior (principle of the conditional relevance of controls; Wikström et al., 2012). As an internal control process, *self-control* drives people to act according to their morals despite external pressure to do otherwise. The higher individuals' self-control capabilities, which depend on dispositional (e.g., executive functions) and momentary factors (e.g., stress levels), the more likely they will be to act in line with their own morals instead of giving in to the setting's moral norms. As an external control process, *deterrence* pushes people in circumstances of moral conflict to act according to the perceived moral norms of the setting. The capacity for deterrence depends on environmental cues that a person processes to infer how likely and severe the consequences (i.e., the enforcement) would be if they violated the setting's norms. The more likely and severe the perceived consequences are, the greater the chance that the individual will give in to the norms and act contrary to their own morals.

3.2.2 | The impact of differential peer associations

Peers can affect the processes of perception and choice (and hence delinquent behavior) by contributing to the setting's perceived moral norms and their enforcement, i.e., by contributing to the moral context (Hirtenlehner & Hardie, 2016; Wikström et al., 2012). They can shape this moral context by showing or proposing a particular behavior or by talking about or reacting to behavior in specific ways (Borsari & Carey, 2001; Brechwald & Prinstein, 2011; Dishion & Tipsord, 2011; Hoeben & Thomas, 2019). Within these situational peer processes, crime-prone peers, on the one

[63] For more information on the characteristics of automatic and deliberate choices and the circumstances in which each is relevant, see Wikström and Treiber (2016b).

hand, will frequently provide cues from which others will infer that the moral norms of a given setting support the violation of rules or laws. Crime-averse peers, on the other hand, will frequently signify to others that law-abiding behavior is expected in a given setting. By providing crime-encouraging or crime-discouraging moral norms in a particular setting, peers increase or decrease the likelihood that an individual will perceive crime as a viable action alternative. When individuals perceive multiple action alternatives, peers may also exert pressure to enforce the moral norms for which they had previously shown their support. The strength of this peer pressure can be understood as the perceived severity of adverse peer reactions if the individual were to deviate from the perceived expectations. Generally, the stronger the perceived pressure, the more likely individuals will be to give in to it and act in ways that run contrary to their own morals.[64] Overall, due to peers' differential influence on moral contexts, association with (or exposure to) crime-prone peers will increase the risk of delinquency, whereas the association with crime-averse peers will reduce it.

However, the influence of peers on moral contexts depends, according to SAT, on the peers' actual presence and on how the peer-oriented time is spent (Wikström et al., 2012). Generally, peer influence should be most relevant when the peers are present in the immediate environment. Only then can situational peer processes such as modeling or proposing a particular behavior provide cues that an individual can process to infer the moral norms that apply in a given setting (see Beier, 2018). However, depending on other environmental aspects, the criminogenic impact of crime-prone peers on moral contexts varies even when the peers are present. In particular, SAT assumes that criminogenic peer influences may be strongly diminished in activities that are structured and monitored. Structured activities typically have a specific agenda (e.g., learning to play tennis) that restricts the set of proper action alternatives to a small, well-defined number, usually excluding criminal behavior. Supervised activities include authority figures (e.g., tennis coaches) who generally expect rule abidance and increase the risk of detection and sanctioning for any rule-breaking, thereby strengthening the perception and enforcement of moral norms in line with established rules or laws. When faced with a distinct agenda and supervision that fosters law-abidance, peers are unlikely to influence a moral context in a criminogenic way. However, in unstructured and unmonitored activities, where these obstacles do not exist, peers may behave more freely, i.e., they are free to deviate. In these circumstances with fewer boundaries, crime-prone peers will much more likely provide crime-encouraging cues. Overall, unstructured socializing facilitates the

[64] Whether a person can resist external pressure depends on their ability to exercise self-control. For more information on the interplay between peers and the ability to exercise self-control, see Hirtenlehner et al. (2015).

criminogenic effect of crime-prone peers on the moral contexts that individuals encounter and hence on their delinquent behavior.

As the setting's moral norms can, according to SAT, explain behavior only through the moral filter, peer influence on delinquency should also depend on personal morals. If a person's morals align with the setting's moral norms, the person will generally act in line with the behavior that is encouraged by both elements of the filter. Hence, in circumstances in which a person's morals and the perceived peer expectations (which may manifest themselves as the setting's moral norms) seem to encourage a crime, the person will most likely commit it. In circumstances in which the personal morals and perceived peer expectations, in contrast, discourage criminal behavior (or encourage a particular law-abiding behavior instead), a person will be unlikely to perceive crime as a viable action alternative. Individuals will typically deliberate over what action decision to take when their morals and the perceived peer expectations are in conflict. Only in this latter instance would peer pressure (as an external control process) guide the action decision. Crime-prone peers may pressure a person who is strongly morally opposed to delinquent behavior into committing a crime. In contrast, crime-averse peers may deter a person who morally approves of delinquent behavior from breaking the law.[65] Overall, the weaker an individual's morals against delinquency, the more strongly their criminal behavior will be affected by criminogenic peer influences.

By combining SAT's assumption about the formation of moral contexts and the mechanism of moral filtering, it is apparent that peer effects on delinquency are contingent on the combination of environmental *and* personal factors (see Figure 3.1). Criminogenic peer effects will be greatest when neither characteristics of the immediate environment (structure or supervision) nor personal morals oppose delinquent behavior. Only under these circumstances can peers (1) shape the moral context in such a way that individuals perceive moral norms to clearly encourage crime, which then, (2) in interplay with personal morals that encourage crime, drastically increases the likelihood of criminal behavior. However, when either the characteristics of the setting or the individual's personal morals counteract criminogenic peer influences, the individual's likelihood of perceiving and choosing crime declines rapidly. This may occur either because peers have a more difficult time shaping the moral context when they are in a structured and monitored environment, or because the individual's moral opposition to delinquency leads to a moral filter conflict. In these circumstances, individuals may at best deliberate about committing crimes, but the setting

[65] This latter example is not explicitly discussed by SAT, and it may be that Wikström and colleagues would prefer not to speak of deterrence in this instance, as they discuss peer pressure mainly in relation to internal control processes in which a person exerts self-control to resist criminogenic peer influence (e.g., Wikström, 2014; Wikström & Treiber, 2016b).

characteristics and the individual's morals will facilitate the perception and choice of other law-abiding alternatives. Finally, criminogenic peer effects should be unlikely when individuals with crime-averse morals spend time in structured and supervised activities. In such contexts, it is highly unlikely that (even crime-prone) peers will model or propose delinquent behavior, and the person's morals will clearly indicate that delinquent behavior is not an option. In a nutshell, the weaker an individual's morals and the less structured and supervised the settings in which that individual spends peer-oriented time, the more substantial the impact differential peer associations are likely to have on individual delinquency.

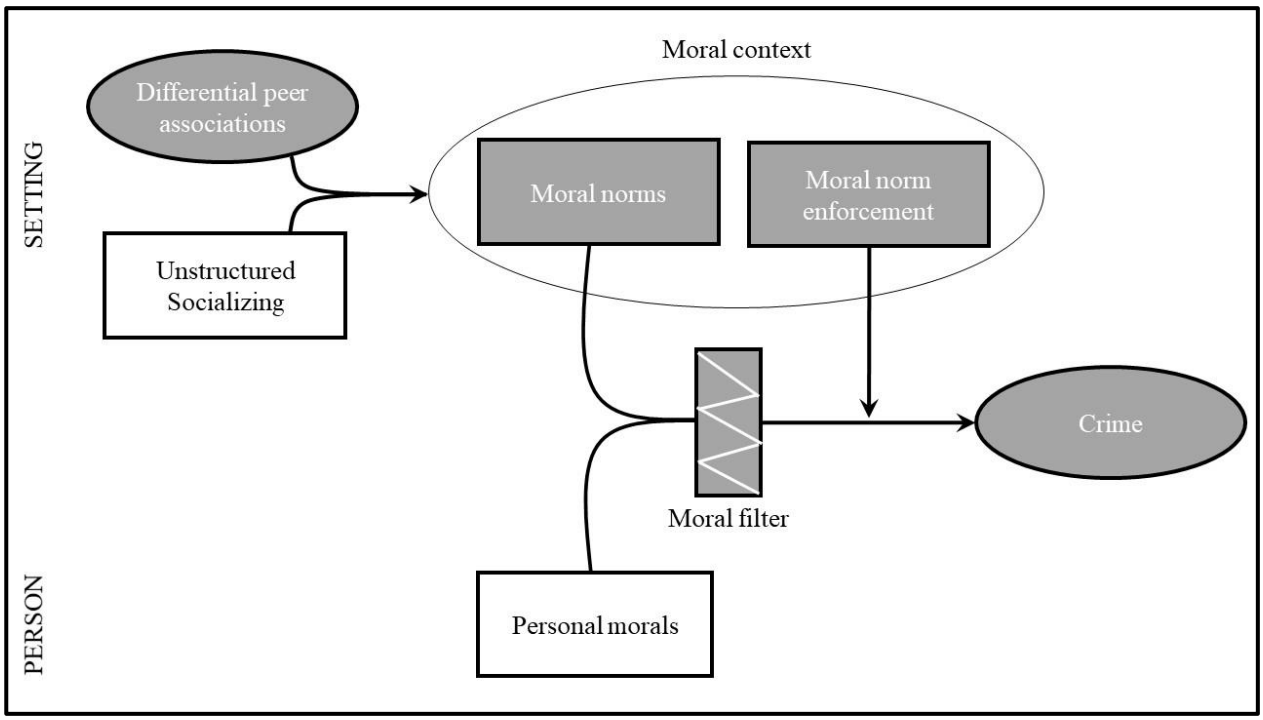

Figure 3.1: Influence of differential peer associations within Situational Action Theory

Notes: The figure is restricted to those concepts that are investigated in the current study. It excludes other concepts (e.g., self-control capabilities) that may also be relevant for explaining peer influences with SAT.

In summary, the current study investigates the following hypotheses, which were derived from SAT:

H1: Differential peer associations (i.e., relative exposure to crime-prone versus crime-averse peers) have, on average, a substantial influence on delinquency.

H2: The less structured and monitored the settings in which the individual spends peer-oriented time, the stronger the influence differential peer associations will have on delinquency.

H3: The weaker an individual's personal morals against delinquency, the stronger the influence differential peer associations will have on delinquency.

H4: *The less structured and monitored the settings in which the individual spends peer-oriented time and the weaker the individual's personal morals against delinquency, the stronger the influence differential peer associations will have on delinquency.*

3.3 | Methods

3.3.1 | Sample

The empirical analyses use data from the panel study *Crime in the modern City* (CrimoC; Boers et al., 2010). CrimoC's objective is to explore the causes and development of deviant and delinquent behavior throughout adolescence and young adulthood. The project includes measures of normative peer influence, personal normative attitudes, and routine activities, all key concepts of the current study. In its first panel wave in spring 2002, CrimoC attempted to sample all seventh-graders in Duisburg, an industrial city in Western Germany. After 40 (of the city's 57) schools were recruited for participation, 3,411 (61 %) of the then on average 13-year-old students completed self-administered questionnaires. In the follow-up waves that were conducted annually, CrimoC attempted to study the same students again. Although the school population shifted somewhat as families moved out of or into the study area, response rates remained satisfactorily high, with about 3,400 participants in subsequent waves.[66]

The current study includes only respondents who participated in at least two of the four panel waves that took place annually between 2003 (wave 2) and 2006 (wave 5). Information from four waves is used to increase the analytical power to detect interaction effects, not to study developments or intraindividual effects. After some data cleaning and the listwise deletion of cases with any missing data on the key measures, 9,654 observations (or interviews) from 3,290 students were available for analysis. Due to the specified conditions and the data cleaning, crime-prone individuals were disproportionately excluded from the investigation. However, differences between included and excluded respondents do not preclude unbiased analysis of statistical relationships, which are generally not suspected to differ systematically across included and excluded respondents (see Gerstner & Oberwittler, 2018; Osgood et al., 1996).

[66] For further information on the CrimoC study and its survey design, see www.crimoc.org.

3.3.2 | Measures

Like most of the previous research, the current study has only ordinary survey measures available that are not spatiotemporally linked but that generalize across time and place. Situational Action Theory, however, specifies its action processes on a situational level and hence can only be tested accurately with situational data (see Wikström et al., 2018). To test SAT's implications using ordinary data, the current study therefore had to make the following auxiliary assumptions: First, the *crimes in the current study actually occurred more likely in those circumstances in which the young people socialized with their peers in unstructured settings*. Second, and relatedly, *in the situations in which people committed their crimes, they were more likely exposed to crime-prone in contrast to crime-averse peers*. Empirical support for the first assumption comes from research showing that people indeed commit deviance much more likely when unstructured socializing than when spending time in a more structured and supervised manner (e.g., Beier 2018; Wikström et al., 2012, 2018). The second assumption is backed up by experimental studies indicating that deviant behavior is more likely when confronted with deviant peer modeling than when unexposed to such models (e.g., Gallupe et al., 2016; Mercer et al., 2018; Paternoster et al., 2013). The third and final auxiliary assumption simply states that *the generalized measures in the current study materialize ("have some bearing") in the real-life situations that individuals encounter* (see Wikström, 2014). I will briefly illustrate this transference from generalized data to situational realities when I present each measure in the following.

Self-reported delinquency. Delinquency, the outcome variable, was measured by asking participants how many times they had committed a variety of crimes since January of the previous year.[67] The frequencies of 16 different offenses (doing graffiti, scratching, other vandalism, theft out of vending machines, shoplifting, bicycle theft, car theft, breaking into cars, bag-snatching, robbery, burglary, other theft, fencing, assault without a weapon, assault with a weapon, and drug-trafficking) were capped at ten and then summed to generate a total delinquency frequency score. The score ranges from 0 to 160.[68] Individuals with higher frequencies are more likely to have been involved in delinquency in real life than individuals with lower crime frequencies.

[67] Because delinquency is measured retrospectively, it refers to a time before the covariates. This design, hence, does not reflect a proper causal time order. Consequently, selection effects may be erroneously interpreted as peer influence effects (McGloin & Thomas, 2019). The current study still uses this "same-wave" design, as peer- and crime-related measures should refer to time points as concurrently as possible, a condition that cannot be fully satisfied by annually collected panel data anyway (Warr, 2002; Wikström et al., 2018). To at least consider the causal time order issue, I also ran the analyses with delinquency information from the subsequent wave, leading to similar, albeit less pronounced and much more uncertain statistical relationships (see online supplementary material).

[68] Sensitivity analyses with uncapped delinquency frequencies or a versatility index produced similar results (see online supplementary material).

Differential peer associations. Differential peer associations were measured by the respondents' assessments of their friends' moral approval or disapproval if the respondent would commit one of the following eight offenses: vandalism, shoplifting, bicycle theft, car theft, extortion, non-residential burglary, assault, and drug-trafficking. The participants assessed how their peers would view these offenses using the response categories (-2) "very bad", (-1) "rather bad", (0) "neither … nor", (1) "rather harmless " (2) "totally harmless". A composite score was created by taking the mean across all eight items. The reliability of the score, reflected by Cronbach's alpha, ranged from 0.91 to 0.92 over the four waves. Individuals with a high differential peer association score were more likely to have been exposed to law-breaking cues when they spent time with their peers in real life. Individuals with a low differential peer association score, on the other hand, were more likely to have been exposed to law-abiding cues when they spent time with their peers.

Although this measure is not the classic peer delinquency measure that researchers usually use to operationalize differential peer associations (Pratt et al., 2010), it is a reasonable choice to assess peer effects within SAT. The theory suggests that peers mainly influence delinquency through their impact on the moral context. As this context is defined by the individual's perception of moral norms and their enforcement, perceived peer expectations regarding behavior should be more relevant for individual delinquency than the peers' objective behavior. However, at best, the study would include perceptions of the peers' law-relevant attitudes, behaviors, and reactions. All three may provide cues that can be processed by the individual to infer the moral norms of a given setting (see Hirtenlehner & Hardie, 2016, who included perceived peer attitudes and behavior).[69]

Unstructured socializing. Unstructured socializing was operationalized by respondents' exposure to risky peer group activities in the following way: First, respondents reported whether they were part of a peer group (or not). Only those who reported being a peer group member were then asked how often they spent time in this group outside of school (response categories: "rarely" / "once or several times a month" / "1 to 3 times a week" / "daily or nearly daily"). Individuals were sorted into a first category if they either had no peer group or spent little time with that group (i.e., "rarely" or "once or several times a month"). The remaining respondents (i.e., those who spent a relatively large amount of time with their peer group outside of school) were asked whether they spent the time with their group (a) just hanging around, (b) going to bars, clubs, and concerts, and (c) drinking alcohol. Response categories ranged from (-2) "disagree" to (2) "totally agree". The three items were combined in a mean score, ranging from -2 to 2. This score was then trichotomized at the

[69] For a more thorough review of measuring peer influences including a discussion about subjective versus objective peer delinquency measures, see McGloin and Thomas (2019; see also Hoeben et al., 2016).

values -0.65 and 0.65 to generate three additional groups or categories.[70] Ultimately, the measurement for unstructured socializing has four categories: (1) spending no or little time with a peer group, (2) spending a relatively large amount of time in low-risk peer group activities, (3) spending a relatively large amount of time in medium-risk peer group activities, and (4) spending a relatively large amount of time in high-risk peer group activities. The likelihood of spending time with one's peers in unstructured and unmonitored settings (i.e., the likelihood of unstructured socializing) in real life should increase from category one to four.[71]

Personal morals. The score for personal morals was constructed with the help of two item batteries. Whereas the first scale reflects a person's moral rules, the latter also captures some moral emotions. The first item battery asked the respondents for their moral approval or disapproval of particular criminal actions. Participants assessed eight offenses (vandalism, shoplifting, bicycle theft, car theft, extortion, non-residential burglary, assault, and drug-trafficking) using the response categories (-2) "totally harmless", (-1) "relatively harmless", (0) "neither … nor", (1), "rather bad", and (2) "very bad". In the second item battery, the juveniles rated their agreement with seven moral reasons for abiding with the law (e.g., "it is worthwhile to have a clear conscience").[72] The response categories varied from (-2) "strongly disagree" to (2) "strongly agree". The two item batteries were combined into a score by computing the mean across all 15 items. Cronbach's alpha of the score is 0.91 in each wave. Individuals with a low score on the personal morals measure should have more frequently held morals that encouraged delinquent behavior in the real-life situations they encountered. Individuals with a high score on the personal morals measure, in contrast, should have held morals more frequently that discouraged delinquent behavior.

3.3.3 | Analytical procedure

Most of the hypotheses in the current study refer to how interactions between multiple variables explain delinquency (see H2-H4). Usually, interaction effects are tested with product terms in an OLS regression (Aiken & West, 1991). However, the traditional OLS procedure is unsuitable for testing the current hypotheses because it does not account for the peculiar nature of crime

[70] These cut-off-points were chosen as they are equally spaced and allow the generation of subgroups that are sufficient in size to have enough power to detect the interaction effects.

[71] Although similar routines were also considered by other researchers to operationalize unstructured socializing (e.g., Bernburg & Thorlindsson, 2001; Gerstner & Oberwittler, 2018; Svensson & Pauwels, 2010), the measure considers only two of the three crucial aspects of the concept, namely the presence of peers and the structuredness of the activities. The lack of information about whether the peer group activities were monitored (or not), makes it a rather conservative test of unstructured socializing (see Haynie & Osgood, 2005). For a more comprehensive discussion of measuring unstructured socializing, see Hoeben et al. (2016).

[72] The other six reasons are: "you just shouldn't do that", "it is important to respect the law", "it is important to follow the rules that others should obey, too", "you are harming others who are innocent", "it is important to be a good example for others (e.g., children)" and "delinquency damages the reputation of one's family."

frequencies (i.e., for the excess of zero and large crime counts). Applied to such data, the procedure produces distorted standard errors and increases the danger of taking ceiling or floor effects for variable-specific interactions (Osgood et al., 2002). Poisson and negative binomial regressions are typically applied in criminology to consider the skewed nature of crime data. However, even these nonlinear models fail to provide a simple solution to study interaction effects (see Hirtenlehner & Hardie, 2016; Oberwittler & Gerstner, 2015; Svensson & Oberwittler, 2010) and produce regression coefficients that are more difficult to understand. This is especially true for coefficients of product terms, which are a combination of two effects: (1) a model-inherent effect that is due to the nonlinear nature of the model, and (2) a "true" interaction effect. Because the two cannot be disentangled easily and can operate in different directions, the regression coefficient of a product term (on its own) gives no reliable information about the true interaction effect in nonlinear models (Bowen, 2012; Mize, 2019).

To overcome these problems, the current study relies on approaches that Mize (2019) synthesized from the methodological literature as best practice to study interaction effects within nonlinear models (for a criminological application that uses similar techniques, see Gerstner & Oberwittler, 2018; Oberwittler & Gerstner, 2015). Mize generally advocates using the estimates from a nonlinear model to calculate predictions, marginal effects, and second differences (i.e., differences between two marginal effects). These quantities are not affected by the same problems as the regression coefficients (of the product terms). They also allow for investigation of interactions on the natural metric of the outcome variable, which makes them much easier for most readers to grasp.

To follow Mize's advice, I first computed a multilevel Bayesian negative binomial regression with an inverse softplus link function using the R package brms (version 2.14.4; Bürkner, 2017). The model includes product terms to analyze the three-way interaction between differential peer associations, unstructured socializing, and personal morals. It furthermore considers the nested (panel) data structure by including a random intercept and controls for temporal effects by including wave dummies (for the full model formula, see Appendix).[73]

After computing this model, I used its estimates to calculate average crime frequency predictions, average marginal effects (AMEs), and second differences (between the AMEs). To facilitate the study of interaction effects, I calculated the predictions and AMEs for different subgroups and

[73] The results of a multilevel negative binomial model with an inverse softplus link are presented, as this model best fitted the data. I, however, also ran multilevel negative binomial models with a log link, multilevel Poisson models with a log and inverse softplus link, and traditional multilevel OLS regressions (with an identity link). Overall, the findings are similar across the procedures, but effects sizes and uncertainties vary (see online supplementary material).

then compared the group-specific AMEs with second differences (see Mize, 2019). Predictions for different subgroups make it possible to investigate whether individuals who differ in their peer associates, peer-group activities, and personal morals are involved to differing degrees in criminal behavior. To predict group-specific average crime frequencies, I applied the *observed-value approach* (Hanmer & Kalkan, 2013). This approach computes predictions for the different subgroups based on the actual observed values that respondents have on each independent variable, instead of relying on particular ideal types (such as a hypothetical person with a specific value on the focal covariate(s) but average values on all other covariates). Group-specific AMEs and second differences make it possible to explore more directly whether and how peer effects differ between various subgroups (varying in their level of unstructured socializing and personal morals). They have the advantage of being intuitive and assembling the interpretation of classical regression coefficients, and thus reflect how the outcome (of some subgroups of individuals) would have changed, *on average*, if the focal independent variable had changed by some specified margin (Mize, 2019). In the current study, they express how delinquency would have changed on average if the peer associations score had increased by a margin of 0.5 (i.e., if the peers had been somewhat more crime-prone). I chose this margin of 0.5 because it seems to be a substantive but still realistic change in peer associations, with a score ranging from -2 to 2. Finally, calculating differences between the AMEs of different subgroups enables a more direct investigation of the interactional hypotheses. These so-called second differences can deliver insights into whether the average peer effects differ between individuals who vary in (the combination of) their unstructured socializing and personal morals.

3.4 | Results

This section presents the findings on each hypothesis sequentially. It first reports the average effect of differential peer associations on delinquency (H1), then examines how peer effects are contingent on each unstructured socializing (H2) and personal morals (H3). Finally, it addresses the current study's central hypothesis that the peer effect depends on the combination of unstructured socializing and personal morals (H4).

3.4.1 | The average peer effect

To investigate hypothesis H1, I computed an AME of differential peer associations for the full sample. This estimate supports the hypothesis, indicating that individuals would have committed

more delinquency on average if they had associated more with crime-prone peers. More precisely, according to the estimate, an increase of 0.5 on the differential peer associations score, i.e., a small rise in crime-prone peer associations, was associated with an increase of 0.45 [CI: 0.38 0.52] criminal offenses on average. The AMEs for the other covariates are also generally in line with SAT's expectations (see Table 3.1): First, the more likely an individual spent time in unstructured socializing, the more the risk of delinquency increased. Second, holding slightly more crime-averse morals was related to a substantially lower number of criminal offenses committed. Finally, the AME estimates for the wave dummies reflect that respondents' delinquency decreased over time.

Table 3.1: Average marginal effects (main effects)

	AME
Differential peer associations	0.45 [0.38 0.52]
Unstructured socializing	
(ref. cat.: No/little time)	
Low-risk	-0.01 [-0.21 0.23]
Medium-risk	0.82 [0.59 1.09]
High-risk	1.48 [1.22 1.77]
Personal morals	-0.67 [-0.75 -0.59]
Panel wave	
(ref. cat.: 2003)	
2004	-0.42 [-0.61 -0.22]
2005	-0.84 [-1.04 -0.66]
2006	-1.13 [-1.33 -0.94]
N (interviews)	9,654

Notes: The numbers in the brackets reflect 95% credible intervals.

3.4.2 | How peer effects vary depending on unstructured socializing

Figure 3.2 allows an initial exploration of hypothesis H2 that unstructured socializing moderates the peer effects. The plot includes the predictions of average crime frequencies (y-axis) depending on differential peer associations (x-axis) for the four unstructured socializing groups (panels). In line with H2, these predictions indicate that the more time an individual spent in unstructured socializing, the stronger the relationship between differential peer associations and delinquency was. For those who spent little time with their peers or spent their peer-oriented time in relatively low-risk activities, delinquency involvement varied relatively little (between zero and about five criminal offenses) depending on having crime-averse versus crime-prone peers. The differences in crime involvement were already stronger among individuals who spent a relatively large amount of time in medium-risk peer group activities and were most pronounced among those who spent peer-oriented time in the riskiest way. The latter group still committed very few crimes when

associated with relatively crime-averse peers, but their predicted average number of crimes rose to about 15 when they reported crime-prone peer associations, suggesting that there may have been a substantial effect of differential peer associations on their delinquency.

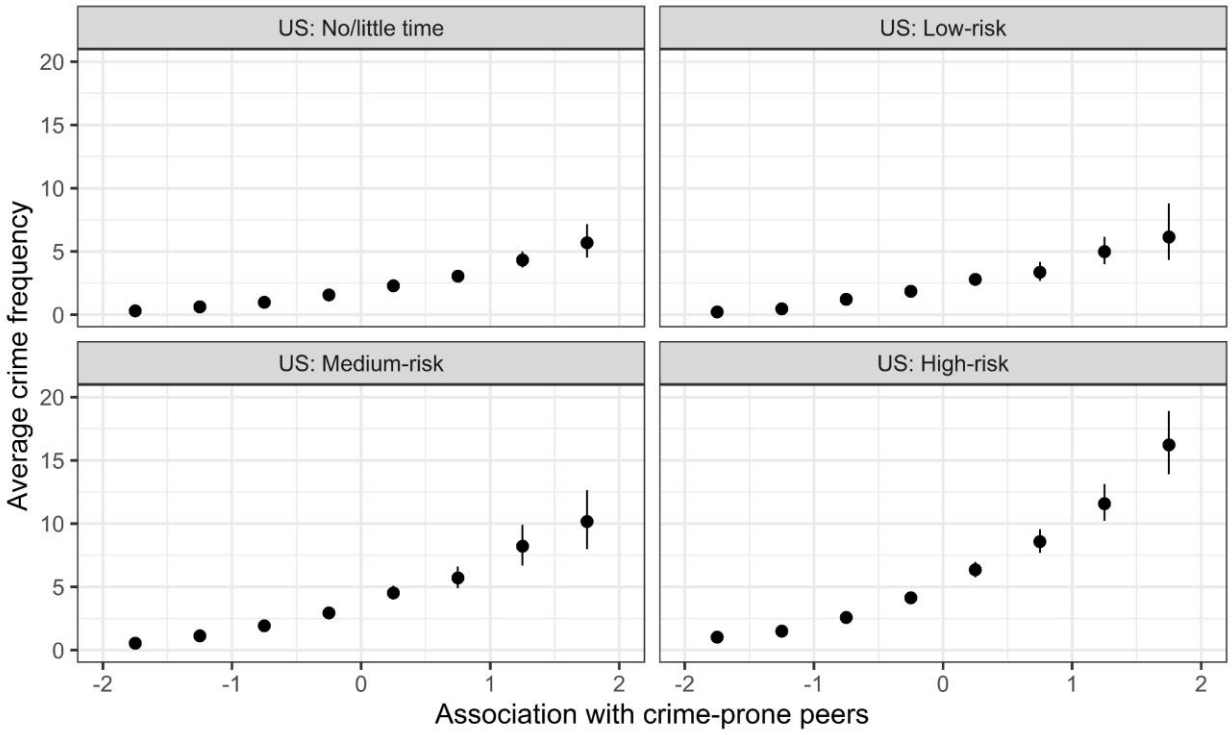

Figure 3.2: Predictions of average crime frequencies (peer associations x unstructured socializing)

Note: The plot shows how the predictions of average crime frequencies vary along the dimension of differential peer associations, contingent on unstructured socializing. The black dots and lines reflect point and 95% credible interval predictions. The focal peer association variable was categorized into eight evenly spaced subsets (-2 to -1.5, -1.5 to -1, …, 1.5 to 2) to predict the average crime frequencies with the observed-value approach. US = unstructured socializing.

In the next step, I investigated the differential impact of peer associates on delinquency more directly with the AME estimates of peer associations for each unstructured socializing group (see Table 3.2). The AMEs support hypothesis H2, indicating that the more likely an individual is to spent time in unstructured socializing, the stronger the peer impact on criminal behavior is. The peer effects increase from spending no or little time with peers to spending a relatively large amount of time with peers in high-risk activities. Peer influence is most pronounced among those individuals who spent a relatively large amount of high-risk time with their peers. If these individuals had associated with somewhat more crime-prone peers, they would have committed 0.89 [0.66 1.14] more crimes on average. The differences between the AMEs are captured by the estimates of the second differences presented in the right half of Table 3.2. These second differences underscore that the peer effects increase from the group spending no or little time with peers to the high-

risk subgroup, allowing for the conclusion that individuals who spent the most time in unstructured socializing are affected more substantially by criminogenic peer influences.

Table 3.2: Average marginal effects (of peer associations, contingent on unstructured socializing)

Unstructured socializing	N	AME	Second differences		
			No/little time	Low-risk	Medium-risk
No/little time	4,065	0.21 [0.14 0.28]			
Low-risk	1,833	0.36 [0.26 0.47]	0.15 [0.03 0.27]		
Medium-risk	1,936	0.60 [0.43 0.78]	0.39 [0.20 0.57]	0.24 [0.05 0.44]	
High-risk	1,820	0.89 [0.66 1.14]	0.68 [0.44 0.94]	0.53 [0.28 0.80]	0.29 [0.00 0.59]

Notes: The second differences reflect the difference between the AMEs of the subgroups in the first column of the table and the AMEs of the particular subset in the last three columns. The numbers in the brackets reflect 95% credible intervals.

3.4.3 | How peer effects vary depending on personal morals

To investigate how personal morals moderate criminogenic peer effects (H3), I first divided the sample into three groups based on individuals' personal morals score: a weak (scores between -2 and 0), a medium (0 to 1), and a strong (1 to 2) personal morals group.[74] This creation of ideal types or groups eases the presentation and interpretation of interaction effects that include a continuous moderator (see Gerstner & Oberwittler, 2018; Mize, 2019) and ensures that the presentation of the moderation results for personal morals resemble those for unstructured socializing.

In Figure 3.3, I show the predictions of the average crime frequencies (y-axis) depending on differential peer associations (x-axis) for each of the three personal morals groups (panels). These average predictions indicate, in line with hypothesis H3, that the weaker a person's morals, the more substantial the effect of differential peer associations on delinquency was. The differences in delinquent involvement between individuals with crime-prone and crime-averse peers were small among individuals with strong personal morals. Even those with crime-prone peers had typically committed relatively few (only about 2.5) crimes. For individuals with medium morals, crime-prone peer associates were related to substantially more criminal involvement. However, this relationship between delinquency and differential peer associations was even more apparent among those with weak personal morals. Individuals in this group with crime-averse peers committed relatively few crimes on average (about 2.5), whereas those with crime-prone peers reported more than ten crimes on average.

[74] Gelman and Park (2009) recommended discretizing variables into three instead of two categories to reduce efficiency loss when communicating scientific results. The personal morals score is trichotomized in an unequally spaced way to consider the skewed nature of the variable (see Kroneberg & Schulz, 2018).

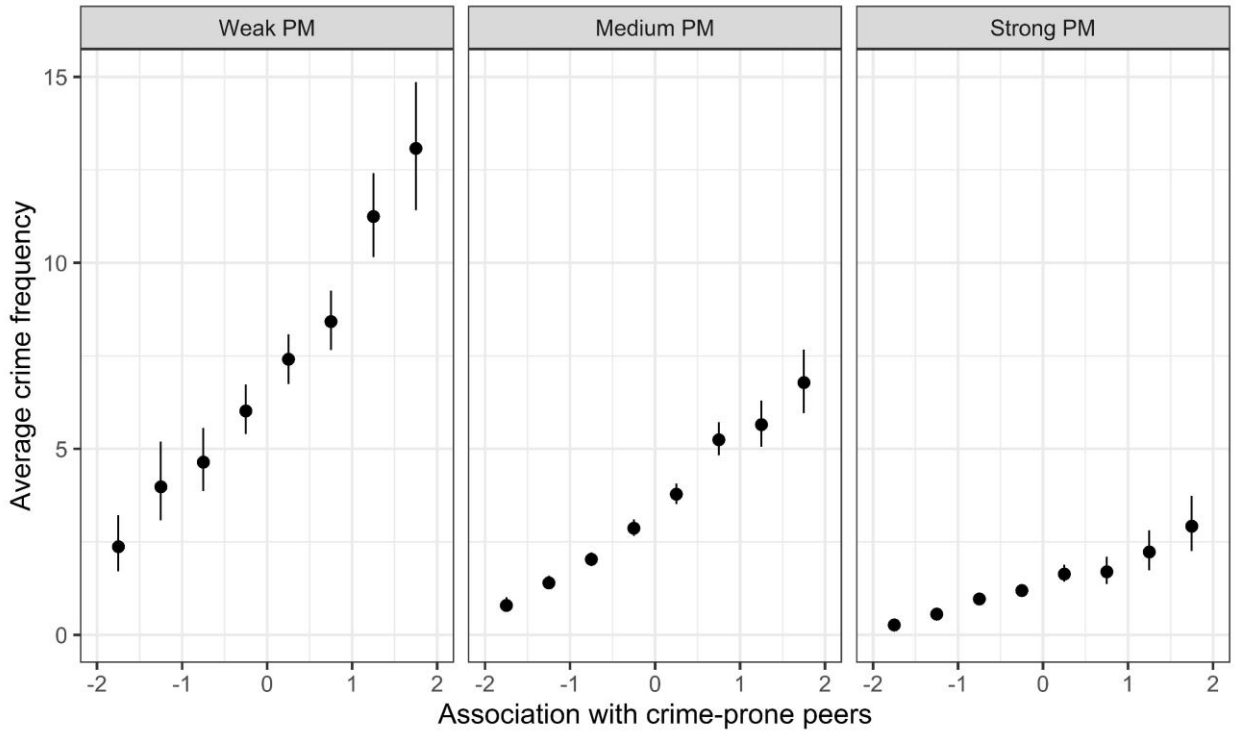

Figure 3.3: Predictions of average crime frequencies (peer associations x personal morals)

Notes: The plot shows how the predictions of average crime frequencies vary along the dimension of differential peer associations, contingent on personal morals. The black dots and lines reflect point and 95% credible interval predictions. The focal peer association variable was categorized into eight evenly spaced subsets (-2 to -1.5, -1.5 to -1, …, 1.5 to 2) to predict the average crime frequencies with the observed-value approach. PM = Personal morals.

The AMEs again corroborate the findings from the prediction plot and hence lend support for hypothesis H3 (see Table 3.3). The peer effects increased the weaker a person's morals against delinquency. Whereas individuals with strong morals against delinquency would have committed only 0.20 [0.16 0.25] more crimes on average if they had slightly more crime-prone peer associates, the estimated rise in crime was 1.15 [0.87 1.43] for those with weak morals. The second differences underscore that the differences in the peer effects across the personal morals groups were substantial and that individuals with weaker morals were clearly most affected by peer influence (see the right half of Table 3.3).

Table 3.3: Average marginal effects (of peer associations, contingent on personal morals)

			Second differences	
Personal morals	**N**	**AME**	**Medium**	**Strong**
Weak	973	1.15 [0.87 1.43]	0.54 [0.33 0.75]	0.94 [0.66 1.23]
Medium	3,529	0.61 [0.51 0.71]		0.40 [0.32 0.49]
Strong	5,152	0.20 [0.16 0.25]		

Notes: The second differences reflect the difference between the AMEs of the subgroups in the first column of the table and the AMEs of the particular subset in the last two columns. The numbers in the brackets reflect 95% credible intervals.

3.4.4 | How peer effects vary depending on unstructured socializing and personal morals

This section explores the fourth and central hypothesis of the current study, H4, which states that peer effects vary conditional on the combination of unstructured socializing and personal morals. It again relies on the three personal morals categories. Combining these three categories with the four unstructured socializing categories divides the sample into twelve different subgroups (3 x 4 = 12). For these twelve subgroups, the relationship between differential peer associations and delinquency is explored again using average crime predictions, AMEs, and second differences.

Figure 3.4 includes the predictions of average crime frequencies (y-axis) depending on differential peer associations (x-axis) for the twelve subgroups (panel; for a scatter plot that shows the same relationship, see Appendix, Figure 3.5). In line with H4, the predictions support the idea that peer associations may have a differential impact on delinquency depending on the combination of unstructured socializing and personal morals. Among those who held strong morals against delinquency, the average number of delinquent acts is small (with the predictions barely exceeding five), irrespective of their peer associations and unstructured socializing (see panels at the bottom of Figure 3.4). This suggests that the relationship between peer associates and their delinquency is marginal at best. Crime involvement differs more substantially across the peer association dimension among individuals with medium morals. Generally, these individuals committed only a few crimes when they had crime-averse peer associates (irrespective of their unstructured socializing). However, when they had crime-prone peers, their crime involvement differed depending on how much time they spent in unstructured socializing. Individuals who spent little time with their peers committed fewer than five offenses on average. Similar individuals who spent a relatively large amount of time in high-risk peer activities, in contrast, reported about ten crimes. However, the differences in delinquency across the peer associations dimension are most pronounced for individuals with weak morals, and this is especially true for those who spent a relatively large amount of time in risky socializing with their peers. The latter individuals committed, on average, about five crimes when they were associated with crime-averse peers. Predictions suggest that they committed about 20 offenses on average when they had crime-prone peer associates, suggesting the peer associates may have had a particularly substantial impact on their delinquency.

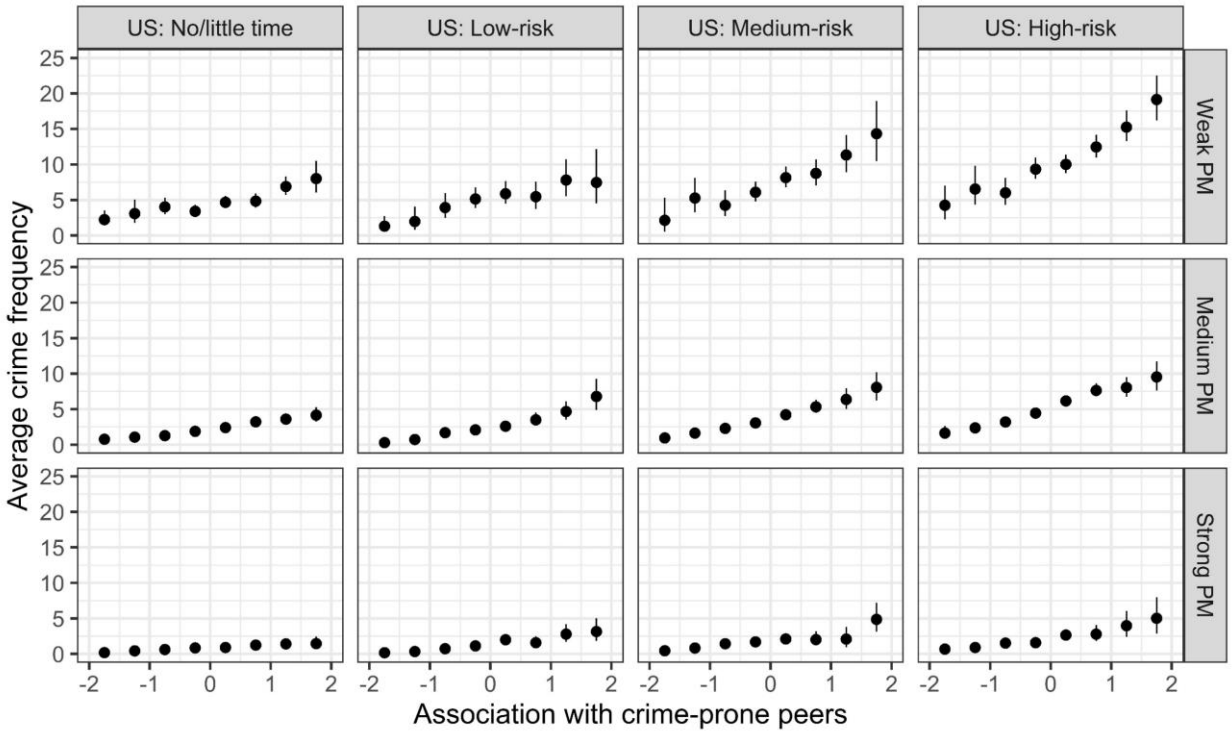

Figure 3.4: Predictions of average crime frequencies (peer associations x unstructured socializing x personal morals)

Notes: The plot shows how the predictions of average crime frequencies vary along the dimension of differential peer associations, contingent on the combination of unstructured socializing and personal morals. The black dots and lines reflect point and 95% credible interval predictions. The focal peer association variable was categorized into eight evenly spaced subsets (-2 to -1.5, -1.5 to -1, …, 1.5 to 2) to predict the average crime frequencies with the observed-value approach. US = unstructured socializing. PM = Personal morals

The AME estimates underline the findings derived from the prediction plot and support hypothesis H4 (see Table 3.4). They point out that peer effects on delinquency are probably rather marginal for most individuals. According to the estimates, most people would increase their delinquent involvement by only 0.5 or fewer offenses if they associated with somewhat more crime-prone peers. The exception to this rule are individuals with somewhat weaker morals who spent a relatively large amount of time in medium-risk or high-risk peer group activities. In the case of individuals with medium morals who spent their time in such more risky activities, an increase of 0.5 on the differential peer associations score would have probably resulted in them committing about (nearly) one more crime (high-risk subgroup: 0.99 [0.73 1.27]). This rise in criminal behavior may have even been higher for individuals with weak morals, committing about 1.5 more offenses on average if exposed to slightly more crime-prone peers (medium-risk subgroup: 1.48 [0.87 2.14]; high-risk subgroup: 1.66 [1.10 2.26]).[75] The second difference estimates provide evidence that the

[75] For the medium-risk group, a false credible interval is reported in the version published in *Kriminologie - Das Online-Journal | Criminology - The Online Journal*.

peer effects among these latter individuals are indeed substantially higher than the peer effects in most other subgroups (see Appendix, Table 3.6). They support the study's central hypothesis H4: Differential peer associations seem to have a particularly criminogenic impact on delinquency among individuals who spend a relatively large amount of time in unstructured socializing *and* hold weak(er) personal morals against delinquency.

Table 3.4: Average marginal effects (of peer associations, contingent on unstructured socializing and personal morals)

Unstructured socializing	Personal morals	N	AME
No/little time	Weak	336	0.59 [0.28 0.94]
Low-risk	Weak	127	0.78 [0.30 1.39]
Medium-risk	Weak	176	1.48 [0.87 2.14]
High-risk	Weak	334	1.66 [1.10 2.26]
No/little time	Medium	1,327	0.30 [0.20 0.40]
Low-risk	Medium	521	0.52 [0.37 0.70]
Medium-risk	Medium	852	0.76 [0.54 1.00]
High-risk	Medium	829	0.99 [0.73 1.27]
No/little time	Strong	2,402	0.11 [0.07 0.15]
Low-risk	Strong	1,185	0.24 [0.17 0.33]
Medium-risk	Strong	908	0.28 [0.16 0.41]
High-risk	Strong	657	0.38 [0.23 0.53]

Notes: The numbers in the brackets reflect 95% credible intervals.

3.5 | Conclusion

Driven by previous research that showed that peer influence differs depending on personal and environmental characteristics, the current study explored to what extent peer effects are contingent on a person's morals and on spending the peer-related time in an unstructured way. To do this, it derived some implications from Situational Action Theory's situational model. Providing strong support for SAT, the current study produced the following findings: First, associating with crime-prone (versus crime-averse) peers substantially increased delinquent behavior on average. This finding is in line with SAT's assumption that exposure to crime-prone (versus crime-averse) peers increases the risk of delinquency, as those peers more likely provide cues that shape the moral context in a criminogenic way. Besides supporting H1, this finding adds to an abundance of previous research highlighting the importance of (crime-prone) peers for criminal involvement (for reviews, see Hoeben et al., 2016; McGloin & Thomas, 2019; Pratt et al., 2010). Second, peer effects were particularly strong among individuals who spent a relatively large amount of time in unstructured peer group activities. This finding supports H2 and is consistent with SAT's presumption that structured and monitored settings counteract criminogenic peer influence by

impeding the perception and choice of criminal action alternatives. It furthermore supplements previous research that found a similar two-way interaction between differential peer associations and unstructured socializing (e.g., Beier, 2018; Bernburg & Thorlindsson, 2001; Svensson & Oberwittler, 2010; Wikström et al., 2012). Third, the weaker a person's morals, the more substantial was the influence of differential peer associations on delinquency. This finding supports SAT's moral filter assumption, which indicates that the influence of environmental factors (such as peers) on the perception of action alternatives is contingent on a person's morals. In line with the findings, the filter suggests that the criminal involvement will be highest when both the setting's moral norms (e.g., provided by peers) and the personal morals are affirmative of crime. Providing some evidence for H3, the result aligns with several previous studies (e.g., Bruinsma et al., 2015; Hannon et al., 2001; Mears et al., 1998; Wikström & Svensson, 2008).

However, the current study's main finding is that associating with crime-prone peers drives crime conditional on the *combination* of exposure to unstructured socializing and personal morals. The results show marginal peer effects among individuals who held strong morals against delinquency and among individuals who spent little to no time in peer groups or spent time in their peer group in relatively non-risky ways. In support of hypothesis H4, the peer effects were, in contrast, much more substantial among individuals with weaker morals who spent a relatively large amount of time in risky socializing with their peers. This finding supports SAT's highly interactional implication that peer effects will be strongest if neither a person's morals nor a setting's features (particularly its structuredness and monitoredness) impede the peer influence. It furthermore supplements the previous research based on data from the Peterborough Adolescent and Young Adult Developmental Study that reported similar evidence of a three-way interaction between peer associations, unstructured socializing, and personal morals (Beier, 2018) or crime propensity (Wikström et al., 2012).

The results can be considered good news for policymakers in the field of criminal law and for their objective of preventing crime. The current study shows that there may be various ways to reduce juvenile delinquency (see Wikström & Treiber, 2016a). Policies can be designed not only with the aim of reducing a juvenile's association with delinquent peers but also with that of restricting activities with these peers to more structured and supervised settings. Furthermore, if changing a person's environmental exposure to particular peers and activities is not possible, measures can be designed to influence personal characteristics such as morals (or self-control capabilities). The study shows that delinquency can be reduced substantially if only one of these causal factors is successfully modified. There are two reasons for this. On the one hand, people are clearly at the

most risk of delinquency if each factor is formed in a crime-encouraging way. If only one factor inhibits the risk of delinquent involvement, this is enough to diminish criminal participation substantially. On the other hand, delinquency can be strongly reduced by the modification of only one factor because positive changes in one cause of crime may influence other causes of crime over time. Processes such as selection and socialization may potentially cause positive cascading effects (Cairns & Cairns, 1994; Kandel, 1978). Suppose an intervention successfully reduces a person's exposure to crime-prone peers: This should result in the person being exposed to criminogenic learning contexts less frequently. In turn, as a result of being less exposed to such crime-facilitating environments, the person will be less likely to internalize crime-encouraging norms over time. Thus, the person's morals will be modified in such a way that she or he is less likely to see crime as a viable action alternative. Overall, reduced exposure to crime-prone peers may, in the long run, strengthen a person's morals through the respective socialization processes (see Wikström & Treiber, 2016a).

Although the results of this study are promising, they are limited in several crucial ways that call for future replication. First, the analyses relied on generalized measures and not on situational data, preventing a direct test of SAT. Future studies should supplement the relatively scarce previous research that used more appropriate designs such as the space-time budget, laboratory experiments, or vignettes to test how criminogenic peer effects are moderated on a situational level (e.g., Beier, 2018; Wikström et al., 2012). Second, the results of the current study are based on data from the 2000s. Adolescents' routine activities and social networks have changed since then, above all due to the rise of social media (McGloin & Thomas, 2019; Warr, 2002). Against this backdrop, future research should explore whether more recent data produce similar results. Third, although SAT considers a person's self-control capabilities as a vital force for withstanding external (e.g., peer) pressure (see Hirtenlehner et al., 2015), the current study does not consider self-control as an additional moderator of peer influence. To not further complicate the already complex analyses, it instead focuses on a person's morals, as SAT deems this personal factor as somewhat more fundamental to delinquency than self-control capabilities (Wikström & Svensson, 2010). Future research should investigate the theory's implications that peer processes are contingent on unstructured socializing, personal morals, *and* self-control abilities.[76]

[76] In sensitivity analyses, I included an indicator of self-control abilities as an additional covariate to check how robust the results are to its inclusion (exclusion). The findings from this model (that also included all two-way interaction terms with the other key covariates) do not differ substantially from the presented ones (see online supplementary material).

Despite these limitations, the study results are valuable as they supplement other evidence implying that it is not enough to study a monocausal influence of peers. Peers seem to have a criminogenic impact only (or particularly) among some individuals, and only when these individuals are exposed to their peers in specific circumstances. This knowledge calls on future studies to consider these interactional aspects. The insight that interactional analyses are crucial is, however, far from new. About thirty years ago, Agnew (1991) urged the study of interactions when researching criminogenic peer influences. Sparked in part by the rise of SAT, some recent research has followed his advice to tackle the pervasive lack of interactional analyses (e.g., Beier, 2018; Hirtenlehner et al., 2015; Hirtenlehner & Hardie, 2016; Svensson & Oberwittler, 2010). Despite this increase in peer studies that specify non-additive effects, more remains to be done to satisfy those who still deem moderation analyses a critical research gap in the peer influence literature (e.g., Hoeben, 2016; McGloin & Thomas, 2019).

3.6 | Appendix

Model formula:

$$
\begin{aligned}
\text{Delinquency}_i &\sim \text{NegBin}(\mu_i, \phi) && \text{[Likelihood]}\\
\text{invsoftplus}(\mu_i) &= \alpha_{ID[i]} + && \text{[Varying intercept]}\\
&\quad \beta_1\,\text{DiffPeers}_i + && \text{[Main effects]}\\
&\quad \beta_2\,\text{US\_Low-Risk}_i + \beta_3\,\text{US\_Medium-Risk}_i + \beta_4\,\text{US\_High-Risk}_i +\\
&\quad \beta_5\,\text{PMorals}_i +\\
&\quad \beta_6\,\text{Year\_2004}_i + \beta_7\,\text{Year\_2005}_i + \beta_8\,\text{Year\_2006}_i +\\
&\quad \beta_9\,\text{DiffPeers}_i * \text{US\_Low-Risk}_i + && \text{[Two-way interactions]}\\
&\quad \beta_{10}\,\text{DiffPeers}_i * \text{US\_Medium-Risk}_i + \beta_{11}\,\text{DiffPeers}_i * \text{US\_High-Risk}_i +\\
&\quad \beta_{12}\,\text{DiffPeers}_i * \text{PMorals}_i +\\
&\quad \beta_{13}\,\text{US\_Low-Risk}_i * \text{PMorals}_i + \beta_{14}\,\text{US\_Medium-Risk}_i * \text{PMorals}_i +\\
&\quad \beta_{15}\,\text{US\_High-Risk}_i * \text{PMorals}_i +\\
&\quad \beta_{16}\,\text{DiffPeers}_i * \text{US\_Low-Risk}_i * \text{PMorals}_i + && \text{[Three-way interactions]}\\
&\quad \beta_{17}\,\text{DiffPeers}_i * \text{US\_Medium-Risk}_i * \text{PMorals}_i +\\
&\quad \beta_{18}\,\text{DiffPeers}_i * \text{US\_High-Risk}_i * \text{PMorals}_i\\
\phi &\sim \text{Exponential}(1) && \text{[prior for shape parameter phi]}\\
\alpha_{ID} &\sim \text{Normal}(\bar{\alpha}, \sigma) && \text{[adaptive prior]}\\
\bar{\alpha} &\sim \text{Normal}(0, 10) && \text{[prior for average person]}\\
\sigma &\sim \text{Normal}(0, 10) && \text{[prior for standard deviation of persons]}\\
\beta_k &\sim \text{Normal}(0, 5) && \text{[prior for all k = 1 … 18 beta coefficients]}
\end{aligned}
$$

Notes: i = individual observation (ranges from i = 1 … 9,654); ID = ID of each person (ranges from i = 1 … 3,290); DiffPeers = Differential peer associations; US_Low-Risk = Unstructured socializing: Low-risk; US_Medium-Risk = Unstructured socializing: Medium-risk; US_High-Risk = Unstructured socializing: High-risk; PMorals = Personal morals; Year_2004 = Panel wave 2004; Year_2005 = Panel wave 2005; Year_2006 = Panel wave 2006.

Table 3.5: Descriptive statistics

Variable	Mean	SD	Min	P05	P25	P50	P75	P95	Max
Delinquency frequency	2.32	6.95	0	0	0	0	1	14	120
Differential peer associations	-0.68	0.99	-2.00	-2.00	-1.50	-0.88	0.00	1.25	2.00
Unstructured socializing									
No/little time	0.42								
Low-risk	0.19								
Medium-risk	0.20								
High-risk	0.19								
Personal morals	0.93	0.74	-2.00	-0.43	0.50	1.00	1.47	1.93	2.00
Panel wave									
2003	0.22								
2004	0.26								
2005	0.27								
2006	0.25								

Notes: n = 9,654.

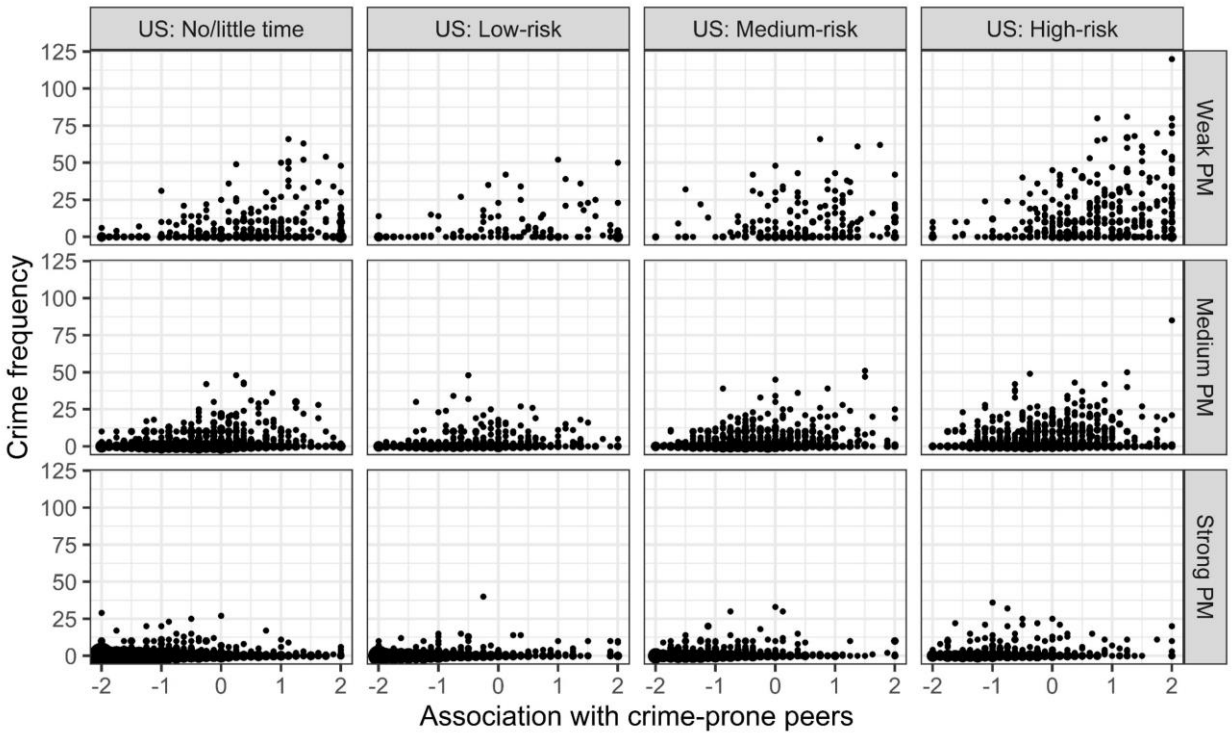

Figure 3.5: Scatter plots (peer associations x unstructured socializing x personal morals)

Notes: The scatter plots show how the crime frequencies vary along the dimension of differential peer associations, contingent on the combination of unstructured socializing and personal morals. The size of the black dots is proportional to the number of observations with precisely the same combination of values. US = unstructured socializing. PM = Personal morals.

Table 3.6: Second (AME) differences (of peer associations, contingent on unstructured socializing and personal morals)

Group 1		Group 2		Second Differences
Unstructured socializing	**Personal morals**	**Unstructured socializing**	**Personal morals**	**(AME_{Group1} - AME_{Group2})**
High-risk	Weak	Medium-risk	Weak	0.17 [-0.65 1.03]
High-risk	Weak	High-risk	Medium	0.67 [0.26 1.08]
High-risk	Weak	Low-risk	Weak	0.88 [0.09 1.62]
High-risk	Weak	Medium-risk	Medium	0.90 [0.29 1.53]
High-risk	Weak	No/little time	Weak	1.06 [0.42 1.75]
High-risk	Weak	Low-risk	Medium	1.14 [0.55 1.75]
High-risk	Weak	High-risk	Strong	1.29 [0.70 1.89]
High-risk	Weak	No/little time	Medium	1.36 [0.79 1.96]
High-risk	Weak	Medium-risk	Strong	1.39 [0.80 1.99]
High-risk	Weak	Low-risk	Strong	1.42 [0.86 2.02]
High-risk	Weak	No/little time	Strong	1.55 [0.99 2.15]
Medium-risk	Weak	High-risk	Medium	0.48 [-0.18 1.21]
Medium-risk	Weak	Low-risk	Weak	0.69 [-0.14 1.50]
Medium-risk	Weak	Medium-risk	Medium	0.72 [0.26 1.23]
Medium-risk	Weak	No/little time	Weak	0.89 [0.19 1.63]
Medium-risk	Weak	Low-risk	Medium	0.95 [0.33 1.64]
Medium-risk	Weak	High-risk	Strong	1.10 [0.47 1.78]
Medium-risk	Weak	No/little time	Medium	1.17 [0.55 1.84]
Medium-risk	Weak	Medium-risk	Strong	1.19 [0.56 1.90]
Medium-risk	Weak	Low-risk	Strong	1.23 [0.62 1.89]
Medium-risk	Weak	No/little time	Strong	1.36 [0.76 2.02]
High-risk	Medium	Low-risk	Weak	0.21 [-0.43 0.78]
High-risk	Medium	Medium-risk	Medium	0.23 [-0.12 0.58]
High-risk	Medium	No/little time	Weak	0.40 [-0.02 0.83]
High-risk	Medium	Low-risk	Medium	0.47 [0.16 0.79]
High-risk	Medium	High-risk	Strong	0.62 [0.42 0.83]
High-risk	Medium	No/little time	Medium	0.69 [0.42 0.98]
High-risk	Medium	Medium-risk	Strong	0.71 [0.43 1.01]
High-risk	Medium	Low-risk	Strong	0.75 [0.48 1.04]
High-risk	Medium	No/little time	Strong	0.88 [0.62 1.16]
Low-risk	Weak	Medium-risk	Medium	0.02 [-0.50 0.66]
Low-risk	Weak	No/little time	Weak	0.20 [-0.38 0.84]
Low-risk	Weak	Low-risk	Medium	0.25 [-0.12 0.74]
Low-risk	Weak	High-risk	Strong	0.40 [-0.10 1.03]
Low-risk	Weak	No/little time	Medium	0.48 [0.00 1.08]
Low-risk	Weak	Medium-risk	Strong	0.50 [0.01 1.12]
Low-risk	Weak	Low-risk	Strong	0.54 [0.04 1.17]
Low-risk	Weak	No/little time	Strong	0.66 [0.19 1.28]
Medium-risk	Medium	No/little time	Weak	0.17 [-0.23 0.55]
Medium-risk	Medium	Low-risk	Medium	0.23 [-0.04 0.51]
Medium-risk	Medium	High-risk	Strong	0.38 [0.11 0.64]
Medium-risk	Medium	No/little time	Medium	0.45 [0.22 0.70]
Medium-risk	Medium	Medium-risk	Strong	0.48 [0.29 0.69]
Medium-risk	Medium	Low-risk	Strong	0.52 [0.28 0.76]
Medium-risk	Medium	No/little time	Strong	0.65 [0.43 0.88]

Table 3.6: (Continued)

Group 1		Group 2		Second Differences
Unstructured socializing	**Personal morals**	**Unstructured socializing**	**Personal morals**	**(AME_{Group1} - AME_{Group2})**
No/little time	Weak	Low-risk	Medium	0.07 [-0.28 0.44]
No/little time	Weak	High-risk	Strong	0.21 [-0.14 0.59]
No/little time	Weak	No/little time	Medium	0.29 [0.04 0.56]
No/little time	Weak	Medium-risk	Strong	0.31 [-0.03 0.68]
No/little time	Weak	Low-risk	Strong	0.35 [0.02 0.70]
No/little time	Weak	No/little time	Strong	0.48 [0.16 0.83]
Low-risk	Medium	High-risk	Strong	0.15 [-0.09 0.37]
Low-risk	Medium	No/little time	Medium	0.22 [0.05 0.41]
Low-risk	Medium	Medium-risk	Strong	0.24 [0.04 0.45]
Low-risk	Medium	Low-risk	Strong	0.28 [0.15 0.44]
Low-risk	Medium	No/little time	Strong	0.41 [0.25 0.59]
High-risk	Strong	No/little time	Medium	0.07 [-0.10 0.26]
High-risk	Strong	Medium-risk	Strong	0.10 [-0.09 0.29]
High-risk	Strong	Low-risk	Strong	0.14 [-0.03 0.31]
High-risk	Strong	No/little time	Strong	0.27 [0.12 0.43]
No/little time	Medium	Medium-risk	Strong	0.02 [-0.13 0.18]
No/little time	Medium	Low-risk	Strong	0.06 [-0.06 0.19]
No/little time	Medium	No/little time	Strong	0.19 [0.11 0.28]
Medium-risk	Strong	Low-risk	Strong	0.04 [-0.11 0.19]
Medium-risk	Strong	No/little time	Strong	0.17 [0.04 0.30]
Low-risk	Strong	No/little time	Strong	0.13 [0.05 0.22]

Notes: The groups are sorted by the size of their respective AME (from large to small; see Table 3.4). The numbers in the brackets reflect 95% credible intervals.

PART III.

FORMAL CONTROL AND CAUSES OF CRIME

4 | PAPER III

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Does contact with the justice system influence Situational Action Theory's causes of crime? A study of English and German juveniles

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ABSTRACT

To explore why system contact often has no crime-preventative effect, the current study examined the effects of juvenile justice contact on Situational Action Theory's causes of crime, including personal morals, deviant peer associations, and detection risk perceptions. The analysis is based on a sample of English (Peterborough Adolescent and Young Adult Developmental Study) and German (Crime in the modern City study) juveniles. Propensity score matching was applied to estimate whether the lenient system contacts influenced the causes of crime in the year after the contact. The treatment effect estimates are mostly insignificant and relatively small. The few significant estimates in the English sample suggest that official contact slightly increased deviant peer associations and decreased feelings of moral guilt. Overall, the findings suggest that system contact may often have no crime-preventative effect as it does not (Germany), or only slightly (England) affect Situational Action Theory's causes of crime. Previous studies, primarily based on U.S. data, often reported more substantial effects that mostly operated in a crime-amplifying direction. It is speculated whether the less substantial impact in the current study can be attributed to the overall more lenient, diversion-oriented handling of the examined English and German offenders.

KEYWORDS

effects of juvenile justice system contact, causes of crime, Situational Action Theory, propensity score matching, cross-national research

4.1 | Introduction

A primary objective of modern criminal justice systems is to prevent crime. If the systems fail to achieve this goal and a person breaks the law, legal actors (e.g., police officers) typically seek to prevent further offending by arresting and, if necessary, sanctioning that person. In recent decades, numerous studies have examined whether such contact with the criminal justice system (including, e.g., apprehension, arrest, diversion, and sanctions) actually prevents reoffending. Contrary to the goals of criminal justice agents, the majority of these studies suggest that system contact is either relatively ineffective or even increases criminal involvement (for reviews, see Barrick, 2014; Bernburg, 2019; Huizinga & Henry, 2008; Kleck & Sever, 2017).

Using data that resulted in insignificant findings in a previous investigation (Boers et al., 2022), the current study addresses the following question: Why do contacts with the criminal justice system often have no crime-preventative impact? Answering this question theoretically and empirically requires focusing on the intervening factors that may mediate the effects of official contact on reoffending.

Criminologists have identified numerous factors as theoretically relevant mediators. *Perceptual deterrence* theorists assume that official contact may reduce reoffending by increasing sanction threat perceptions (Paternoster, 2018). *Labeling* scholars suggest that official contact may amplify reoffending (1) by initiating or facilitating the development of a deviant self-concept, (2) by initiating or increasing the association with deviant peers, or (3) by inhibiting the punished offenders' social bonds and life chances (Bernburg, 2019). The *procedural justice* theory stresses that the effect of system contact depends on whether apprehended offenders feel that the proceeding against them was fair and just and whether they consequently view the law and its enforcement as legitimate (e.g., Slocum et al., 2016). *Defiance* theory emphasizes that the impact of punishment depends on how offenders perceive the sanction (e.g., unfair and stigmatizing), how strongly they are bonded to the sanctioning agent and community, and whether they subsequently develop feelings of shame or self-righteous anger (Sherman, 1993).

Confronted with such an extensive (but not exhaustive) list of potentially relevant intervening factors, one wonders which are the most relevant mediators. So far, most empirical studies did not concentrate on this question. They instead tested the presumptions of a single theory, such as deterrence theory (e.g., Anwar & Loughran, 2011; Pogarsky et al., 2005) or labeling theory (e.g., Bernburg et al., 2006; Wiley et al., 2013). However, the empirical literature indicates that mechanisms proposed in different theories may be at work simultaneously. Hence, it may be better to

integrate these mechanisms into a more general theoretical framework. The need for such integration was recently highlighted by Piquero et al. (2011, p. 338), who stressed that "it may be more profitable to think of a general theory of sanctions rather than deterrence, labeling, or defiance theory." When considering the requirements for such a general theory of sanctions, it is apparent that it must clearly define the factors that directly cause crime. It is only when a sanction reduces these key causal inputs that it can ultimately reduce reoffending. By distinguishing between direct causes of crime and more distal factors, a general theory should be able to "separate the wheat from the chaff."

All theories that clearly define the causes of crime are thus candidates for a starting framework for a general theory of sanctions. The current study selects Wikström's *Situational Action Theory* (SAT) as a promising starting point (Wikström et al., 2012). SAT is chosen because it has performed well empirically so far (Pauwels et al., 2018)[77], provides a sophisticated action model, and goes to great lengths to distinguish between direct antecedents of criminal offending and more distal factors. In particular, the theory differentiates the *causes of crime* from the *causes of the causes* (Wikström, 2011). *Causes of crime* are the few factors that directly influence criminal involvement. *Causes of the causes* are, in contrast, factors that have only an indirect effect on criminal offending through their influence on the causes of crime. By providing this terminology, SAT allows identifying the causes of crime as the crucial mediators of system contact effects. Some of these causes of crime coincide with intermediate factors outlined in contemporary versions of deterrence and labeling theory. By using SAT as starting framework, the current study can thus integrate some ideas of the latter theories about sanctioning effects. In doing so, it is—at least to my knowledge—the first empirical application of SAT to study the impact of criminal justice interventions.[78]

The current study aims to shed some more light on why a relatively large number of previous studies—among them one that used the data at hand—found no crime-preventative effects of system contact. It does so by empirically examining to what extent a juvenile justice contact influences the antecedents of crime as postulated in SAT. The analyses are based on data from two criminological panel studies conducted in England and Germany. So far, nearly all studies that

[77] Unfortunately, no systematic review of the SAT literature which was published after 2015 exists. My own (probably biased) assessment of the more recent literature is that it produces evidence largely in favor of SAT and that strong research designs produce supportive evidence even more likely (e.g., Wikström et al., 2018).

[78] Recent SAT publications focus on developmental aspects (e.g., Wikström, 2020) or crime prevention in general (Wikström & Treiber, 2016a). Accordingly, it can be expected that the theory will be applied more frequently to criminal justice issues in the future.

examined official contact effects on antecedents of crime were conducted with U.S. samples. While experts already consider this study base to be too small (e.g., Bernburg, 2019; Farrington & Murray, 2014; Huizinga & Henry, 2008; Paternoster & Iovanni, 1989), research outside the U.S. can be described as extremely sparse (for exceptions, see Murray et al., 2014; Schulte, 2019; Zhang & Messner, 1994)[79]. This lack of research is unfortunate because system contact in different juvenile justice systems (with their various policies, sanctions, and police behavior) may produce different effects (see Huizinga et al., 2003). The German and English systems offer an insightful research context. They are much more lenient than the U.S. system but still characterized by some differences, like in police behavior. Hence, it is worth exploring whether results obtained with U.S. samples can be generalized to the English and German contexts.

4.2 | Intervening factors in the framework of SAT

This section shows how SAT can provide a starting framework for a general theory of sanctions. For this purpose, it first introduces how contemporary versions of labeling and deterrence theory are applied to study sanctioning effects. By highlighting some theoretical weaknesses of these applications, the section then shows how SAT attempts to overcome these problems and may integrate some ideas of the classic theories.

Rooted in symbolic interactionism, labeling theory assumes that humans are creatures that continuously construct meaning of the environment and themselves by interpreting significant symbols in social interactions. As a result of the meaning people attach to official contact, it may trigger three criminogenic processes according to contemporary versions of labeling theory (Bernburg, 2019; Krohn & Lopes, 2015): (1) the development of a deviant self-concept, (2) the reduction of conventional social bonds and life chances, and (3) the involvement in deviant peer groups. These processes are also studied and typically supported in recent empirical applications of labeling theory (e.g., Bernburg et al., 2006; Krohn et al., 2014; Wiley et al., 2013). However, from my point of view, this labeling research is theoretically underspecified. It typically only defines a set of "orienting" criminogenic processes triggered by societal reaction to deviance. It lacks a uniting explanation in the form of an action model which specifies how the changes (e.g., in self-concept) brought about by these processes ultimately operate (together) to explain an increase in deviant or criminal behavior.

[79] Schulte (2019) used the German data of the current study to examine how the severity of system contact affected some intermediate factors.

Modern deterrence theory, in contrast, borrows its action model specification from rational choice theories. Consequently, deterrence research assumes that humans are self-interested and rational beings that actively choose one action alternative over others as a result of cost-benefit considerations (Paternoster, 2010, 2018). Recent sanctioning research typically tests perceptual versions of deterrence theory (e.g., Lochner, 2007; Matsueda et al., 2006; Pogarsky et al., 2005). These versions assume that an intervention can affect future criminal behavior only through its impact on the perceptions of punishment certainty, severity or celerity. Due to its recourse to rational choice frameworks, deterrence theory allows—other than labeling theory—for precise implications about how formal control ultimately leads to changes in delinquency via changes in sanction threat perceptions. However, standard rational choice models have been immensely criticized over the last decades. Critics primarily noted that these approaches view humans as overly rational and calculative beings. Consequently, they cannot explain automatic-spontaneous, habitual or emotionally driven behavior, which may make up a large part of human behavior (e.g., Kroneberg et al., 2010; Wikström & Treiber, 2016b).[80]

Partly as a response to these problems of existing criminological theories, Per-Olof Wikström introduced SAT at the beginning of the 2000s (Wikström et al., 2012; Wikström & Treiber, 2016b). One of his primary goals was to specify a proper action model which explains why an act of crime is (not) committed. To do this, SAT's action model attempts to consider modern insights from the behavioral sciences, among them the finding that human (including deviant) behavior is often habitual. Taking the perspective that humans are rule-guided (instead of primarily self-interested) beings, SAT's action model has fared well in empirical tests so far (Pauwels et al., 2018). As it also acknowledges the importance of some of the concepts included in deterrence theory (e.g., perceptions of consequences) and labeling theory (e.g., deviant peers), it seems a promising framework to integrate some of the mechanisms proposed for decades in these classic contenders.

Before discussing this integration in more detail, however, I will first introduce the basic ideas of SAT and thereby extract the causes of crime as the article's outcome variables of interest (for a more detailed introduction to SAT, see Wikström et al., 2012). In its action model, SAT describes how particular characteristics of a person interact with inducements of the setting (i.e., the immediate environment) to create motivations (temptations and provocations) and initiate and guide a perception-choice process that finally produces criminal behavior.

[80] Some contemporary versions of rational choice theory try to consider why humans often act in a more habitual or automatic manner (e.g., Kroneberg et al., 2010). These theories, however, have so far not been applied to sanctioning research.

The main causal factors in the perception process are an *individual's personal morals* and the *setting's moral norms*. While personal morals are a person's "value-based and emotionally grounded views about what is the right or wrong thing to do or not to do in particular circumstances," a setting's moral norms are the "perceived shared rules of conduct and their degree of homogeneity" (Wikström, 2020, p. 193). The combination of the two factors determines the action alternatives an individual perceives (Wikström & Treiber, 2016b). Ultimately, SAT predicts that individuals are more likely to perceive (and choose) crime as an action alternative in circumstances in which their own morals and the perceived moral norms of the setting are more affirmative of crime.

After perceiving particular actions as potent alternatives, the choice of action can typically proceed in two different modes (Wikström & Treiber, 2016b): choices can be made in a habitual or in a more deliberative manner. Habitual decisions are typically made in familiar circumstances in which the personal morals and setting's moral norms are congruent (e.g., both are crime-affirmative) because individuals then normally perceive only one viable course of action. Choices are, in contrast, typically deliberative if a person acts in unfamiliar circumstances, if there is a conflict between a person's morals and the setting's moral norms, and if opportunities for reflection are not undermined by, for example, time pressure or strong emotions. In this circumstance, individuals have time to deliberate over multiple action alternatives. According to SAT, it is only in this more deliberative mode of decision-making that additional factors—beyond personal morals and the setting's moral norms—have a causal influence on criminal behavior.

SAT suggests that these additional causal factors, namely the *ability to exercise self-control* and the *perceived risk of consequences*, exert their causal impact within two so-called control processes (Wikström & Treiber, 2016b). As an internal control process, self-control refers to acting according to one's own morals despite external incentives or pressure to do otherwise. The *ability to exercise self-control* depends on momentary (e.g., intoxicated) and dispositional (e.g., executive functions) factors. SAT generally assumes that the higher the self-control ability of people, the more likely they will resist external criminogenic influences (e.g., provided by deviant peers) and act law-abiding. Situational Action Theory's external control process is called *deterrence*. Deterrence describes "the avoidance of breaking a moral rule (committing an act of crime) because of the fear[81] of consequences" (Wikström, 2008, p. 347). The likelihood that individuals are deterred

[81] Although deterrence research (like SAT) acknowledges that the deterrence process is fear-based (see Paternoster, 2010), studies (including the current one) typically analyze risk perceptions. Future research should embrace deterrence as emotion-based process by analyzing how the fear of consequences affects criminal activity and how this fear is shaped by official contact.

increases with their perceptions of how likely and severe consequences are when committing a crime. These perceived consequences depend on deterrent cues provided by the setting (e.g., by authority figures) and on how sensitive a person is to these cues, that is, how likely the person processes them so that they perceive them as punishment threats (Wikström, 2008). Generally, SAT assumes that the higher the *perceived threats of punishment*, the more likely a person will abide by the law despite their urge to commit a crime (Wikström et al., 2011).

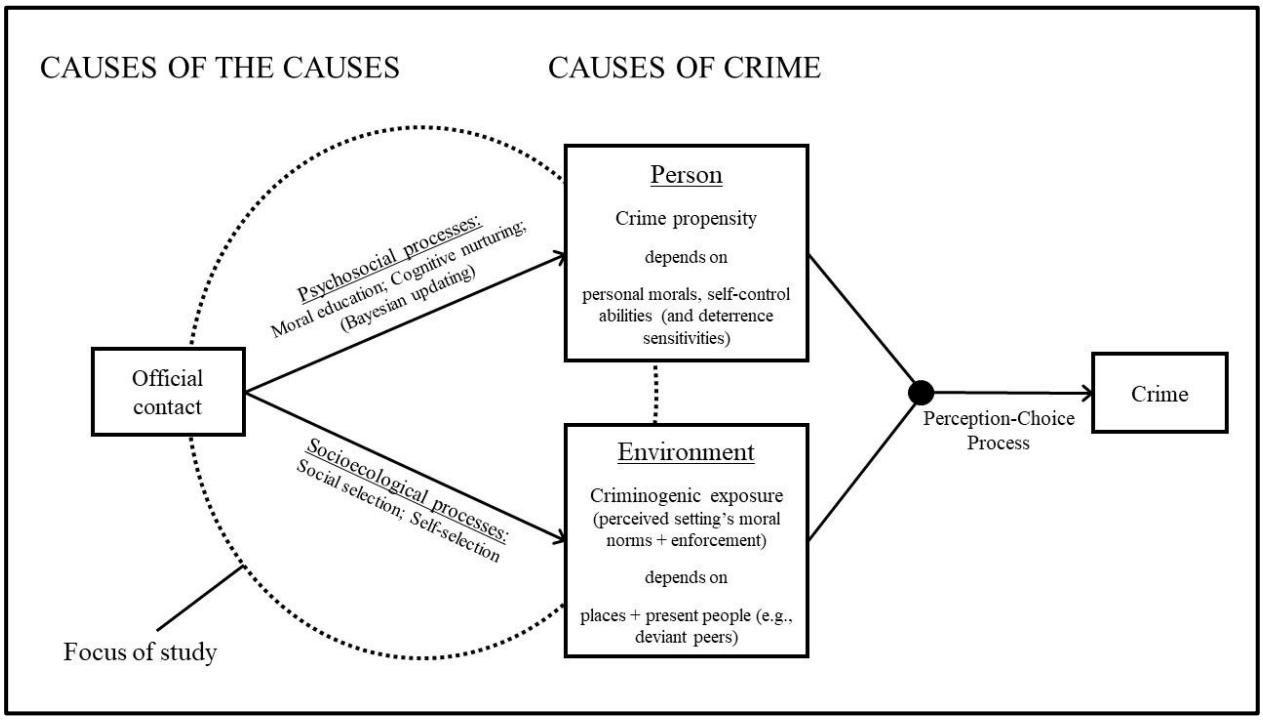

Figure 4.1: Situational Action Theory and the impact of official contact (see also Wikström, 2020)

Overall, SAT's action model thus suggests that the direct causes of crime operate in a person-environment interaction to produce moral (including criminal) action (see Figure 4.1). The causes of crime that form a person's *crime propensity* are their personal morals and self-control capabilities. Deterrence sensitivities (or sanction threat perceptions) can be regarded as an additional personal cause of crime (see Wikström et al., 2011). SAT furthermore suggests that particular places (e.g., the city center) and the exposure to deviant peers are major environmental causes of crime (e.g., Wikström et al., 2012). These two factors contribute to the criminogeneity of the immediate environments a person takes part in and thus to their *criminogenic exposure* (i.e., the criminogeneity of the setting's moral norms and enforcement). Especially deviant peers have been found to be a critical facilitator of criminogenic exposure by providing (and enforcing) crime-affirmative moral norms (see Beier, 2018; Kaiser, 2021; Wikström et al., 2012).

How can a criminal justice contact now influence delinquency? According to SAT, formal control cannot directly impact criminal involvement but can change it only indirectly via changing the

causes of crime. As a cause of the causes, official contact must trigger the following mechanisms specified in SAT's recently refined Developmental Ecological Action model (DEA model; Wikström, 2020): (1) psychosocial processes and (2) socioecological processes. The two key psychosocial processes that influence a person's crime propensity are moral education and cognitive nurturing. While moral education is a learning and evaluation process responsible for changes in an individual's personal morals, cognitive nurturing describes an experiential process that influences the ability to exercise self-control. The socioecological processes of self-selection and social selection, on the other hand, are responsible for changes in a person's criminogenic exposure. While self-selection refers to a person's choices to participate in particular settings (including particular people), social selection refers to how cultural and structural conditions in a jurisdiction enable or restrict the access of particular people (and thus also their self-selection) to particular settings.

However, since SAT has so far not been applied to study the impact of criminal justice interventions, the developmental processes are somewhat general or abstract in this regard. The DEA model, furthermore, does not explicitly specify a mechanism affecting deterrence sensitivities (or sanction threat perceptions), probably because these typically play only a minor role in SAT introductions. Integrating ideas of classic sanctioning theories may help to overcome some of these problems, as these have specified detailed mechanisms triggered by formal control over decades (including processes influencing sanction threat perceptions). They, thus, seem a valuable source to enrich the processes defined in the DEA model. Accordingly, the following subsections will consider ideas of the DEA model and the classic theories when discussing how official contact may affect some of SAT's key causes of crime. In particular, the presented processes will be related to personal morals, deviant peer associations, and detection risk perceptions since reliable, comparable data are available only for these factors (which are among the mainly tested causes of crime in previous SAT research; Pauwels et al., 2018).

4.2.1 | Official contact and personal morals

Since the classic sanctioning theories have not specified any processes relevant to personal morals, the current study will rely on the DEA model to infer implications regarding how official contact may change this cause of crime. The DEA model suggests that people typically acquire their morals gradually and change them through a continuous process of moral education (see Wikström, 2020; Wikström & Treiber, 2016a, 2018). Moral education, as one example of social learning, has three sub-mechanisms: (1) instruction, (2) observation, and (3) trial and error. *Instruction* takes

place if moral instructors (e.g., parents or police officers) actively "communicate information about the rules of conduct which apply to different contexts" (Wikström & Treiber, 2016a, p. 80). *Observation* means that a person keeps track of others' actions and the action-induced consequences (e.g., rewards and punishments). The *trial and error* process refers to a person's own experimentation with actions and their consequences. Deterrence experiences, i.e., "experiences of threats of punishment, and punishments" (Wikström, 2008, p. 356), are one form of experience that influences an individual's morals. As one particular type of deterrence experience, official interventions can be described as *instructive* reactions of legal actors to the "*trial-and-error*-experimentation" of an offender.

SAT does not assume that official contact with the criminal justice system increases or decreases personal morals on average. It instead implies that the effect depends on the specific ways others react to a criminal offense and subsequent official contact, and on how the offender evaluates her or his crime and sanctioning experience as well as the formal and informal reactions of others. In particular, the moral education process depends on such factors as (1) the consistency of reactions and instructions, (2) the offender's internal evaluation of these instructions or reactions, (3) the offender's prior experiences and existing personal morals, and (4) the offender's psychosocial machinery (e.g., cognitive capabilities; Wikström & Treiber, 2016a, 2018).

Formal reactions may influence a person's morals only substantially when they initiate changes in their day-to-day moral experiences in interaction with significant others (e.g., parents or peers). That is because daily activities and interactions with significant informal others play a continuous role in an individual's moral development, while rare encounters with the police or prosecutors typically do not (Wikström, 2008). Before youths have official contact with the criminal justice system, they are likely to have already had several crime-related experiences, including (moral and deterrence) experiences with the reactions of, for example, peers or parents. As a result, these youths' morals are likely to be relatively consolidated and, as a consequence, less malleable by new experiences. Their moral learning contexts have probably already been formed and solidified by previous reactions to criminal activity and are thus unlikely to change substantially due to new reactions. If the moral learning context is relatively fixed, the moral content learned within it should remain relatively stable because a person's morals only develop and change in interaction with the environment (Wikström, 2008). However, if the social context changes due to a system contact (e.g., a person gets more involved with deviant peers), personal morals may also change (e.g., may get more crime-affirmative) through the altered continuous moral experiences within these modified contexts.

The impact of official contact on personal morals has been largely neglected as an object of empirical investigation (Wikström, 2008). Only recently have Wiley and colleagues devoted part of their work to this topic using data from the *Gang Resistance Education and Training program* (Slocum et al., 2016; Wiley, 2015; Wiley et al., 2013, 2017; Wiley & Esbensen, 2016). Matching juveniles who had police contact to juveniles who had no contact but were otherwise similar (e.g., in their pre-contact morals), they found that those with contact subsequently reported lower personal morals (measured by the increased adherence to street codes and less anticipated guilt for deviant behavior) than their matched counterparts. The effects were generally stronger among individuals who had been arrested than among those who had only been stopped by the police (Wiley et al., 2013; Wiley & Esbensen, 2016). Besides Wiley and colleagues' investigations, other—less methodologically sophisticated—analyses found similar results (Ageton & Elliott, 1974; Kaplan & Johnson, 1991; Schulte, 2019).

4.2.2 | Official contact and deviant peer associations (criminogenic exposure)

To discuss potential changes in deviant peer associations, I rely on ideas established over several decades by labeling theorists, who have identified mechanisms that may lead from official contact to an individual's exclusion from conventional networks and inclusion in deviant peer groups. These mechanisms can be seen as potential manifestations of the more general self-selection (and social selection) processes specified in the DEA model (see Figure 4.1).

Labeling theorists outline the following processes that lead to the association with deviant peers (Bernburg, 2019; Paternoster & Iovanni, 1989): In a first step, prosocial others (e.g., family, conventional peers, employers) may—due to their knowledge of the crime or system contact—vilify the offender and turn away from her or him. Second, the offender may actively withdraw from conventional others to avoid anticipated adverse reactions to the crime and the system contact. Both processes lead to exclusion from conventional society and socially isolate the individual. In a third step, the individual who came into contact with the criminal justice system may become involved in a deviant peer group, often because of her or his actual or perceived isolation. Labeling theory assumes that the apprehended offender may befriend individuals who share the same offending and sanctioning experiences because she or he hopes for acceptance or even admiration among these new friends.

Labeling theory, however, also acknowledges that official contact may have no impact or may even weaken deviant peer associations (Bernburg, 2019; Paternoster & Iovanni, 1989). Negative

reactions of parents or conventional friends to a crime may, for example, warn the offender that future offending could alienate them (further). When the offender is ashamed in front of significant others (e.g., parents or partners) or fears losing them due to further delinquency, she or he may decide to stop spending time with deviant friends to prevent them from providing opportunities for and exerting pressure to engage in further rule-breaking. As a consequence of official contact, an offender may also be monitored more closely by law-abiding others, who may actively (try to) restrict the offender's exposure to criminogenic settings or persons (e.g., deviant peer groups). Furthermore, labeling theory assumes that, while a first system contact strongly influences sanctioned individuals, official contact effects diminish the more a person experienced labeling and stigmatization before. Thus, if previous informal and formal deterrence experiences have already led to an association with deviant peers, further system contacts are likely to have significantly less or no further influence on those associations.

Despite the importance of deviant peer groups as one of the strongest predictors of criminal involvement, only a few longitudinal studies have investigated the influence of official contact on the subsequent association with deviant peers, peer groups, or gangs. Most of the results indicate an increased involvement with deviant friends or gangs among juveniles who previously had (more frequent or severe) contact with the juvenile justice system compared with juveniles who had less or no system contact (Bernburg et al., 2006; Johnson et al., 2004; Kaplan & Johnson, 1991; Krohn et al., 2014; Restivo & Lanier, 2012; Schulte, 2019; Wiley et al., 2013; Wiley & Esbensen, 2016). A related line of research, furthermore, found some evidence that official contact also had the potential to isolate offenders from conventional friends (e.g., Wiley et al., 2013; Zhang, 1994; Zhang & Messner, 1994)—which, according to labeling theory, is a precondition for the association with deviant peers.

4.2.3 | Official contact and detection risk perceptions

Probably because SAT introductions typically grant deterrence sensitivities (or perceptions of punishment threats) no prominent spot, the DEA model does not present a process that may lead to changes in these sensitivities. Deterrence theorists, in contrast, have put great effort into explaining how individuals modify their risk perceptions (or risk assessments, which can be seen as indicator of one's deterrence sensitivities; Wikström, 2008) after contact with the criminal justice system. The current study, thus, relies on their ideas when reviewing the so-called *Bayesian updating model* (Anwar & Loughran, 2011; Kreager & Matsueda, 2014). This model states that people will

gradually adjust their prior (i.e., initial or existing) perceptions (according to probability rules) when exposed to new evidence or information related to the perception at hand. Individuals should, for example, decrease their risk perceptions when they or others commit a crime and get away with it, but should increase them when they or others are apprehended or sanctioned for committing a crime.

The Bayesian updating model, furthermore, assumes that the extent of these perception adjustments depends on the prior (criminal and sanctioning) experiences of the individual. The more prior information individuals possess relevant to their perception, the less weight the new experience or information has in modifying the perception. Suppose an action (crime) is novel for individuals. In that case, they must rely mainly on the actual experience to infer future consequences (e.g., apprehension and sanction) of similar actions since there is no or only little former experience on which to base their inferences. Consequently, this recent experience has a relatively large impact on the modification of the individuals' perceptions. Among individuals with more (criminal and sanctioning) experience, in contrast, the recent incident has less weight in adjusting a relevant perception because this perception is much more strongly informed by prior experiences.

There is growing longitudinal research on how an individual's own or significant others' apprehension or punishment experiences change her or his perceptions of detection or arrest risk (e.g., Anwar & Loughran, 2011; Lochner, 2007; Matsueda et al., 2006). Results indicate that people are generally "capable Bayesians" (Paternoster, 2010, p. 809). If they were (frequently) caught when committing a crime, they modified their detection risk perceptions on average upwards. If they were not (or infrequently) caught, they lowered their perceptions. The reported effects were generally small to modest, and some other studies found more mixed, inconclusive, or even conflicting evidence (e.g., Pogarsky et al., 2005; Schulte, 2019; Schulz, 2014).

Additional investigations suggest that the extent of updating depends on the criminal history of the individual (Anwar & Loughran, 2011). For offenders with a larger ratio of current crimes (i.e., crimes committed in the period of interest) to past crimes (i.e., crimes committed in the time before the period of interest), risk perception modifications after apprehension were larger than for those with a smaller current-to-past-crimes ratio. Experienced offenders seem to need a stronger signal (i.e., a high arrest-crime ratio for many crimes) within a specific period to update their risk perceptions as strongly as novice offenders.

4.3 | The English and German juvenile justice systems

The theoretical ideas above indicate that more information is needed on the nature and certainty of system contacts to predict their impact on the juvenile population under investigation.

Generally, the legal framework of the English and German juvenile justice systems differed in several important ways in the early 2000s (i.e., in the current study period; Boers et al., 2022). The more punitive English system was characterized, for example, by an earlier age of criminal responsibility, fewer diversion possibilities, and longer durations of custodial sanctions. These differences in punitivity are also reflected in nation- and county-wide statistics on the reactions of the juvenile justice systems. German juvenile offenders who had contact with the justice system were much more likely to be diverted than English offenders. English offenders, in contrast, were much more likely to be convicted to long-term custodial sentences than their German counterparts.

However, these differences in punitivity were much smaller in the samples that were analyzed in the present study. Most of the 15-year-old respondents experienced quite similar and relatively lenient treatment during the period of interest (see Table 4.2 in section *Measures*). Most cases were diverted out of the formal court system for informal handling, and the remaining juveniles were almost exclusively given educational measures (mainly unpaid community service).

Because the legal reactions were so similar in both samples, the main differences in the treatment experience of respondents with official contact were probably due to the divergent police practices in the two countries. English police acted more intrusively than their German counterparts when apprehending an offender, even when the individual was apprehended for a relatively minor offense (which made up most offenses in the current study). Officers usually arrested the juvenile and took her or him to the police station, where the offender's primary caregiver was called to pick her or him up. German police officers, in contrast, mainly recorded the offender's personal data in such a minor case and released the individual. In case of a standard diversion, the German prosecutor usually simply sent a formal letter informing the offender and principal caregivers of the decision, whereas in England, the decision (reprimand/warning) was delivered at the police station by a police officer in uniform after the offender's admission of guilt (Dünkel & Heinz, 2017; Home Office & Youth Justice Board, 2002).

English police officers acted not only more intrusively but were also encouraged to act more proactively than their German colleagues between 2002 and 2007. This proactive policing style was due to a short-lived policy change brought about through the *Offenses Brought to Justice Target* (OBJT). The OBJT required the justice system to increase the number of offenders who received a

formal reaction by 20% within five years (Office for Criminal Justice Reform, 2004). To meet this target, the police acted more proactively than before and actively searched for crimes, which they found especially among juveniles who tended to be involved in more easily detectable delinquency (e.g., Bateman, 2017; Flanagan, 2007). This policy change led to the criminalization of many first-time, low-level juvenile offenders, whose cases previously mainly were dealt with informally (e.g., through a stern talking-to). Accordingly, the number of recorded first-time offenders rose by 53% in Cambridgeshire, and the arrest rate increased by 48.2% between 2002 and 2006 (Ministry of Justice, 2010). German officers, in contrast, generally responded only to those (youth) crimes that were reported to the police (Albrecht, 2010).

4.4 | Hypotheses

Previous empirical research suggests that contact with the criminal justice system affects SAT's causes of crime. The findings indicate that system contact decreases personal morals and increases deviant peer associations and risk perceptions. These empirical results generally align with predictions that can be derived (under particular circumstances) by mechanisms postulated in SAT, labeling theory, or deterrence theory.

However, the theories mentioned above also acknowledge that the effects of official contact depend on individuals' previous crime-related experiences not only with formal control agents (e.g., the police) but also within their informal social network (e.g., with parents or peers). They imply that the more personal morals, peer associations, and risk perceptions were formed by previous (consistent) experiences, the less malleable they are.

Past experiences may be much different in the English and German samples than in previous studies that were based mainly on U.S. samples. The U.S. juvenile justice system generally acts (through the police) in a more proactive way in searching for crimes, and the subsequent sanctions are more punitive than in the English and German systems (Howell et al., 2013; Huizinga et al., 2003; Snyder & Sickmund, 1999). Therefore, juveniles in the United States who have had contact with the criminal justice system may, on average, due to their higher risk of detection when committing crimes, not only have less experience with delinquency than their counterparts in England or Germany at the time of the official contact but may also be subjected to more intrusive interventions.

To derive a more informed hypothesis on the impact of criminal justice contact, one must thus consider the criminal and sanctioning history of the treated individuals (i.e., of those who had contact with the juvenile justice system) in the English and German samples (see online supplementary material S1). Information about this history indicates that 15-year-old delinquents who had official contact with the criminal justice system were indeed already typically quite experienced before this contact. That is, they reported a relatively high number of acts of (undetected) crime before their system contact.

In most cases, the individuals committed these crimes together with friends, and it is not unrealistic to assume that parents or teachers were also aware of at least some of their past offenses. Therefore, most youths probably had quite a few (informal) deterrence experiences before the investigated system contact. In this process, they already learned how likely or unlikely it is to be caught and how significant others in their social environment typically react to criminal activity. These experiences had already shaped and consolidated their morals, social associations, and risk perceptions, making them less malleable. In accordance with these learning processes, treated juveniles reported weaker personal morals and risk perceptions as well as stronger deviant peer associations than their untreated counterparts before their official contact (see section *Pretreatment covariate balance*).

The theories assume that an intensive intervention or a high detection or arrest certainty would be necessary to change such consolidated causes of crime. The severity and certainty of sanctions, however, were relatively weak among the individuals under investigation. On the one hand, they were apprehended only once or twice in the period of interest for the many crimes they committed (see online supplementary material S1). On the other hand, official reactions were relatively lenient (see section *The English and German juvenile justice system*). Against this backdrop, it could be argued that only relatively small official control effects on SAT's causes of crime can be expected among the examined English and German offenders.

H1: Official contact, on average, has no or only a weak impact on personal morals.

H2: Official contact, on average, has no or only a weak impact on deviant peer associations.

H3: Official contact, on average, has no or only a weak impact on detection risk perceptions.

4.5 | Data and methods

4.5.1 | Samples and design

The panel studies *Peterborough Adolescent and Young Adult Development Study* (PADS+; Wikström et al., 2012) and *Crime in the modern City* (CrimoC; Boers et al., 2010) provide longitudinal information from students in the cities of Peterborough (England) and Duisburg (Germany) who were 13-years old at the beginning of the projects.[82] The data in both studies stem from questionnaires and administrative registers providing self-reported information on various domains (including a person's temperament, family, neighborhood, and criminal involvement) and official records of the participants' criminal and sanctioning history.

PADS+ achieved its goal of sampling approximately one-third of a Peterborough student cohort by gathering information from 710 youths in its first wave of interviews in 2004. CrimoC's objective to survey an entire Duisburg school cohort, on the other hand, resulted in the collection of valid data from 3,411 juveniles in its first wave in 2002 (i.e., about two-thirds of the student population in Duisburg). Due to high retention efforts, PADS+ was characterized by exceptionally low panel attrition in the follow-up waves, which were first conducted annually and later at longer intervals. Of the 710 students, who responded in the first wave, 693 still participated in wave 5. Besides, 700 participants consented to the collection of their official contact records from the *Police National Computer* system. Unlike PADS+, CrimoC allowed new students to enter the study in follow-up panel waves (e.g., by entering a participating school). Partly due to the differences in design, CrimoC's retention rate was somewhat lower than that of PADS+. However, participation was still satisfactorily high, with more than 3,200 participants taking part in the first five waves. In CrimoC's fourth wave, most students (87.0 %; 2,964 of 3,405) also consented to the collection of their official criminal and sanctioning records from the *Erziehungsregister* and the *Bundeszentralregister*.[83]

To guarantee that each individual provides sufficient information for the treatment effect estimation, I defined two conditions for inclusion in the *final analysis samples*. Juveniles were included if they (1) participated in panel waves 3 to 5 and (2) consented to the collection of their official records. These criteria applied to 690 PADS+ students, who make up most of the original respondents (97.2%; 690 of 710). In CrimoC, the analysis sample consists of 2,117 juveniles, a more reduced subset of the total sample (62.2%; 2,117 of 3,405 participants in wave 4). Fulfilling the

[82] Online supplementary material S2 provides a brief comparison of both cities.
[83] For more information on PADS+ and CrimoC, see www.cac.crim.cam.ac.uk/research/padspres and www.crimoc.org or the publications Wikström et al. (2012) and Boers et al. (2010), respectively.

conditions less likely, youth with a high risk of criminal behavior and sanctioning experiences were disproportionately excluded from analyses in CrimoC (see online supplementary material S3.2). Hence, treatment effect estimates may not be representative for Duisburg's entire juvenile population but may be rather sample-specific.[84]

4.5.2 | Measures

The current study distinguishes three periods: *pretreatment* (T1), *treatment* (T2), and *post-treatment* (T3). This breakdown makes it easier to refer to particular periods in PADS+ and CrimoC and helps establish a proper causal time order between the covariates, treatment, and outcomes (see Table 4.1). The T2 treatment measures include information on system contacts in the year 2006 in PADS+ and in the year 2004 (March to December) in CrimoC. The T1 covariates and T3 outcomes (causes of crime), on the other hand, are mainly based on self-reports from questionnaires that were completed typically in the first half of the year before or after the system contact, respectively. Thus, the time between T2 official contact and T3 outcomes ranges between 1 month and a year, depending on the time the contact took place and the time the respondent completed the T3 questionnaire. The study, thus, analyzes the short-term impact of formal control on the causes of crime, which may differ from its long-term consequences as some influences may unfold only over a longer time period.

Table 4.1: Causal time order of measures

		Time periods		
Phase	**Ø-age**	**PADS+**	**CrimoC**	**Measures**
T1	14	01/ - 12/2005	01/2003 - 02/2004	Covariates
T2	15	01/ - 12/2006	03/2004 - 12/2004	Official contact
T3	16	01/ - 05/2007	01/2005 - 04/2005	Causes of crime

Notes: CrimoC's treatment period (T2) is shorter to take into account that some covariates (e.g., self-reported delinquency in T1) refer to the period from January 2003 to January/February 2004, whereas comparable measures in PADS+ refer only to the whole years (e.g., the entire year 2005).

The following presentation of the measures is restricted to the most crucial concepts. Online supplementary materials S3.1 and S3.2 provide additional insights into the measurements. The materials also give information about item non-response. Overall, CrimoC was much more affected by item-missingness than PADS+. While only 5.1 % (35 of 690) of the PADS+ juveniles had at least

[84] It is difficult to speculate on the precise implications of this systematic exclusion of high-risk youths. On the hand, it may diminish treatment effects, as high-risk youth may be exposed to more severe interventions that are deemed to have stronger effects on average. On the other hand, the excluded juveniles may be disproportionately offenders who have had a relatively high number of (informal) deterrence experiences before the official contact. Hence their causes of crime may be quite consolidated and consequently less malleable.

one missing value in the covariates, treatment, or outcomes, the same was true for most CrimoC participants (69.2%; 1.465 of 2,117). However, the average missing data proportion per individual was relatively low in both studies (PADS+: <1 %; CrimoC: 3.0 %) and was dealt with by multiple imputation in the current study (see section *Analytical procedures*).

Outcomes. All outcome variables are generated by computing the mean score of the indicators that measure the respective concept. Cronbach's alpha of the scores varies from 0.76 to 0.87 in PADS+ and 0.86 to 0.93 in CrimoC, indicating that the concepts were measured quite reliably in both studies.

PADS+ operationalized personal morals with three concepts, including personal moral rules (cognitive component) and anticipated emotions of guilt and shame when breaking a particular legal rule (emotive component). CrimoC, on the other hand, collected information about personal moral rules and additionally measured youths' general legal rule acceptance. The measures in PADS+'s were explicitly developed to test SAT and thus closely resemble the concept of personal morals as indicated by the theory. CrimoC's two measures, on the other hand, map the cognitive component of morality but do not (or only to a small degree) consider its emotive parts, depicted mainly by the emotions of guilt and shame according to SAT.

Personal moral rules were operationalized very similarly in both projects. Participants assessed how wrong (PADS+) or bad (CrimoC) they thought several delinquent acts were. For the evaluation of five offenses (burglary, graffiti spraying, robbery, shoplifting, smashed streetlight), PADS+ participants had four different response categories available ranging from (0) "not wrong at all" to (3) "very wrong." CrimoC respondents, on the other hand, evaluated seven delinquent acts (assault, burglary, bicycle theft, extortion, shoplifting, theft of a car, vandalism) using categories from (0) "entirely harmless" to (4) "very bad."

Moral emotions of guilt and shame were measured in PADS+ with questions concerning whether one would feel guilty or ashamed (in front of significant others) when committing different deviant or criminal acts (guilt: assault, break a parental rule, cheated on test in school, shoplifting, teased another school/work mate, theft from a car; shame: best friends/parents/teachers found out about shoplifting, best friends/parents/teachers found out about breaking into a car) and, if yes, how much. Response categories for the six items ranged from (0) "No, not at all" to (2) "Yes, very much."

CrimoC, instead, operationalized individuals' *general acceptance of legal rules* by asking about why one should not commit crimes. The ten listed reasons for abiding by the law were: "most of

the time you will get caught," "you just shouldn't do that," "it is important to respect the law," "it is important to follow the rules that others should obey, too," "it is worthwhile to have a clear conscience," "you are harming others who are innocent," "it is important to be a good example for others (e.g., children)," "it would be detrimental to me," "the possible sanction is just too high," "delinquency damages the reputation of one's family," "it is important to respect the law," and "it is valuable to have a clear conscience." Participants rated these statements on a scale from (0) "strongly disagree" to (4) "strongly agree."

Association with deviant peers (or *peer deviancy*) was measured in PADS+ by asking juveniles about their friends' involvement in six different deviant acts (assault/get into fights, get drunk, use drugs, shoplifting, skipping school, vandalism). They rated the frequency of involvement on a scale ranging from (0) "No, never" to (3) "Yes, very often (every week)." CrimoC, on the other hand, collected information about participants' *deviant peer group association* using six items: "There are other opposing groups.", "We also use violence to pursue our interests.", "We fight with other groups.", "When we show up together, others truly have respect.", "When we're together, we drink a lot of alcohol, too.", and "To have fun, we sometimes do something illegal." The participant assessed how much these statements applied to their peer group by using the response categories (0) "disagree" to (4) "totally agree".

Finally, the *perception of detection risk* (or deterrence sensitivity) was operationalized very similarly in both studies by asking the juveniles to estimate the risk of getting caught for various crimes. In PADS+, the detection risk for four offenses (assault, shoplifting, theft from a car, vandalism) was rated using the response categories (0) "No risk at all" to (3) "A very great risk." In CrimoC, participants evaluated the risk of getting caught for seven different crimes (assault, bicycle theft, burglary, extortion, shoplifting, theft of a car, vandalism) on a scale ranging from (0) "very unlikely" to (4) "very likely."

Treatment. Juvenile justice contact, the treatment, differentiates between juveniles who had an official record of at least one criminal activity within period T2 and those who did not. While in PADS+, 37 (out of 690; 5.4%) participants had an official record of contact with the juvenile justice system within T2, the same is true for 88 (out of 2,117; 4.2%) CrimoC participants. Most of the treated respondents generally experienced a rather lenient system contact in both systems (see Table 4.2). Most cases were diverted out of the official system (PADS+: 27 out of 37, 73.0%; CrimoC: 71 out of 88, 80.7%), and only a very few juveniles were imprisoned (PADS+: 1 out of 37, 2.7%; CrimoC: 3 out of 88, 3.4%). Hence, most individuals who were officially recorded for a crime at T2 had their case dismissed with only minimal system contact (typically including a

stern talking to by police officers or an official latter by the prosecution), or had their case dismissed but were encouraged to participate voluntarily in a rehabilitation program (England) or on the condition that there was some educational intervention by the youth's parents, school, or employer (Germany). Although the studied system contact was the first for most treated juveniles, some had had contact before (16.2 % or 12.5% had an official contact at T1 in PADS+ or CrimoC, respectively).

Table 4.2: Type of formal reaction and offenses the juveniles were recorded for

Formal reaction	**ENG**	**GER**	**Offense type**	**ENG**	**GER**
Diversion	73.0 %	80.7 %	Violent offenses	21.6 %	24.1 %
Conviction	27.0 %	19.3 %	*Offenses against the person*	*18.9 %*	*20.7 %*
Non-custodial measures	*24.3 %*	*15.9 %*	*Robbery*	--	*3.4 %*
Short-term detention	--	*3.4 %*	*Sexual offenses*	*2.7 %*	--
Long-term imprisonment	*2.7 %*	--	Property offenses	37.8 %	43.7 %
			Theft & handling	*24.3 %*	*18.4 %*
			Aggravated theft/burglary	*10.8 %*	*11.5 %*
			Fraud	*2.7 %*	*13.8 %*
			Vandalism	13.5 %	1.1 %
			Motoring offenses	2.7 %	26.4 %
			Drug offenses	5.4 %	2.3 %
			Other (weapons, threats)	18.9 %	2.3 %

Notes: ENG = English sample (PADS+); GER = German sample (CrimoC); n(PADS+) = 37; n(CrimoC) = 88.

Covariates. Following recommendations by experts (e.g., Kainz et al., 2017; S. L. Morgan & Winship, 2015), the selection of 52 covariates was based on theories that either explained why juveniles had contact with the juvenile justice system or/and why they varied in the outcomes of interest. All covariates selected in this way either refer to a time within T1 or are time-invariant. They include the baseline (T1) outcomes because these are among the most helpful variables to condition on when identifying causal estimates (S. L. Morgan & Winship, 2015; Steiner et al., 2010). In addition to these lagged outcomes, other covariates come from different domains, including a person's criminal and deviant history, personal characteristics (e.g., temperament/character), and social associations (e.g., peers and family). For each domain, multiple indicators were included to diminish selection effect threats (Steiner et al., 2010; for a list of all domains and covariates, see online supplementary material S3.1 and S3.2).

4.5.3 | Analytical Procedures

For all outcomes, the estimand of interest is the *Average Treatment Effect on the Treated* (ATT; S. L. Morgan & Winship, 2015). The ATT is generally defined as the average difference in two potential outcomes, Y_i^1 and Y_i^0, among individuals who were actually treated (Tr = 1)[85]:

$$ATT = E[Y_i^1 - Y_i^0 \mid Tr = 1] = E[Y_i^1 \mid Tr = 1] - E[Y_i^0 \mid Tr = 1].^{86} \quad (1)$$

In this study, Y_i^1 refers to a juvenile's outcome (e.g., risk perception) at T3 if she or he had had official contact in T2. Y_i^0, in contrast, refers to the potential outcome at T3 if the same juvenile had not had official contact in T2. Because treated juveniles had official contact in T2 by definition (in the current study: treatment = official contact), Y_i^1 is observed for all of them. It can, therefore, easily be entered into the ATT equation. The information for Y_i^0 is, however, missing among the treated individuals because they did not experience the counterfactual state in which they had no system contact. Therefore, the ATT cannot be computed with observed data alone.

I used matching methods to infer the ATT for the juveniles with official contact from their counterparts with no official contact who had similar values on all key pretreatment characteristics (Stuart, 2010). In particular, I followed four matching steps. The goal of the *first three steps* was to find a *propensity score matching* (Apel & Sweeten, 2010; Rosenbaum & Rubin, 1983) procedure that best balanced the treated and control group in terms of their covariate distributions. A propensity score refers to the probability that an individual is treated. In the first step of the matching procedure, I estimated multiple propensity scores for each individual using the covariates as predictors in three different estimation procedures.[87] Second, I utilized the various computed propensity scores in different matching algorithms.[88] In a third step, I selected the best propensity score matching combination by using so-called balance statistics that indicate how well balanced the treated and untreated groups are in terms of covariate distributions after the respective matching (Kainz et al., 2017). The same propensity score matching procedure does not need to be the best in balancing both the English and German samples. In fact, different matching techniques

[85] From a policy-perspective, it seems reasonable to narrow the treatment effect to treated individuals because they are the ones in danger of being apprehended and sanctioned.
[86] E[.] refers to the expectation operator from probability theory. In this article, the expectations are averages of particular quantities.
[87] I included 35 and 52 covariates as predictors in PADS+ and CrimoC, respectively. For the covariates included in propensity score estimation, see appendices A3.1 and A3.2. Propensity scores were estimated with the following three procedures: Bayesian logistic models (McElreath, 2016), Bayesian additive regression trees (Chipman et al., 2010), and Covariate balancing propensity score estimation (Imai & Ratkovic, 2014). All computations were conducted in *R* (version 3.5.2). For a list of all R packages used, see online supplementary material S6.
[88] The applied matching algorithms are (Stuart, 2010): (1-5) nearest neighbor matching with replacement, a caliper of 0.25, and ratios of 1:1 to 1:5, (6-11) optimal matching with ratios 1:1 to 1:5, and (12) weighting by the odds.

delivered the best balance for each sample. Therefore, ATT estimates derived by different matching procedures are reported for each sample. This trial and error approach in the first three matching steps is recommended because a good covariate balance diminishes the threat of selection bias due to pretreatment differences in observables (e.g., Kainz et al., 2017; S. L. Morgan & Winship, 2015).

Whatever the most successful matching technique, the resulting best-balanced samples were finally used in the fourth step in regression analyses to estimate the ATTs and their uncertainty. In particular, normal linear regressions were applied using the measures of personal morals, deviant peer associations, and risk perceptions one after the other as dependent variables. In each case, the regression model included as predictors the treatment indicator, the lagged outcome variable, and their interaction term.[89] The matched nature of the sample was taken into consideration using weights—the final product of the first three analytical steps.

Because the final samples were affected by item non-response, all analytical steps (propensity score estimation, matching, outcome analyses) were applied to multiple imputed data sets.[90] As recommended by Penning de Vries and Groenwold (2017), propensity score estimation, matching, the generation of weights, and outcome analyses were conducted for each imputed data set. The imputed information was finally combined by merging the vectors of all ATT simulations.

Each step in the analytical procedure was, furthermore, conducted using different combinations of multiple imputation, propensity score estimation, matching, and outcome methods to check the robustness of the ATT estimates to different method combinations (Young & Holsteen, 2016). Overall, 48 method combinations in PADS+ and 60 in CrimoC were included as promising candidates in model dependence assessments. Promising candidates are those method combinations that successfully balanced the treatment and control groups regarding covariate distributions. To assess model sensitivity, I computed the ATT median point estimate of each candidate model and plotted their distribution using density plots.

[89] For robustness checks, I additionally ran models including (1) only the treatment as independent variable (weighted mean differences), or (2) a relatively rich set of predictors.

[90] Multiple imputation embraces the uncertainty in the ATT estimation that is due to missing data by predicting missing values multiple times. I predicted the missing values in the current study for each variable with predictive mean matching within *fully conditional specification* (van Buuren, 2018). In doing so, I produced 70 imputed data sets for CrimoC, but only 12 for PADS+. Because PADS+ was less affected by item non-response than CrimoC, fewer imputations should suffice to generate reproducible results (van Buuren, 2018). In addition to predictive mean matching, I also applied other imputation procedures (e.g., random forests). These sensitivity analyses show that the ATT estimates are relatively robust to the type of imputation technique used (see online supplementary material S5).

4.6 | Results

This section first reports the covariate balance before and after adjustment. In doing so, it addresses how well each selected matching scheme balanced the groups of individuals with and without official contact and thereby diminished the threat of selection bias. In a second step, the section presents the ATTs computed with the help of the adjusted samples. Finally, the model dependence of the ATT estimates is assessed.

4.6.1 | Pretreatment Covariate Balance

As recommended by experts (Kainz et al., 2017), I assess the covariate balance by using the balance statistics standardized bias and variance ratios. Standardized bias (SB) is the difference in covariate means between the treated and control group divided by the treated individuals' standard deviation. Variance ratios (VRs) of continuous covariates are computed by taking the ratio of variances observed in the treatment and control group for the covariate at hand. The literature identifies SB values below 0.1 (0.2, 0.25; the literature is not settled on a threshold) and VRs between 0.5 and 2 as an indicator of a reasonable balance in a covariate's distribution across the treatment and control group (Harder et al., 2010; Kainz et al., 2017).[91] Due to limitations of space, Tables 4.3 and 4.4 report balance statistics only for the lagged outcomes. Online supplementary material S4 includes balance statistics for the rest of the covariates.

4.6.1.1 | English sample (PADS+)

Before matching, treated and untreated English juveniles differed substantially concerning key pretreatment characteristics (see Table 4.3). Most (42 of the 52) covariates exhibited SBs larger than 0.1, thus indicating an imbalance between the groups. Many (29) covariates still exceeded a less stringent threshold of 0.2. The mean (0.16) and the median (0.10) of the absolute SB across variables were also beyond the threshold. All lagged outcome variables had absolute SB values of 0.4 or larger, with the highest standardized difference in the peer deviancy measure. Its difference of 0.71 implies that individuals with official contact reported much more peer deviancy at T1 than individuals without official contact. Contrary to SB statistics, VRs of continuous covariates point to only a few substantial differences in covariate distributions across the treated and untreated

[91] Variance ratios were standardized so that they are always larger than 1. Consequently, ratios above 2 indicate covariate imbalance. For categorical covariates, raw proportional differences are used as balance statistics, although no thresholds exist for them so far (Kainz et al., 2017).

group in PADS+. Most continuous covariates (13 out of 16) had VRs of well below 2, and the mean and median of the VRs across covariates were only 1.63 and 1.40, respectively. Overall, balance statistics indicate that groups were quite imbalanced before matching and that this was especially true for the lagged outcome variables.

Table 4.3: Covariate balance statistics for the English sample (short version)

English sample (PADS+)	Unadjusted Sample		Adjusted Sample	
	SB	VR	SB	VR
COVARIATES - LAGGED (T1) OUTCOMES				
Personal moral rules	-.53	2.04	-.05	1.10
Moral shame	-.55	1.52	-.02	1.27
Moral guilt	-.50	1.66	.05	1.40
Deviant peers	.71	1.32	-.17	1.38
Detection risk perceptions	-.42	1.86	-.01	1.68
GLOBAL COVARIATE BALANCE STATISTICS				
Mean (absolute)	.16	1.63	.05	1.28
Median (absolute)	.10	1.40	.04	1.28
Maximum (absolute)	1.02	3.47	.32	1.74

Notes: SB = Standardized bias; VR = Variance ratio.

According to the balance statistics, *optimal matching* with a 1:3 ratio without replacement on the linear propensity score estimated via Bayesian Additive Regression Trees (Chipman et al., 2010) best balanced the English treatment and control groups. Optimal matching is a procedure that minimizes a global distance measure by matching control units to treated ones (Hansen, 2004). The matched English sample included an adjusted number of 148 (37 treated and 111 control) juveniles. Optimal matching significantly improved the balance across the treated and control groups (see Table 4.3). After the procedure, 17 covariates exceeded the SB threshold of 0.1, whereas the same was true for only one variable considering the less stringent 0.2-threshold. The mean and median absolute SB across covariates also diminished (from 0.16 and 0.10) to values of 0.05 and 0.04. Juveniles with official contact, however, were still slightly more involved with legal actors before their contact, had lower self-control capabilities, perceived the risks of consequences (when committing crime) as lower, had more disadvantaged families, reported more informal social control in their neighborhood, and were less successful and socially integrated in school than juveniles without official contact (see online supplementary material S4). In terms of the lagged outcome variables, treated and untreated English juveniles were balanced satisfactorily after matching, except for peer deviancy. Juveniles with official contact reported less (SB = -0.17) peer deviancy than matched youths without official contact. VRs were all well below the threshold of 2 after matching. Overall, treated and untreated groups are much more similar as a result of the optimal matching procedure. Hence, we are much closer to comparing “apples to apples” in the

forthcoming analyses, as Morris and Piquero (2013, p. 848) put it. Accordingly, it is less likely that differences in pretreatment characteristics confound the ATT estimates derived with the matched sample. To further reduce the risk that the remaining imbalances bias the treatment effect estimates, the regression models used for ATT estimation include the respective lagged outcome variable as a predictor (see section *Analytical procedures*).

4.6.1.2 | German sample (CrimoC)

In the German sample, covariate imbalance before adjustment was considerably lower than in the English sample (see Table 4.4). Only half of the covariates (28 out of 52) exceeded the 0.1-SB-threshold (10 covariates the threshold of 0.2). Across all variables, the mean and median absolute SBs were just 0.07 and 0.04. However, all lagged outcome variables exceeded the 0.1-threshold, and deviant peer group association as well as general legal norm acceptance were among the most imbalanced of all variables, with values of 0.37 and -0.35, respectively. Juveniles who had official contact at T2 reported that at T1, they were more strongly involved in deviant peer groups and felt less bound to legal norms than their counterparts without official contact. VRs, in contrast, were already all well below the threshold of 2, indicating no substantial differences in the variances of the continuous covariates across the treated and the untreated groups. In a nutshell, covariate distributional differences, although less substantial than in the English sample, were still large enough to potentially bias treatment effect estimates.

Table 4.4: Covariate balance statistics for the German sample (short version)

German sample (CrimoC)	Unadjusted Sample		Adjusted Sample	
	SB	**VR**	**SB**	**VR**
COVARIATES - LAGGED (T1) OUTCOMES				
Personal moral rules	-.12	1.23	.00	1.06
General legal norm acceptance	-.35	1.46	-.03	1.12
Deviant peer group association	.37	1.35	.03	1.06
Detection risk perceptions	-.21	1.04	.00	1.12
GLOBAL COVARIATE BALANCE STATISTICS				
Mean (absolute)	.07	1.23	.01	1.13
Median (absolute)	.04	1.24	.00	1.10
Maximum (absolute)	.37	1.46	.03	1.51

Notes: SB = Standardized bias; VR = Variance ratio.

The balance statistics indicate that the method that best balanced the German treatment and control groups was *weighting by the odds* on the Covariate Balancing Propensity Score (Imai & Ratkovic, 2014). Weighting by the odds puts more weight on individuals from the control group who are more similar to treated persons on the propensity score than on less similar control units to weight

the whole control group up to the treatment group (Harder et al., 2010). The weighted sample includes an (efficient) adjusted number of 300.6 (88 treated and 212.6 control) juveniles. Weighting by the odds diminished the distributional imbalance in observed covariates across the board (see Table 4.4; online supplementary material S4): Not a single covariate was imbalanced after weighting. The mean of the absolute SB across variables is very close to null (0.01; median: 0.00), and the VRs' value of 1.13 is not far from a perfect variance balance across treated and untreated participants. For the German sample, the weighting procedure likely prevents potential selection bias due to observed covariates. To use Morris and Piquero's (2013) language, it appears we are comparing the same varieties of apples in the weighted treated and control groups. Consequently, the likelihood of confounding by pretreatment characteristics is vastly diminished.

4.6.2 | Effects of System Contact on the Causes of Crime

The effects of official contact on the causes of crime (estimated with equation 1) were mostly relatively small in both studies (see gray-shaded areas or black solid lines and dots in Figure 4.2). First, official contact hardly affected personal morals, with most effect estimates being not significantly different from null. For example, young offenders in PADS+ changed *personal moral rules* on average only by 0.01 [89%-CI[92]: -0.08 0.11] and in CrimoC by 0.08 [89%-CI: -0.08 0.23] because of official contact. Additionally, formal contact seemed not to or only weakly influence the *moral shame* of English offenders (0.00 [89%-CI: -0.08 0.07]) and the *general legal norm acceptance* of German offenders (-0.01 [89%-CI: -0.20 0.20]). However, as an exception, the ATTs suggest that *moral guilt* among English offenders may have been affected more substantially by official contact. The effect probably ranged from modestly decreasing to rather unsubstantial (-0.11 [89%-CI: -0.17 -0.05]). Whereas juveniles with official contact anticipated that they would, on average, not have felt guilty or would have felt only a little guilty after committing a crime ($E[Y^1]$ = 0.85), estimates indicate that they would have felt slightly more guilty on average if they had not had official contact ($E[Y^0]$ = 0.96 [89%-CI: 0.90 1.02]).

Second, ATT estimates suggest that the *deviant peer group association* of German offenders was not substantially affected by system contact (-0.08 [89%-CI: -0.29 0.13]). In contrast, English juveniles reported more *peer deviancy* in the following year on average. The ATT indicates that

[92] I report 89% confidence intervals (CIs) instead of the usual 95%-CIs to highlight that the classic choice of a 95%-CI over any other interval is arbitrary, and that the end of a CI should not be interpreted as particularly important but just as a description of how many simulations (e.g., 89%) lie within a particular range (see McElreath, 2016). I also abstain from reporting p-values as these encourage binary thinking (significant versus not significant).

treated English juveniles had around 0.20 [89%-CI: 0.09 and 0.30] higher peer deviancy scores after official contact with the justice system in T2 compared to a (hypothetical) situation in which they would not have had official contact. The model implies that the treated youths would have reported a deviant peer score of around 1.34 [89%-CI: 1.24 1.44] on average if they had not had official contact in the previous year, while their observed average score was 1.54.

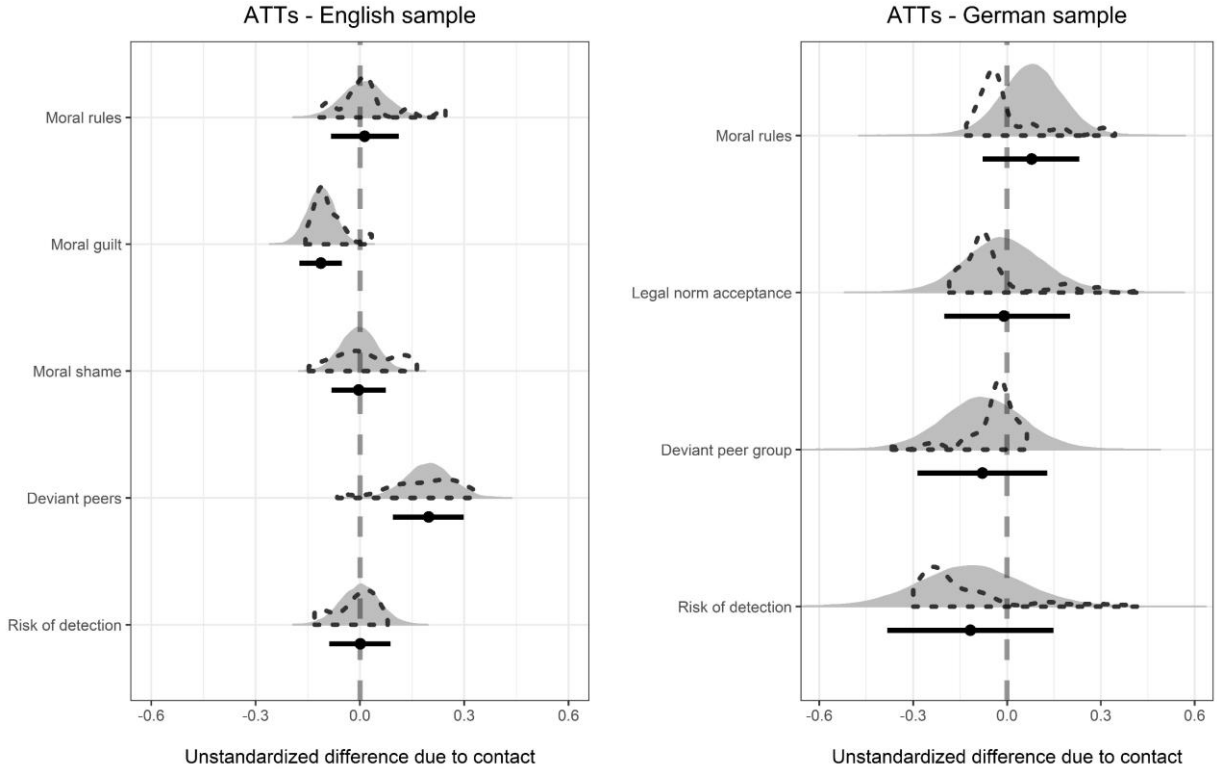

Figure 4.2: Average Treatment Effect on the Treated (ATT) estimates

Notes:

Gray-shaded area	*= Distribution of ATT simulations of the best-balancing model;*
Black dots	*= Medians of ATT simulations of the best-balancing model;*
Black lines	*= 89% confidence intervals of the best-balancing model;*
Dotted black lines	*= Distribution of medians of the ATT simulations of all candidate models.*

Finally, according to the treatment estimates, official contact appears to have had no substantial effect on juveniles' detection risk perceptions. The effect estimates for treated German participants are, however, relatively imprecise. They allow for both small increases and moderate decreases in detection risk perception due to official contact (-0.12 [89%-CI: -0.38 0.15]). Therefore, we cannot learn much from this estimate. In contrast, the ATT for the English sample indicates that the official control effect on detection risk perception is at best relatively small (0.00 [89%-CI: -0.09 0.09]).

4.6.3 | Assessment of Model Robustness

The robustness of the ATT estimates to changes in the modeling procedure was assessed by density plots (see dotted lines in Figure 4.2). They imply that the treatment estimates are generally more model-sensitive in the English than in the German sample.[93]

Whereas ATT median point estimates for the English youths were relatively insensitive to alternative modeling procedures for moral rules, moral guilt, and detection risk perception, the effect estimates on moral shame and peer deviancy were less robust. For moral shame, point effect estimates vary from small increases to small decreases depending on the methodological approach used. For peer deviancy, density plots imply that effect sizes may vary from relatively unsubstantial (although positive) to moderate increases. Consequently, the latter two outcomes must be discussed with much more caution than the others.

For the German sample, the medians of the ATT estimates are all quite robust to changes in the methodological approach. Especially for moral rules, legal norm acceptance, and deviant peer group association, different methodological approaches nearly all lead to very similar results. The outcome that is most sensitive to the modeling procedure is the detection risk perception. The different methodological approaches produced estimates indicating that the effects on risk perceptions ranged from relatively unsubstantial to weakly decreasing. Against this background, ATTs of risk detection must be interpreted with caution for the German sample.

4.7 | Discussion

Why do criminal justice contacts often have no crime-preventative impact? To shed light on this question, the current study examined the influence of system contact on factors that may mediate official contact effects on reoffending. Unlike previous empirical studies, which mostly tested either deterrence or labeling theory, the current study defined SAT's causes of crime as potential mediators. Situational Action Theory was chosen as the theoretical framework primarily for the following reasons. First, SAT provides terminology that clearly distinguishes between factors that directly cause crime and more distal factors. Second, using SAT as a theoretical framework allows for simultaneous integration of assumptions about contact-induced mechanisms by both deterrence and labeling theory. The causes of crime identified by SAT and selected for analysis were personal morals, deviant peer associations, and risk perceptions. The analysis explored whether contact with

[93] For more information on the model robustness of the ATTs, see online supplementary material S5.

the English and German juvenile justice system influenced these causes of crime. The results supplement the sparse body of research outside the U.S. that had previously studied the effects of system contact on antecedents of crime.

The ATT estimates suggest that the mostly lenient system contacts in this study had relatively minor effects on most causes of crime. In the German sample, neither the young offenders' morals nor their deviant peer group association or detection risk perceptions were significantly—or substantially—affected by official contact. In the English sample, three of the five effects were statistically insignificant. The significant estimates suggest that contact had a weak guilt-reducing effect and moderately amplified peer deviancy among the English juveniles (with the latter finding challenged by some sensitivity analyses producing smaller effect estimates). Overall, the results indicate—in line with Hypotheses H1 to H3—that official controls did not (CrimoC) or only weakly (PADS+) trigger crime-relevant processes. Although not analytically testable due to different measures, the results seem furthermore to suggest that effects were more likely among English than German youths. These more substantial findings in the English sample indicate that system contact triggers criminogenic rather than crime-preventative mechanisms. This missing or adverse impact of official control may explain why most previous studies (including one using the data at hand; Boers et al., 2022) found no crime-preventative effects. The results, furthermore, indicate that the components of morality may be influenced differentially by a system contact. While the cognitive part (moral rules) was unaffected in both samples, the findings suggest that system contact may have decreased feelings of guilt. Future research should try to replicate these differential effects.

How can it be explained that the current study finds, in contrast to previous research, mostly insignificant effects on the examined intermediate factors and that this is especially true for the German study? A first explanation for these findings may be a low certainty and lenient nature of the system contacts in this study compared to previous studies. The studied juveniles mainly were not detected when they committed a crime, and if they were, were they mostly diverted out of the formal system. Prior studies, in contrast, were primarily based on U.S. samples, in which the juveniles studied were likely to have been detected with higher certainty and handled more harshly on average due to the more proactive and punitive criminal justice system in the U.S. (Howell et al., 2013; Huizinga et al., 2003; Snyder & Sickmund, 1999).[94] Deterrence and labeling theory

[94] A cross-national study of the U.S. and German system, for example, shows that the percentage of officially arrested juveniles was much higher in the United States than in Germany and that the arrested U.S. juveniles were dealt with more harshly by the juvenile justice system than their German counterparts (Huizinga et al., 2003).

suggest that the more intrusive and certain (consistent) official interventions are, the stronger their effect should be. This assumption is backed up by past empirical findings indicating that the certainty and severity of police contact and sanctions indeed play a crucial role in influencing young offenders' future delinquency and causes of crime (e.g., Anwar & Loughran, 2011; McAra & McVie, 2007; Wiley et al., 2013). The mostly weak control effects found in this study, hence, do not seem to differ substantially from the theories' predictions, as the interventions were lenient and uncertain for most of the treated offenders in this study. Furthermore, the more substantial effects in England may be explained by the fact that the English system reacted more proactively and intensively to crime than the German one.

A second and related explanation for the minor effects of system contact may be that the causes of crime were probably already quite solidified before the contact and, therefore, less malleable. Due to the low risk of detection, juveniles probably already had a relatively high number of informal deterrence experiences before the formal intervention. Through these experiences, they had already learned how likely it is to be caught and how (informal) others react to their misbehavior. These learning processes led over time to quite consolidated morals, risk perceptions, and peer associations. Labeling and deterrence theory indicate that rare and non-intensive formal interventions should have a relatively weak influence on more experienced offenders. As police in England acted more proactive than German police in the studied time period, English offenders may have been less experienced with crime and sanctioning when they had their system contact. This inexperience with formal and informal reactions may explain why official contact was more influential among English youth.

4.7.1 | Limitations and Perspectives

This study has some limitations that future research should resolve. First, it cannot test the two arguments that official contact influences the causes of crime differently (a) depending on its nature and certainty, and (b) depending on the offender's previous criminal and sanctioning history. To test the argument that more intense interventions should have larger effects, future examinations must differentiate between various levels of system contact (e.g., diversion versus non-custodial sanctions versus imprisonment). However, to conduct a more differentiated investigation of this kind, large samples are needed that typically have not been available in existing studies to date (including the current one). For this reason, only a few empirical studies (e.g., McAra & McVie, 2007) have so far distinguished between various intervention levels, let alone conducted analyses

of mediating factors and mechanisms to explore how these factors are affected by different sanctions. To test for the effect of the certainty of punishment more directly, future studies should follow the growing deterrence literature on Bayesian updating (e.g., Anwar & Loughran, 2011). This literature typically operationalizes the treatment variable as the ratio of arrests per crime. It thereby directly explores the impact of the strength of a deterrence signal within a particular period on subsequent risk perceptions. A follow-up study with the present data will rely on such a crime-arrest ratio to study the Bayesian updating predictions more directly. Finally, to test the second argument that contact effects depend on the treated person's criminal and sanctioning history, analyses of subgroups divided by their number of prior crime-related experiences are needed. So far, existing research supports the argument that contact effects are smaller among more experienced offenders (e.g., Chiricos et al., 2007; J. T. Ward et al., 2014). However, the few studies that investigated this hypothesis analyzed official contact effects on reoffending. Future research should also study the differential impact of formal interventions on the direct antecedents of crime.

A second limitation of the current study is that it—like all observational designs—is not immune to selection bias arising from potentially unobserved confounders. Having matched on a relatively large number of important observed covariates, this analysis and other more recent studies should, however, be able to account for most of the crucial differences in pretreatment characteristics between treated and untreated individuals. Future research should, nevertheless, be conducted with the explicit goal of studying the impact of criminal justice contact on the causes of crime. It should therefore aim to measure and balance all the covariates that may theoretically confound the effect estimates.

Third, like some previous research (e.g., Wiley et al., 2013), the current study measures the causes of crime by combining items that tap into various forms of deviant and criminal acts. The construction of such global measures, however, is only valid under the assumption that official contact influences various forms of behavior independent of the type of criminal act one was apprehended for. This assumption may be problematic as SAT and other theories (e.g., rational choice approaches) assume that committing a crime and learning from its consequences (e.g., formal contact) are probably action-specific processes (e.g., Anwar & Loughran, 2011; Wikström et al., 2011). Some existing research (including this study) may thus underestimate the effects of official intervention for the specific sanctioned behavior. Therefore, future research should investigate how formal contact for a particular type of criminal activity may influence action-specific morals, peer activities, and risk perceptions (and how this differs from its effect on more global measures).

To do this, larger sample sizes are needed that provide the power to analyze offense-specific effects.

Fourth, although the current study takes a cross-national perspective, it cannot formally test the differences in effect sizes between the English and German samples and how they relate to previous U.S.-based studies. That is because the measures in the two studies differ, and no U.S. data were available. The current study's findings and interpretations are thus explorative in nature. They should trigger future research that is already set up cross-nationally from the beginning to explore differential system effects more directly. So far, although called for by experts (e.g., Bernburg, 2019), cross-national designs were applied only very scarcely in the research of sanctioning effects (for an exception, based on samples that were initially not designed for comparison, see Huizinga et al., 2003). However, large-scale cross-national designs should supplement studies that delve deep into one system by showing how various criminal justice systems have a differential impact on apprehended individuals. At best, such studies are also able to consider treatment heterogeneity in more detail by exploring whether differential effects across different settings are due to the prevalence of particular system responses (e.g., one system reacts more with diversion than others) or due to how the same interventions types are implemented across settings (e.g., systems vary in their diversions practices).

Fifth, although the current study is the first one using SAT as a starting framework to investigate official contact effects on potential intervening factors, it does not test SAT's full theoretical model (see Figure 4.1). It focuses on the impact of official contact on specific causes of crime, ignoring the subsequent effect of these causes of crime on delinquency. A structural equation model (SEM) would have been able to study the full model (see Wiley et al., 2013), which could not be accurately estimated with the data at hand. This is because, according to SAT, the causes of crime interact in a relatively complex way to explain delinquency (Wikström et al., 2012). The study of such interaction effects is only possible with a rather large number of observations. However, this number is typically relatively small in studies based on samples from general populations, as these include only a few juveniles with official contact in a specific period. As this is also true of the current study, it does not rely on SEM. Nevertheless, I believe that the analysis of only a part of the full model is still an informative approach to study the research question at hand, since only when the causes of crime are affected by official contact can they ultimately alter delinquency.

Finally, although, in my view, SAT constitutes a promising framework for a general theory of sanctions, other approaches may be similarly helpful to study official contact effects in their complexity. All general theories of crime may be fruitful starting points for a general theory of

sanctions, as they all specify the factors that should be directly causally relevant for offending. Although many often-tested general theories of crime exist, they have—at least to my knowledge—so far not been applied to investigate sanctioning effects empirically. Instead, most studies have tested classic sanctioning theories such as deterrence or labeling theory (e.g., Anwar & Loughran, 2011; Bernburg et al., 2006; Wiley et al., 2013). Despite the value of testing these theories, I agree with Piquero and colleagues (2011) that it may be helpful for future researchers to move beyond the testing of single sanctioning theories. More general frameworks of investigation, as applied here, have the advantage of being able to integrate and study mechanisms postulated by theories as diverse as deterrence and labeling theory.

4.7.2 | Conclusion

The findings in the current study imply that official contact significantly affected only a few of the causes of crime in the English juvenile justice system and had basically no impact in the German system. These findings contrast with results of previous U.S. studies, which generally found more substantial control effects on the examined causes of crime. Most of these operated in directions that amplified rather than diminished reoffending. Against this backdrop, a less proactive and punitive control style—as is used in most European countries—may be, on average, the better approach, if not to diminish juvenile delinquency, then at least not to exacerbate it. Diversion, in particular, might be seen as a mechanism that by construction does not trigger strong processes, be they crime-amplifying or crime-preventing. Given that the current state of research indicates that traditional sanctions rather amplify than prevent future crime, diversion may be seen as a more efficient and cost-saving way to handle non-serious juvenile offenses than traditional criminal justice processing (see Petrosino et al., 2014). Juvenile justice systems may make diversion practices even more fruitful by implementing family-focused programs that help parents communicate and enforce rules and keep their children away from deviant peers. Research suggests that such family-oriented diversion programs may be particularly efficient in reducing delinquency (Schwalbe et al., 2012). This efficiency may be explained by the fact that moral education and parental monitoring are directly related to factors (personal morals, deviant peer associations) that SAT deems as causes of crime (Wikström & Treiber, 2016a).

5 | PAPER IV

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Revisiting the experiential effect: How criminal offending affects juveniles' perceptions of detection risk

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ABSTRACT

Perceptual deterrence research has consistently found that criminal offending is inversely related to subsequent perceptions of the risk of being caught or arrested. This inverse relationship has been dubbed an "experiential effect," reflecting the idea that people learn by committing (undetected) crimes that the detection or arrest risk is lower than first feared. The current study explores the validity of this experiential argument. It relies on self-report data from 3,259 adolescent participants in the panel study Crime in the modern City (Duisburg, Germany). We computed detection rates and risk perceptions, and used fixed effects models to investigate the proposed experiential learning process. Most findings support the experiential argument: (1) juvenile offenses were rarely detected by the police, (2) juveniles (especially those inexperienced with crime) tended to overestimate the detection risk, (3) juveniles reduced their risk perceptions when they committed crimes, (4) this reduction occurred primarily among those who overestimated the detection risk in periods when they were not committing crimes. However, the study also produced the surprising finding that the experiential effect seems to be short-lived: People appeared to return to initial risk perception levels when they stopped committing crimes. Overall, the results corroborate the experiential argument. However, they also indicate that the argument may need revision to account for the potential short-term nature of the experiential effect. This "ephemerality effect" is good news for policy, as lowered risk perceptions will in most cases only temporarily increase the likelihood of future delinquency.

KEYWORDS

deterrence theory, experiential effect, naiveté effect, novelty effect, detection risk, risk overestimation

5.1 | Introduction

The threat of sanctions is omnipresent in modern societies. It is evident in security guards and detectives in stores; police patrolling by foot, car, and bicycle; and video surveillance in public and private spaces. As early as the eighteenth century, Cesare Beccaria (1764/1872) outlined the principles behind the threat of punishment. He argued that if legal agents' punitive reactions to criminal offenses are sufficiently certain, severe, and swift, they will deter both the offenders and other individuals from committing (further) crimes.

In the mid-1970s, research on the mechanisms underlying these basic principles highlighted the importance of individual *perceptions* in the deterrence process (e.g., Geerken & Gove, 1975; Waldo & Chiricos, 1972). Based on the idea that it is solely what individuals perceive in a given situation that guides their actions, this research suggested that the legal (threat of) punishment can only prevent crimes when people process information about the punishment in a way that heightens their sense of imminent consequences. Successful information processing is reflected in the formation or revision of individuals' perceptions of sanction threats, including their perceptions about the certainty, severity, and celerity of punishment. With sanction threat perceptions as the central transmitter of punishment information, perceptual deterrence theory outlines two deterrence linkages or processes (see Pogarsky et al., 2004): a perceptual and a behavioral one.

According to the perceptual linkage, the legal (threat of) punishment can deter criminal behavior only indirectly by shaping perceptions of the certainty, severity, or celerity of punishment. How people form and update these sanction threat perceptions was outlined in Stafford and Warr's (1993) reconceptualization of deterrence theory. Stafford and Warr assume that individuals learn sanction threat perceptions through personal and vicarious (e.g., friends') experiences of punishment and punishment avoidance. Perceptions of punishment certainty are likely to increase when individuals are punished or observe others being punished, but should decrease through experiences of punishment avoidance. Perceptions of punishment severity and celerity are likely to be altered depending on how severe and swift the punishment is, but should not be affected by experiences of punishment avoidance.

According to the behavioral linkage, individuals who consider committing a crime may be deterred by (altered) sanction threat perceptions. To formalize this *deterrent effect*, perceptual deterrence research has often resorted to the use of subjective expected utility models (e.g., Matsueda et al., 2006; Piliavin et al., 1986). Inspired by Bentham (1789/2000), these rational choice models assume that deterrence happens when people abstain from illegal behavior because of its anticipated

costs, including the perceived sanction threats, and instead engage in legal behaviors that are associated with higher expected utility (i.e., a higher benefit-cost balance).

A decade after this shift to focus on perception, empirical research relied on panel designs to collect repeated self-report data and investigate the two deterrence linkages simultaneously (e.g., Bishop, 1984; Hirtenlehner & Wikström, 2017; Matsueda et al., 2006; Minor & Harry, 1982; Paternoster et al., 1985; Piliavin et al., 1986; Saltzman et al., 1982; Seddig et al., 2017). These panel studies explored how perceptions of detection or arrest certainty (hereinafter called *risk perceptions*) and criminal behavior affect each other over time.[95] Most of the aforementioned studies found weak to moderate effects of criminal conduct on subsequent risk perceptions: Individuals who reported (more) crimes had subsequently lower risk perceptions than individuals who reported no (or less) criminal offending. This inverse relationship was dubbed an "experiential effect" (Saltzman et al., 1982), reflecting the idea that individuals learn through their criminal experiences that the detection or arrest risk is relatively low and downgrade their risk perceptions accordingly. The behavioral linkage, in contrast, received less support from the results of the panel studies: Risk perceptions were often not or only weakly related to subsequent criminal offending.

The experiential effect estimates, however, do not necessarily reflect the proposed experiential learning process since they only show that criminal behavior is associated with lower risk perceptions. Their interpretation as experiential effects typically relies on the following assumptions (e.g., Hirtenlehner & Wikström, 2017; Paternoster et al., 1985; Seddig et al., 2017): (1) individuals are rarely detected (or arrested) when committing their crimes; (2) individuals without (much) criminal experience tend to overestimate the detection risk; (3) when these inexperienced individuals begin committing crimes, they reduce their risk perceptions over time as they learn through (repeated) undetected offending that the actual detection risk is lower than first feared. Although plausible, almost no study so far has tested all three assumptions of this experiential argument (see Nagin, 1998).[96]

In the current article, we provide a thorough test of the experiential argument by subjecting its assumptions to closer scrutiny. To do so, we first review research on (1) the low detection risk for crime, (2) the overestimation of detection risk by individuals with no or little criminal experience, and (3) the decrease in risk perceptions due to (undetected) offending among individuals who had no or little previous criminal experience. After summarizing these assumptions in the form of

[95] Fewer perceptual studies investigated the perceived severity or celerity of punishment (for a review of this research, see Paternoster, 2018).
[96] Only Lochner (2007) studied all three assumptions, but his data were limited in crucial aspects (see section 5.2.2).

hypotheses, the second part of the article empirically studies their validity using panel data from adolescents in Germany.

5.2 | State of research

5.2.1 | Low detection risk

The assessment that most criminal offenses go undetected and unpunished is far from new. In the first treatises on criminal statistics, researchers like Adolphe Quetelet (1842/2013) noted that their statistics included only those breaches of law that were recorded by official authorities (typically the police) and that there is a high "dark figure" of crime (Biderman & Reiss, 1967, p. 2). The primary reasons why delinquent acts "stay in the dark" (i.e., are not recorded) are that they are not *detected* and recognized as a crime, that they are not reported to official authorities even if recognized (e.g., by a victim), or that they are not recorded by the official authorities even if reported (Black, 1970).

To empirically estimate how large the actual risk of detection by the police (as key official authority) is, two types of information are necessary: the total number of crimes in which the police detected or identified the offender ($Crimes_{Detected}$) and the total number of crimes committed ($Crimes_{Total}$). A detection rate (DR) can then be computed by dividing the former by the latter number ($DR = Crimes_{Detected}/Crimes_{Total}$). Due to the filtering processes mentioned above (detection, reporting, recording), official crime statistics severely underestimate the criminal activity in a population and thus overestimate the true detection rate (Apel, 2013). Official data are, therefore, not a reliable source for the computation of a detection risk (but see Ahlberg & Knutsson, 1990).

To solve the problem of crime underestimation, researchers can rely on self-reports. Under the assumption that individuals disclose their criminal and detection experiences honestly and accurately,[97] self-report data allow for a more reliable estimate of the detection rate. Most previous perceptual panel studies, although based on self-reports, lacked data on detection and thus could not construct a detection rate (Hirtenlehner & Wikström, 2017). Other previous self-report studies, however, collected both crime and detection data and computed detection rates (per offense type) in one of two ways: either by dividing the total number of crimes detected by the police according

[97] Thornberry and Krohn (2000) noted that self-reports of crimes and detections (arrests) are reasonably reliable and valid, but that there is substantial underreporting. However, as long as both types of information are underreported to a similar degree, a detection rate will be relatively unbiased. Findings of Köllisch and Oberwittler (2004) confirm that respondents who underreport do so with respect to both their criminal activity and their contact with the police.

to the offenders by the total number of reported crimes, or by calculating the proportion of offenders who reported that they were detected by the police when committing their *last* criminal offense.[98]

The latter procedure was applied in the *Second International Self-Report Delinquency Study* (ISRD-2), which contains what is probably the most extensive source of self-reported information on crime and detection. The ISRD-2 collected data from 12- to 15-year-old juveniles in 31 (mainly European) countries between 2006 and 2008. Enzmann's (2012) analysis of the ISRD-2 data indicates that police detection is a rare phenomenon, with juveniles typically reporting detection in only one out of ten offenses or even less frequently. More serious offenses with a victim had higher detection rates (e.g., assault, burglary, or theft of a car) than minor or "victimless" offenses (e.g., drug dealing, shoplifting, or vandalism). Other self-report research has reported similar or even lower detection rates that varied similarly across offenses (e.g., Erickson & Empey, 1963; Lochner, 2007; Wikström et al., 2012; Williams & Gold, 1972).[99]

5.2.2 | Overestimation of detection risk (by individuals inexperienced with crime)

In a second step, the experiential argument suggests that risk overestimation by individuals with no or little experience in violating the law is a major reason for the inverse relationship between criminal offending and risk perceptions. Researchers have often assumed that the inexperienced may overestimate the detection risk because they base their perceptions (mainly) on media-created stereotypes of criminals (e.g., Geerken & Gove, 1975; Matsueda et al., 2006; Paternoster, 2018). In the movies, on television, and in the news, criminals are usually caught and arrested. The media thus convey the impression that the legal system is more efficient in detecting crimes than it actually is. Although both individuals with no or little experience and experienced offenders are affected by this indirect information, the latter also have more direct information from their own personal experiences with criminal behavior. This personal knowledge allows them to assess the detection risk more accurately (Geerken & Gove, 1975). Those with no or little criminal experience must rely on more indirect information and thus tend to be surrounded by what Tittle (1980,

[98] The latter procedure works if this last offense can be treated as a sample of all offenses committed (Enzmann, 2012).
[99] Similar arrest rates were also calculated by other scholars who divided official arrest data by self-reported criminal behavior (e.g., Elliott, 1995; Nguyen & Reuter, 2012).

p. 67) coined a “shell of illusion,” which is a “perceptual system incorporating assumptions that terrible consequences will follow from violation of the rules.”

There are two strands of empirical research providing some insights into the assumption of risk overestimation. The first strand, lacking data on detections, has simply explored whether experienced offenders and individuals with no or little experience in offending differ in their risk perceptions. In line with the experiential argument, this research has consistently found that individuals with no or little criminal experience assess the detection risk as higher than (more) experienced offenders (e.g., Bishop, 1984; Paternoster et al., 1985; Saltzman et al., 1982). However, without calibrating the perceived risk with a detection rate, this literature cannot determine whether this correlation exists because the detection risk is overestimated by the inexperienced or because experienced offenders underestimate it.

The second strand of research has compared whether individual risk perceptions align with the actual detection rates in a given population. To our knowledge, only one of these so-called calibration studies compared risk perceptions with arrest rates that were calculated accurately based on self-report data (see section 5.2.1). In a study on adolescents and young adults from the USA, Lochner (2007) found that individuals substantially overestimated the arrest risk on average. In line with the experiential argument, non-offenders overestimated the risk to a higher degree than recent offenders. However, Lochner did not rely on “lifetime” offending data but only compared individuals who had and had not recently committed offenses. He therefore could not distinguish between individuals who had no or little experience in violating the law and individuals who had more prior experience at a point in the more distant past. The latter, however, should have already reduced their risk perceptions through their prior offending, according to the experiential argument. Lochner’s calibration study, hence, can be seen as a conservative test of the overestimation thesis.

Finally, a comparison of the low detection rates reported in Section 5.2.1 with risk perception estimates provided by other (external) studies (Anwar & Loughran, 2011; Matsueda et al., 2006; Schulz, 2014) further supports the overestimation thesis. Average estimates of the detection or arrest risk are typically much higher than the low police detection or arrest rates reported above, and most studies report particularly high risk estimates among individuals with no or little experience in violating the law.

5.2.3 | Decrease of risk perceptions due to (undetected) offending

The final and most crucial assumption of the experiential argument is that individuals with no or little criminal experience decrease their risk perceptions when they begin committing crimes, as they learn through undetected offending that the detection risk is lower than first feared. This argument aligns with Stafford and Warr's (1993) hypothesis that experiences of punishment avoidance should generally lead to a lowering of risk perceptions, as "successful" (undetected) offenders learn through experience that detection is relatively unlikely. It is also consistent with Bayesian updating models (e.g., Anwar & Loughran, 2011; Lochner, 2007; Matsueda et al., 2006) that consider detection (avoidance) experiences as signals that people process to update their initial (i.e., prior) risk perception. According to these models, detection avoidance should lead to a decreased subsequent (i.e., posterior) risk perception compared to one's prior risk perception.

Furthermore, one strand of the experiential literature discussed whether the *"novelty" of the criminal behavior* or the *perceptual "naiveté" of individuals* is the more critical facilitator of the experiential effect (see Minor & Harry, 1982; Paternoster et al., 1985). The term *novelty effect* suggests that the downgrading of risk perceptions due to criminal offending happens mainly (or is especially strong) among those with no or little previous involvement in criminal behavior. For them, involvement in illegal activity is a new experience, providing novel information that should lead to a more substantial alteration of risk perceptions than committing one more in a long series of offenses. The term *naiveté effect*, in contrast, refers to the importance of the level of prior risk perceptions for the updating process. It assumes that only (or mainly) "naïve" individuals, i.e., those who have high (overestimated) risk estimates prior to committing crimes, lower their risk perceptions substantially after violating the law. Individuals who already possess low and thus more accurate risk estimates are in little need of adjusting their perceptions to more realistic levels.

Many perceptual panel studies have not adequately explored the risk perception changes proposed in the experiential argument. This is because they utilized their data in ways that were not designed to explore effects *within* individuals over time but rather estimated differences in risk perceptions *between* individuals (e.g., Bishop, 1984; Minor & Harry, 1982; Piliavin et al., 1986; Saltzman et al., 1982; Seddig et al., 2017). The few studies that have concentrated on intra-individual changes were typically hampered by including information on criminal offending only over the last few months or years (e.g., Hirtenlehner & Wikström, 2017; Lochner, 2007; van Veen & Sattler, 2018). These studies consistently found that the more people had been involved in recent deviant or criminal activity, the lower their subsequent risk perceptions were. However, due to the lack of "lifetime" offending information, these studies could not accurately examine the updating process

outlined in the experiential argument, which begins with the assumption of risk overestimation by those with *no (or little) criminal experience* (and not just by those with *no recent criminal experience*).

So far, Schulz (2014) is the only researcher to have used fixed effects models to calculate within estimates to analyze how risk perceptions of individuals with no previous criminal experience change when they begin committing crimes. In a subsample of British adolescents and young adults with no prior criminal experience, she found evidence of the proposed updating process. The individuals who started committing crimes during the period under examination lowered their risk perceptions. However, a weakness of her analysis is her categorization of the criminal offending variable. She categorized periods in which a person stopped committing crimes (after having initially started) in the same category with periods in which they committed a small number of crimes (fewer than three). This procedure prevented her from exploring whether individuals revert to prior risk perception levels when they stop offending and whether renewed criminal offending after a temporary cessation of criminal activity has similar downgrading effects. Such a finding could challenge the assumption that the novelty of criminal behavior is a primary source for the updating process.

Two other studies have utilized residual change score models to explore whether novelty or naiveté effects are the primary driver of the proposed experiential learning process. While the findings of Paternoster et al. (1985) were relatively inconsistent, the analysis of Pogarsky et al. (2004) based on US high school students produced more clear-cut results. It showed more support for the naiveté than for the novelty effect (see also Minor & Harry, 1982). After committing crimes, a substantial lowering of risk perceptions was only found among individuals with high prior risk perceptions and not among those with lower risk estimates. Changes in risk perceptions due to offending, in contrast, did not differ significantly between individuals with no, moderate, or extensive previous criminal experience.

5.2.4 | Current study and hypotheses

The current study revisits the experiential argument by investigating its three major underlying assumptions. Following on the research of Seddig et al. (2017), the current study uses data of a general sample of German juveniles to supplement the relatively scarce research outside of the United States that has explored processes of experiential learning (for notable exceptions, see Hirtenlehner & Wikström, 2017; Schulz, 2014; Seddig et al., 2017; van Veen & Sattler, 2018). In

particular, the current study investigates the following hypotheses to explore the validity of the experiential argument:

H1: The risk of being detected by the police is low for most criminal offenses.

H2: Individuals with no (or little) criminal experience overestimate the detection risk, whereas individuals with (more) criminal experience do not or do so to a lesser extent.

H3: When individuals begin committing crimes, they subsequently reduce their risk perceptions on average because they typically observe that the detection risk is much lower than first feared.

H3a: The less criminal experience individuals have before committing crimes, the more their risk perceptions are reduced due to criminal offending (novelty effect).

H3b: The higher the individuals estimate the detection risk before committing crimes, the more their risk perceptions are reduced due to criminal offending (naiveté effect).

5.3 | Methods

5.3.1 | Data

The current study relies on data from the study Crime in the modern City (CrimoC; Boers et al., 2010; Seddig & Reinecke, 2017). The initial survey started in 2002 with 3,411 seventh graders at secondary schools in Duisburg, a town with a population of approximately 500,000 in the western part of Germany. Eight annual panel waves were conducted between 2002 and 2009, covering the period from early to late adolescence. Five additional biannual panel waves were conducted between 2011 and 2019 to cover the period from late adolescence to young adulthood. The students' ages ranged between approximately 13 and 30 years. Self-administered questionnaires were completed in the classroom up to the ninth grade. After leaving secondary school, participants were usually contacted by mail. If repeated attempts were unsuccessful, personal contacts were realized to conduct the interviews. Retention rates were between 82 and 91%.[100]

Four panel waves (2003 to 2006) covering the adolescents' age range were used for the analyses presented here. Only participants who fulfilled particular conditions were considered. First, we selected only juveniles who participated in at least two of the four panel waves. Second, we kept

[100] Details of the CrimoC study can be found at www.crimoc.org.

only observations with complete information on all the variables used for our analyses. Due to these conditions, our final sample includes 9,362 observations from 3,259 respondents.[101]

5.3.2 | Measures

Perceptions of detection risk. As in Seddig et al. (2017), the measurement of our dependent variable, detection risk perceptions, is based on the respondents' assessments of their likelihood of being caught when committing four different types of criminal offenses (assault, burglary, shoplifting, and vandalism). Response categories were (0) very unlikely, (1) unlikely, (2) neither/nor, (3) likely, and (4) very likely. We constructed a *general risk perception* score by taking the mean across all four offense-specific perceptions (range: 0-4).

Self-reported criminal offenses. Our measure of criminal offending is also based on the items used in Seddig et al. (2017). It considers the juveniles' self-reported frequency of committing fifteen different criminal offenses over the last year. The crimes at hand were assault (no weapon), assault (with a weapon), bag-snatching, bicycle theft, burglary, fencing stolen goods, robbery, shoplifting, scratching, theft of a car, theft from a car, theft from a vending machine, theft (other), vandalism (graffiti), and vandalism (other). The reported frequencies of the various crime types were added to construct a *recent total criminal offending* score for each individual.

Self-reported police detections. Detection information is based on the juveniles' reports of how many of the crimes they reported in the last year the police were aware of (for all 15 offense types mentioned above). By adding together the number of reported detections for each offense type, we constructed a score of *recent total police detections* for each individual. We relied on self-reports because the fact that people remembered having committed offenses and being detected should be more relevant for perceptual updating processes than the fact that they "objectively" committed crimes and were detected according to official sources.

Detection rates. The calculation of detection rates is necessary to assess the first two assumptions of the experiential argument. As a reminder, a detection rate is computed by dividing the total number of detected crimes by the total number of crimes committed in a given population. For each of the 15 offense types, we calculated a detection rate by dividing the number of police

[101] The first wave was not included because it lacked risk perception measures. Panel attrition led to some differences in variable distributions: more female participants, somewhat fewer respondents from lower secondary schools and more from upper secondary schools in the panel data compared with the cross-sectional data (Kleinke et al., 2020; Reinecke & Weins, 2013).

detections (per offense) by the number of crimes (per offense) reported by the sample as a whole. Additionally, we computed a *total detection rate* encompassing all types of offenses by dividing all reported police detections by all reported crimes.

Criminal experience. Our criminal experience measure, used as the primary independent variable in our updating analysis, combines data on recent criminal offending and criminal history. First, we categorized the *recent total criminal offending* score, which is highly skewed to the right, to diminish the effects of outliers (for a similar approach, see Matsueda et al., 2006; Schulz, 2014). The generated categories were the following: 0 crimes, 1-2 crimes, 3-9 crimes, and 10 or more crimes. To revise these categories further, we also considered juveniles' reports on whether they had ever committed any of the fifteen crimes to derive whether they really had no criminal experience (and not just no recent experience). Integrating this information, the *criminal experience* measure consists of the following categories: (0) never committed a crime, (1) committed a crime, but not in the past year, (2) 1-2 crimes in the past year, (3) 3-9 crimes in the past year, (4) 10 or more crimes in the past year.[102]

Covariates. The covariate selection reflects the importance of vicarious experiences with punishment (avoidance) for individual risk perceptions (Stafford & Warr, 1993). As relevant sources of vicarious information, we selected deviant peer exposure, perceived neighborhood disorder, and time spent watching crime movies as key covariates. All these are discussed as factors that may present indirect sources of information about the risk of detection or punishment (e.g., Cook, 1980; Geerken & Gove, 1975; J. Q. Wilson & Kelling, 1982). *Exposure to a deviant peer group* is an ordinal variable with the following categories: (0) spending no or little time with a peer group, (1) spending much time in low-deviant peer group activities, (2) spending much time in medium-deviant peer group activities, and (3) spending much time in high-deviant peer group activities. *Perceived neighborhood disorder* is a continuous variable ranging from -2 to 2, with larger values indicating more perceived disorder. To assess the *time spent watching crime movies*, we

[102] We recoded a substantial number of cases in which individuals reported in later panel waves that they had never committed any crimes, but had admitted offending in earlier waves to be in the category "committed a crime, but not in the past year." In a sensitivity analysis, we kept these inconsistencies, acknowledging that individuals may forget crimes committed further back in time. The results of this analysis resemble those presented in the current article (see online supplementary material). Additionally, we constructed another criminal experience variable considering personal police detections. However, as detection is a rare phenomenon in the given sample, categories including detection information were small and thus estimation uncertainty too high to be informative. Besides having low power, this alternative specification produced similar results (see online supplementary material).

considered respondents' reports on whether they watched crime movies (0) never, (1) rarely, (2) sometimes, (3) often, or (4) very often.[103]

5.3.3 | Analytical procedure

The first analytical step assesses whether the detection rates in the given sample are low. To do this, we report offense-specific and total detection rates. We use the panel data in a pooled way to construct these rates, as we want to give an overview of the actual police detection risk over the whole period of adolescence (and not for a specific panel wave).

The second analytical step explores whether individuals who have never committed a crime overestimate the risk of detection. To do this, we compare whether individuals with no criminal experience have higher risk perceptions than those with criminal experience. Unfortunately, our risk perception measures cannot be reliably transformed to a probability scale. Thus, we refrain from formally testing the difference between the detection rates and risk perceptions. We, instead, use descriptive statistics to explore whether individuals with no criminal experience overestimate the detection risk and whether they do so to a larger extent than individuals with criminal experience.[104]

Finally, the third analytical step investigates whether individuals reduce their risk perceptions when they learn through "successful" (undetected) offending that the detection risk is lower than first feared. To analyze this updating process, we rely on fixed effects models (Allison, 2009). These models adjust for all of the respondents' observed and unobserved time-stable characteristics. By investigating *within*-person changes (rather than *differences* between respondents), fixed effects models allow us to examine how, for example, changes in criminal experiences among respondents are associated with changes in their perceived detection risks. To study whether the criminal experiences lead to the risk perception updating outlined in the experiential argument, we utilize the *criminal experience* measure as an independent variable and the *general risk perception*

[103] For the specific items that were used to calculate all variables included in the fixed effects models, see Table 5.4 in the Appendix.

[104] In an additional analysis, we transformed the risk perception values into POMP scores. These scores represent the percentage of maximum possible values and, as percentages, could be compared much more directly with the detection rates. However, we think that such comparison may give the false impression that we can analytically assess the alignment of our (ordinal-scaled) risk perception measures with the detection rates in a proper way. To prevent such an impression, we present the POMP scores only in the online supplementary material.

score as the dependent variable. In a second step, we also include other covariates to account for potential confounding.[105] Our fixed effects models have the following structure:

$$(Y_{it} - \overline{Y_i}) = (X_{it} - \overline{X_i}) + (\varepsilon_{it} - \overline{\varepsilon_i})$$

In the analysis, differences to the within-person mean of perceived detection risk $\overline{Y_i}$ at time point t are regressed on differences to the within-person mean in the covariates $\overline{X_i}$. Since unobserved heterogeneity among individuals is completely eliminated by the reduction to intra-individual changes, the risk of over- or underestimating effects as well as causal misinterpretations is significantly reduced in fixed effects models compared to competing methods. However, this advantage is countered by relatively large standard errors and thus less efficient estimates, which are caused by the lack of consideration of time-constant variables in fixed effects models (Allison, 2009). From our point of view, however, it is crucial to report results that have a lower risk of being biased by unobserved factors and more closely mirror the proposed experiential learning process that unfolds over time *within* individuals.

5.4 | Results

5.4.1 | Low detection risk

The calculated detection rates support the assumption that the detection risk is low for most criminal offenses (see Table 5.1). Overall, the juveniles reported that the police detected only 2.2% (795) of their 36,484 crimes.[106] This very low estimate is generally consistent with previous research, although some studies have reported somewhat higher detection rates (see Enzmann, 2012; Erickson & Empey, 1963; Lochner, 2007; Wikström et al., 2012; Williams & Gold, 1972). The ranking of the detection rates by offense types also aligns with previous research: More serious offenses that include a victim have higher detection rates (e.g., assault with a weapon: 6.6%; theft of a car: 6.3%) than relatively minor or "victimless" offenses (e.g., bicycle theft: 0.9%; shoplifting: 2.3%; theft from a vending machine: 2.2%; vandalism (graffiti): 1.2%).

[105] To pick up potential year shocks resulting from underlying unobservable systematic differences between observed time units (period effects), all fixed effects models also control for the current panel wave.

[106] The detection rates were computed for observations with complete information on all variables included in the updating analysis. If we loosen this condition and also use information from observations with missing data, the overall detection risk increases to 3.0% (see online supplementary material).

Table 5.1: Total and offense-specific detection rates

Offense	Police detections	Crimes	Detection rate
Bag-snatching	0	106	0.0%
Theft (other)	5	723	0.7%
Bicycle theft	9	1,047	0.9%
Fencing stolen goods	24	2,240	1.1%
Vandalism (graffiti)	89	7,379	1.2%
Vandalism (other)	65	4,845	1.3%
Robbery	15	1,076	1.4%
Theft from a car	4	252	1.6%
Theft from a vending machine	10	452	2.2%
Shoplifting	148	6,560	2.3%
Assault (no weapon)	165	5,393	3.1%
Burglary	20	552	3.6%
Scratching	194	5,137	3.8%
Theft of a car	13	208	6.3%
Assault (with weapon)	34	514	6.6%
Total	795	36,484	2.2%

5.4.2 | Overestimation of detection risk (by individuals inexperienced with crime)

In this subsection, we explore whether the respondents (with no criminal experience) estimated the detection risk accurately or if they overestimated it. Risk estimates were only collected and thus reported for a subset of criminal offenses. As a reminder, risk perception scores range from 0 to 4. To assess the detection risk accurately as (very) low, individuals should have scores between 0 (very unlikely) and 1 (unlikely).

Table 5.2: Descriptive statistics of general and offense-specific risk perceptions

Risk perceptions by offense	Full sample		No crim. experience		Some crim. experience		M_1-M_2
	M	SD	M_1	SD	M_2	SD	[SE]
Vandalism (graffiti)	1.58	1.42	1.65	1.42	1.21	1.32	0.44 [0.04]
Assault (no weapon)	1.97	1.32	2.10	1.30	1.65	1.30	0.45 [0.03]
Shoplifting	2.29	1.31	2.39	1.32	2.14	1.28	0.26 [0.03]
Burglary	2.91	1.29	2.93	1.29	2.66	1.31	0.27 [0.06]
Total	2.18	0.99	2.35	1.07	2.08	0.92	0.27 [0.03]

Notes: The number of person-observations of those with no and some criminal (crim.) experience differs across offense types and the numbers reported in the following apply to the total offenses: n_{Obs} (full sample) = 9,362; n_{Obs} (no criminal experience) = 3,646; n_{Obs} (some criminal experience) = 5,716; Mean (M) differences and their cluster-robust standard errors were calculated with regression models (all corresponding p values < 0.001).

The results show that the juveniles tended to overestimate the detection risk but ranked the risk for different crime types in roughly correct order (see Table 5.2, column 2). The mean of the general risk perception score is 2.18 for the full sample, indicating that juveniles, on average, perceived the general detection risk as neither likely nor unlikely (=2). This risk assessment does not align

with the actual detection rates, according to which the detection risk was very low. Thus, the juveniles overestimated the risk of detection on average. However, they were relatively accurate in ordering the detection risk across different kinds of criminal offenses. In line with the actual detection rates (see Table 5.1), they assessed the risk of detection for vandalism (mean = 1.58) as substantially more unlikely than for burglary (mean = 2.91). The risks for shoplifting and assault were somewhere in between.

However, to empirically investigate the experiential argument, we must inspect the risk perceptions for individuals with no (or little) criminal experience who are expected to be particularly prone to overestimation compared to their counterparts with (more) criminal experience. The calculated risk perceptions support this assumption. On average, the inexperienced individuals perceived the detection risk to be higher than individuals who had reported at least one previous criminal offense (see Table 5.2, columns 4 and 6). This overestimation is true for all offense-specific risk estimates and the general risk perception score, with a mean of 2.35 among the inexperienced and only 2.08 among the experienced. However, even if most of the inexperienced overestimated the detection risk (73% with values of at least 2; 34% with values of at least 3), a substantial portion had much lower (and more realistic) risk perceptions (11% with values between 0 and 1). This considerable risk perception variation is also reflected in the relatively large standard deviation of 1.07 (offense-specific perceptions vary even more; see Table 5.2, column 5). Among those who had been involved in criminal activities before, the variation is only somewhat smaller and only for some offenses (see Table 5.2, column 7). This lower risk perception variation is mainly because experienced offenders much less often assessed the general detection risk to be very high (only 19% with values of at least 3).

5.4.3 | Decrease of risk perceptions due to (undetected) offending

We finally present the results of various fixed effects models to assess the main experiential argument that individuals (with no criminal experience) decrease their risk perceptions when they start committing crimes (see Table 5.3).[107] Overall, these models support the outlined updating process.

Model 1 includes only individual criminal experience as an independent variable. It suggests that the more recent illegal activity people are involved in, the more they reduce their risk perceptions

[107] In addition to the models presented, we also computed a null model to explore how much variation in general risk perceptions is within and between individuals. Derived from this null model, the intraclass correlation (ICC) of 0.31 suggests that risk perceptions vary substantially between and within individuals.

relative to periods before having ever committed a crime (i.e., the reference category). This inverse relationship between criminal involvement and risk perceptions followed a monotonic pattern: More offenses were associated with lower risk perceptions (e.g., >=10 recent crimes: β = -0.37 [-0.49 -0.25]). The model, furthermore, indicates that even if a person had not recently (i.e., in the last 12 months) committed a crime but did so at some point in the past, they may still had a slightly reduced risk perception (no recent crime: β = -0.07 [-0.16 0.03]). However, the estimate is too uncertain to claim whether such a small "sustained" risk perception decrease holds in the population.

Table 5.3: Modelling the updating process: Changes in general risk perceptions

	Model 1			Model 2			Model 3		
Predictor	**β**	**95%-CI**		**β**	**95%-CI**		**β**	**95%-CI**	
Criminal experience (CrEx)									
(ref.: never crime)									
No recent crime	-0.07	[-0.16	0.03]	0.02	[-0.08	0.12]	-0.01	[-0.12	0.10]
1-2 recent crimes	-0.13	[-0.24	-0.01]	-0.06	[-0.17	0.06]	-0.47	[-0.61	-0.34]
3-9 recent crimes	-0.20	[-0.32	-0.09]	-0.13	[-0.25	-0.01]	-0.51	[-0.67	-0.36]
≥ 10 recent crimes	-0.37	[-0.49	-0.25]	-0.26	[-0.39	-0.13]	-0.69	[-0.86	-0.52]
CrEx X Perceived risk in times without offending									
(ref.: never crime)									
No rec. crime X Low risk							0.09	[-0.11	0.30]
1-2 rec. crimes X Low risk							1.00	[0.77	1.23]
3-9 rec. crimes X Low risk							0.87	[0.62	1.12]
≥ 10 rec. crimes X Low risk							0.89	[0.63	1.15]
Peer group exposure									
(ref.: no or little exposure)									
Low-deviant				-0.04	[-0.09	0.02]	-0.04	[-0.09	0.01]
Medium-deviant				-0.18	[-0.25	-0.10]	-0.19	[-0.27	-0.11]
High-deviant				-0.26	[-0.38	-0.14]	-0.26	[-0.40	-0.12]
Neighborhood disorder				0.02	[-0.02	0.07]	0.05	[0.00	0.09]
Watch crime movies									
(ref.: never)									
Rarely				0.01	[-0.06	0.09]	0.01	[-0.07	0.09]
Sometimes				0.05	[-0.04	0.13]	0.05	[-0.04	0.13]
Often				0.08	[-0.01	0.18]	0.07	[-0.03	0.17]
Very often				0.14	[0.02	0.26]	0.13	[0.00	0.25]
Panel wave									
(ref.: 2003)									
2004				-0.16	[-0.22	-0.11]	-0.17	[-0.23	-0.12]
2005				-0.07	[-0.13	-0.02]	-0.08	[-0.14	-0.02]
2006				-0.16	[-0.22	-0.10]	-0.17	[-0.23	-0.10]
Constant	2.27	[2.21	2.33]	2.33	[2.24	2.43]	2.36	[2.27	2.45]
Persons		3,259			3,259			2,858	
Observations		9,362			9,362			8,497	

Notes: Unstandardized regression coefficients of fixed effects models with cluster-robust confidence intervals (CI).

The idea of a sustained risk perception decrease is also called into question by the results of Model 2, which includes the other covariates besides personal criminal experience. Criminal involvement has a more modest effect on risk perceptions in this model specification than in the first. Only if individuals committed at least a minimal number of offenses, did they have substantially reduced risk perceptions (3-9 recent crimes: β = -0.13 [-0.25 -0.01]; >=10 recent crimes: β = -0.26 [-0.39 -0.13]) compared with a period in which they had no criminal experience. Furthermore, the same person's risk perceptions do not seem to differ much depending on whether they had never committed a crime or whether they had, but not in the past 12 months (no recent crime: β = 0.02 [-0.08 0.12]). This finding suggests that individuals who decrease their risk perceptions directly after committing (many) crimes seem to return to their initial risk perception level when the crime experiences recede further into the past. When they then commit crimes again, they reduce their risk perceptions to a similar degree as before. This latter finding poses a challenge to the novelty effect assumption that it is especially first-time criminal experience that leads to risk perception updating.

The other covariates included in the model are only partly related to the risk perceptions as hypothesized in the literature. First, in line with vicarious learning (Cook, 1980; Stafford & Warr, 1993), the more an individual was exposed to a deviant peer group, the lower their risk perceptions. Second, and at odds with the suggestion of broken windows theory that neighborhood decay may signal a low detection risk (J. Q. Wilson & Kelling, 1982), perceived neighborhood disorder had no substantial effects on individual risk assessments. Third, and as expected (Geerken & Gove, 1975; Matsueda et al., 2006), frequent viewing of crime movies was associated with increased risk perceptions.

Finally, we present the results of Model 3, which has the same specification as Model 2 but also includes an additional interaction term between a newly generated variable and criminal experience to analyze the naiveté effect. The new variable is time-invariant and binary and could be coined "risk perception level in times of non-offending." It distinguishes between individuals who had, on average, risk perceptions between 0 and 2 in periods in which they committed no crimes and individuals who had values between 2 and 4. The estimates for the interaction term are significant for all criminal experience categories except for the category "no recent crime" (see Table 5.3). This result suggests that the two groups differ substantially in how they update their risk perceptions when they commit criminal offenses but that both groups return to prior risk estimate levels when they stop committing crimes (see Figure 5.1). Whereas those with low risk perceptions (in times without offending) do not change or instead increase their risk perceptions when they become involved in criminal activity, those with high risk perceptions do the opposite. They

substantially reduce their risk estimates, and they reduce them the most when they commit a large number of crimes (>= 10 recent crimes: β = -0.69 [-0.86 -0.52]). This finding of differential updating conditional on "initial" risk perceptions (i.e., the average risk perception level in times of non-offending) lends some support to the naiveté effect.

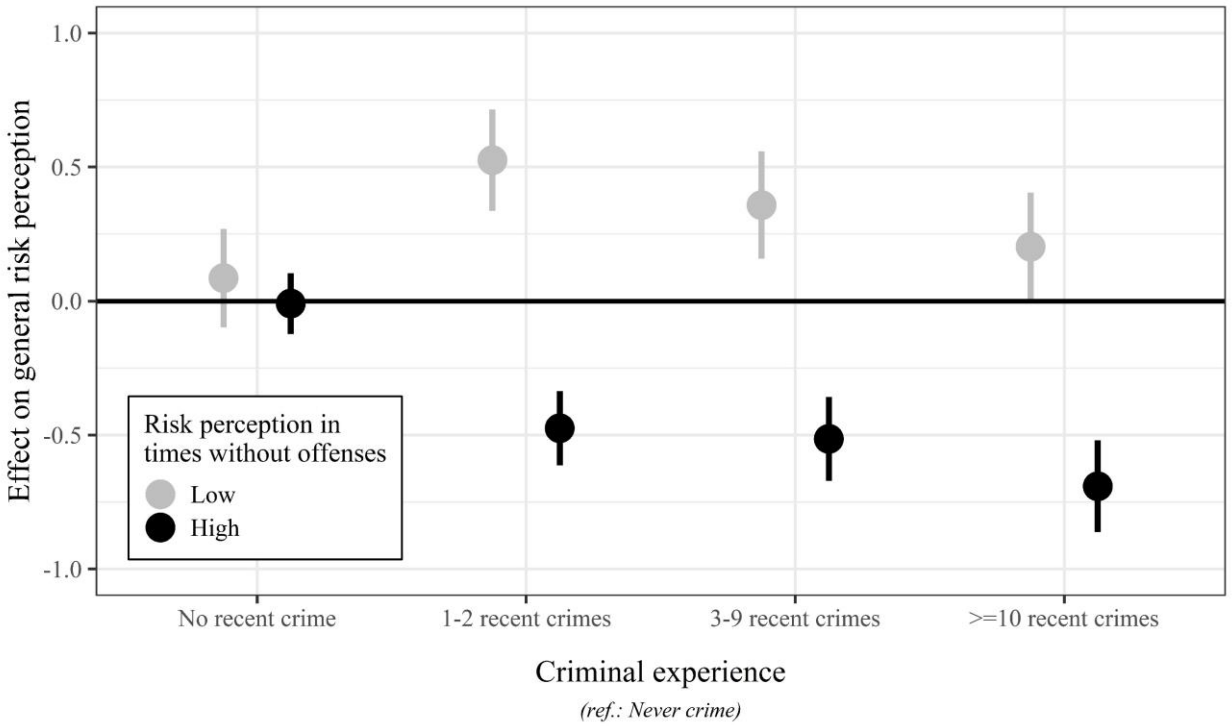

Figure 5.1: Differential updating contingent on the "initial" risk perception level
Notes: N_{Pers}(Low "initial" risk perception) = 1,130; N_{Pers}(High "initial" risk perception) = 1,728; Results represent unstandardized regression coefficients of a fixed effects model (see Table 5.3, Model 3) with cluster-robust confidence intervals.

5.5 | Discussion

The current study assessed the validity of the experiential argument by exploring its three underlying assumptions. Overall, our evaluation strengthens the experiential argument but also poses some intriguing challenges. The following finding supports the first hypothesis (H1): The total detection rate in the given sample was very low, with only 2.2% of all crimes detected by the police. Offense-specific detection rates, furthermore, were all below 10%. These detection rates align with previous research, although tending toward the lower bound of the previously reported rates (Enzmann, 2012; Erickson & Empey, 1963; Lochner, 2007; Wikström et al., 2012; Williams & Gold, 1972). Overall, current and previous findings support the assumption of a low risk of police detection for criminal offending.

The second assumption of the experiential argument was only partially supported by the current study. In line with Hypothesis H2, the findings indicate that individuals overestimate the detection risk on average and that those without criminal experience do so to a greater extent than those who possess some criminal experience. Offense-specific perceptions furthermore suggest that individuals rank the detection risk for various crime types roughly in the correct order. Both results correspond to findings reported by Lochner (2007). K. J. Thomas et al. (2018, p. 81) produced supporting evidence that "individuals are locally coherent in the rank order of arrest risk among different crime types." The finding of risk overestimation is further corroborated by comparing the low detection rates with the high risk perceptions consistently reported in other (external) studies (e.g., Matsueda et al., 2006; Schulz, 2014). However, although our analysis seems to support the overestimation thesis, additional explorations highlight the extensive variation in risk perceptions. Most strikingly, a substantial portion of juveniles without criminal experience estimated the detection risk accurately as being (very) low. These individuals hardly fit the experiential argument since they do not overestimate the detection risk and hence cannot downgrade their risk perceptions any further by starting to commit undetected crimes.

Finally, the third assumption of the experiential argument, that individuals without (much) criminal experience lower their risk perceptions when they start committing crimes, was also partly supported by the current study. With our fixed effect models, we analyzed how risk perceptions changed depending on intra-individual changes in criminal offending. In line with Schulz's (2014) findings, the detection risk was assessed as substantially lower when an individual had recently been involved in repeated criminal activity than when the same individual had never committed any crime before. This finding is generally supportive of the updating process outlined in the experiential argument (see H3).

However, the current study also produced a surprising finding not observed in prior research: Risk perceptions did not differ substantially over time depending on whether a person had never committed a crime before or whether they had, but not in the past 12 months. This finding indicates that after individuals reduce their risk perceptions to more realistic levels, they do not stabilize their estimates but instead fall back to their initial overestimated levels when they stop offending. When they start to commit crimes again, they again reduce their risk perceptions to a similar degree. This finding poses some challenge to the experiential argument and especially to the assumption of the novelty effect that perceptions that were previously influenced by relevant information should be less malleable in the future (see H3a).

There has been little other research to date showing that people may return to overestimation (or into the "shell of illusion") when they stop committing crimes (however, see Lochner, 2007; Paternoster et al., 1985). One exception is Lochner (2007, p. 455), who interpreted the results of one of his model specifications to suggest that there is "little persistence in the effects of new information on reported beliefs." He noted that individuals seem to have baseline risk beliefs to which they revert back to in times in which they gather no or little direct information. His finding aligns with our results and may be explained in the following ways: First, individuals may have short memories of (unfulfilled) risks (e.g., Lochner, 2007; Pogarsky et al., 2004). Criminal experiences without harmful consequences (e.g., detection) may thus fade from memory when they recede into the past. This forgetting may be especially relevant for illegal behavior since (most) individuals may want to suppress memories of immoral activity to preserve a positive, coherent self-concept (Fawcett & Hulbert, 2020).[108] Second, more recent information, even if indirect, proves to be more important for risk estimation than more distant information, even it is more direct (e.g., Lochner, 2007; Pogarsky et al., 2004). From this perspective, a return to higher risk perceptions may be explained by the "overwriting" of old personal information with newer (less accurate) vicarious information (e.g., provided by the media).[109] Finally, perceptions of one's detection risk may be grounded in one's self-confidence in committing crimes without being apprehended (Loughran et al., 2013). The more frequently an individual commits crimes, the more confident they may become that they can avoid detection and punishment. When individuals then stop committing crimes, this self-confidence may wane over time as they lack recent experiences that indicate that they can still avoid detection.

Whereas the latter finding sheds doubt on the novelty effect, our study strengthens the naiveté effect. It shows that individuals update differently when they commit crimes depending on their initial risk perception level in times without criminal offenses. In line with the naiveté effect (see H3b), the lowering of risk perceptions due to criminal offending can be only observed among those who had relatively high risk perceptions in times in which they did not commit a crime. This finding is consistent with the few other studies that also found evidence for the naiveté effect while providing little or at best inconsistent support for the novelty effect (e.g., Minor & Harry, 1982; Pogarsky et al., 2004). However, in the current study, it is striking that individuals who had low

[108] The thesis of forgetting seems to be supported by the substantial number of juveniles who reported criminal offenses in previous waves but reported in later waves that they had never committed any of the crimes at hand.
[109] Comparing Model 1 and 2 lends some evidence supporting this explanation. Through the inclusion of covariates (see Model 2), the difference in risk perceptions between the reference category "never crime" and the category "no recent crime" disappears. Bringing in vicarious information, the covariates thus may explain part of the "bouncing-back" process when a person stops committing crime.

initial risk perceptions (in times without criminal offenses) even increased these perceptions after committing crimes. In their early study on this topic, Paternoster et al. (1985, p. 419) considered these kinds of "re-equilibrating" effects in times without criminal offenses but offered no decisive explanation for them. It seems reasonable to assume, however, that vicarious information may also play a role in these upgrading processes. Individuals who generally estimate the risk of detection as relatively low may be affected in their lives by continuous vicarious experiences that provide a realistic view of detection risk. When they become involved in criminal activities, this indirect information is typically confirmed. Still, they may fear at some point that their "streak of luck" (i.e., committing crimes without being apprehended) may come to an end. This feeling of being due to be caught may lead them to adjust their risk perceptions upward. Such a "resetting" of risk perceptions was already hypothesized for punished offenders in the opposite direction (i.e., they adjust their risk perceptions downward after being punished, see Pogarsky & Piquero, 2003).

5.5.1 | Limitations

Although providing essential support and challenges to the experiential argument, the findings presented above have to be seen in the light of three notable limitations of the current study. First, although we call our measures "perceptions" in line with previous research, Wikström (2008) rightly noted that such measures actually reflect relatively abstract risk assessments: Perceptions are situational and cannot be measured by such contextless risk evaluations. However, Wikström also acknowledges that general risk assessments should be related to perceived risks and should therefore give at least some insights into how respondents perceive the detection risk in real-life circumstances. Additionally, our general risk assessments are not collected in ways that are easily scalable to align with the detection rates. Thus, we abstained from analytically comparing both measures. Future research should replicate our findings with probability-scaled risk assessment measures (for such measures, see Lochner, 2007; Schulz, 2014).[110] This research should also include perceptual measures that specifically refer to the perceived risk of detection by the *police*.

[110] We agree with Apel (2013, p. 94) that "probabilistic measures of risk perceptions […] seem to be the most desirable relative to other response formats" (e.g., because they allow calibrating risk perceptions with detection rates). We, however, do not think that our results would change much if we had applied such measures instead. There are two reasons for our assessment: First, respondents seem to think about detection risk verbally and not in fine-grained numeric terms, limiting the additional value of probability scales (Roche et al., 2020). Second, existing studies with numerical or probability-scaled measures produced results in line with our main findings: (1) individuals overestimate the detection risk (Lochner, 2007); (2) Individuals with criminal experience assess the likelihood of detection as higher than those inexperienced with crime (e.g., Paternoster et al., 1985; Schulz, 2014); (3) Individuals lower their detection risk estimates after they start committing crimes (Schulz, 2014).

Otherwise, this research is hampered, like ours, by calibrating perceived risks of *general* detection with *police* detection rates.

Second, although the fixed effects models used in the current study have distinct advantages over other methods used in previous perceptual updating research, they are not a panacea in terms of the temporal ordering of the intra-individual changes. Even if our models are specified such that changes in criminal activity influence changes in risk perceptions, they are not able to actually ensure that the influence operates (exclusively) in the specified direction. Instead, our results might also (partially) reflect a (deterrence) influence of risk perceptions on criminal activity. However, two aspects increase our confidence that our results can be interpreted as experiential effects rather than deterrence effects. On the one hand, previous research that tried to disentangle the two effects typically found much more substantial experiential effects (e.g., Hirtenlehner & Wikström, 2017; Saltzman et al., 1982; Seddig et al., 2017). Thus, even if our results represent a mixture of both effects, experiential effects should be the primary contributor. On the other hand, the reference periods of our two key measures are consistent with our interpretation: While data on criminal activity were collected retrospectively (offenses in the past 12 months), data on risk perceptions referred to the time of data collection. Assuming that respondents can adequately process temporal cues in the questionnaires, our results should reflect pure experiential effects.

Third, our fixed effect models explain only a small portion of the intra-individual variation in risk perceptions (up to 5.4%). Such lack of explanatory power is true for most previous risk updating studies and was highlighted as a "dirty little secret in deterrence research" (Paternoster, 2010, p. 808). Confronted with the fact that personal and vicarious experiences with punishment (avoidance) explain only a small portion of the risk perception variation, the deterrence literature offers two pathways to develop more powerful explanatory models. On the one hand, research should consider situational determinants of risk perceptions (Apel, 2013). Recent research suggests that heuristics such as anchoring or availability may play a prominent role in forming risk perceptions (Pogarsky et al., 2017) and thus should be considered alongside experiential or vicarious learning processes. On the other hand, literature on differential deterrability highlights that updating and deterrence processes may vary across situations and persons. For updating, this literature has provided some first evidence that the strength of updating differs across individuals who vary in their previous criminal involvement, self-control abilities, and personal morals (e.g., Anwar & Loughran, 2011; Pogarsky et al., 2005; Schulz, 2014). Future research should follow these two promising paths to develop more sophisticated models of the formation and change of risk perceptions.

5.5.2 | Policy implications

Despite these limitations, the current study seems to offer some critical implications for criminal justice policy. It indicates that a low risk of police detection is responsible for juveniles lowering their detection risk assessments when they start committing crimes. According to deterrence theory, such a decrease in risk perceptions is startling because it should lead to more future criminal offending by those whose crimes go undetected (and those who witnessed their impunity). Confronted with similar results, Matsueda et al. (2006, p. 117) concluded that this finding "underscores the importance of early interventions, occurring before delinquent careers develop and risk estimates decline." Rather than calling for criminal justice agents to implement measures dramatically increasing the risk of detection, however, Matsueda and colleagues proposed the establishment of early educational programs emphasizing the long-run risk of detection in criminal careers. We agree with their proposal and their assessment that disproportionate measures would be required to increase the police detection rate substantially.

Furthermore, two aspects may reassure policymakers and legal authorities that the lowering of risk perceptions may be less detrimental than it seems at first glance and that strong reactions would be unwarranted. First, if it is true that only more recent criminal experiences are responsible for the formation of risk perceptions, this is positive news. When former offenders temporarily or permanently stop committing crimes, they will typically return to their prior, mostly overestimated, risk assessment levels. This return to overestimation (or into the "shell of illusion") may in turn hinder their involvement in future illegal activity. Second, according to perceptual deterrence theory, deterrence consists of two processes. Seen from this broader perspective, the lowering of risk perceptions (perceptual linkage) may be less dramatic because the evidence for the behavioral linkage is relatively modest. Many perceptual studies found no or only relatively modest effects of risk perceptions on criminal offending (for reviews, see Nagin, 1998; Paternoster, 2018; Wikström, 2008). However, if risk perceptions have relatively little influence on future delinquency, then their change through prior undetected offending should lead to relatively low increases in criminal offending.

5.5.3 | Conclusion

The current study produced several findings that support the experiential argument: First, juveniles are rarely detected by the police when committing crimes. Second, juveniles overestimate the risk

of detection on average, and individuals with no criminal experience overestimate this risk to a larger extent. Third, when juveniles start committing (relatively large numbers of) crimes, they reduce their risk perceptions. Fourth, in line with the naiveté effect, this reduction occurs primarily among those individuals who estimate the detection risk as high in periods in which they are not committing crimes. However, our research also yielded an intriguing finding that challenges the experiential argument in its current form. This finding suggests that people seem to return to initial overestimated risk levels when they stop committing crimes. If they start committing crimes again, they again reduce their risk estimates to a similar degree. This finding challenges the assumption of the novelty effect that once a person develops more accurate perceptions through first criminal experiences, perceptual changes brought about through new information should be less likely and less extensive. Future research should investigate this bouncing-back effect through episodes of non-offending in more detail. The experiential argument may eventually need to be refined to account for the short-lived nature of criminal experience effects. In this refined version, the novelty effect may give way to the "ephemerality effect," which would state that only (or especially) recent criminal activity is relevant for the lowering of risk perceptions, while older criminal experience loses its perceptual impact over time.

5.6 | Appendix

Table 5.4: Description and descriptive statistics of the variables included in the fixed effects models

Variable	Description	Descriptive statistics
(General) Perception of detection risk	The assessment or perception of the general detection likelihood when offending. Operationalization: Mean score based on the following items: How likely do you think it is that you would be caught committing one of these acts? [assault / burglary / shoplifting / vandalism]; Response categories: (0) very unlikely, (1) unlikely, (2) neither/nor, (3) likely, and (4) very likely.	Range = 0 to 4, Mean = 2.18, SD = 0.99
Criminal experience	Past and recent personal involvement in criminal activity. Operationalization: Combining the sum of criminal acts in the past year (frequency of committing assault (no weapon), assault (with a weapon), bag-snatching, bicycle theft, burglary, graffitiing, fencing stolen goods, robbery, shoplifting, scratching, theft of a car, theft from a car, theft from a vending machine, theft (other), and other vandalism) with data on criminal activity (of the criminal offenses mentioned before) ever in the past, lead to an ordinal variable with the following categories: (0) never committed a crime, (1) committed a crime, but not in the past year, (2) 1-2 crimes in the past year, (3) 3-9 crimes in the past year, (4) 10 or more crimes in the past year.	Percentages = (0) 38.9% / (1) 35.5% / (2) 9.1% / (3) 7.8% / (4) 8.8%
Peer group exposure	The exposure to deviant pattern in one's peer group. Operationalization: Deviant peer group exposure was measured with an ordinal variable. Its first category includes people who reported to have no peer group at the given time or to spend little (i.e., "rarely" or "once or several times a month") time with it; the additional three categories are based on trichotomizing a mean score (range -2 to 2) of deviant peer group exposure (at the values -0.65 and 0.65) based on the items: In how far do the following statements apply to your friend group? [There are other opposing groups. / We also use violence to pursue our interests. / We fight with other groups. / When we show up together, others truly have respect. / When we're together, we drink a lot of alcohol. / Sometimes we do something illegal for fun.]; Response categories: (-2) disagree, (-1) fairly disagree, (0) agree partly, (1) fairly agree, (2) totally agree; The resulting variable has the following categories: (0) spending no or little time with a peer group, (1) spending much time in low-deviant peer group activities, (2) spending much time in medium-deviant peer group activities, and (3) spending much time in high-deviant peer group activities.	Percentages = (0) 42.1% / (1) 40.9% / (2) 12.9% / (3) 4.1%

Notes: All categorical (ordinal) variables are included in modelling as dummies with the reference category being the category 0 of each variable.

Table 5.4: (Continue.)

Variable	Description	Descriptive statistics
Perceived neighborhood disorder	The assessment or perception of disorder in one's own neighborhood. Operationalization: Mean score based on the following items: Do the following problems apply to the neighborhood where you live? [run-down, vacant buildings / waste and bulky waste lying around on pavements and green areas / drunkards / drug addicts / destroyed telephone booths, letter-boxes, trash cans, bus stops, benches / sprayed, smeared house walls / juveniles who threat, mug or beat up others / too many foreigners, asylum seekers / undisciplined drivers / noisy neighbors / juveniles who are bored and doing nothing / loud argument between adults]; Response categories: (-2) no problem, (-1) little problem, (0) medium problem, (1) quite a problem, (2) large problem.	Range = -2 to 2, Mean = -0.45, SD = 0.82
Watch crime movies	The frequency of watching crime movies. Operationalization: The measurement is based on the following item: How frequently do you watch crime movies?; Response categories: (0) never, (1) rarely, (2) sometimes, (3) often, or (4) very often.	Percentages = (0) 18.4% / (1) 28.2% / (2) 29.2% / (3) 15.7% / (4) 8.6%
Panel wave	The panel wave the observations comes from. Operationalization: This measure contains the simple information from which panel wave the respective data comes from. It is an ordinal variable with the following categories: (0) Panel wave 2003, (1) Panel wave 2004, (2) Panel wave 2005, (3) Panel wave 2006.	Percentages = (0) 22.2% / (1) 26.2% / (2) 27.0% / (3) 24.6%

Notes: All categorical (ordinal) variables are included in modeling as dummies, with the reference category being category 0 of each variable.

6 | PAPER V

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Differential updating and morality: Do people learn differently from police detection depending on their personal morals?

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ABSTRACT

Differential deterrability has been studied from a one-sided perspective in the research to date. Most existing studies have investigated whether people differ in the extent to which a perceived threat of sanctions deters them from committing a crime. Far less research has explored the differential influence of criminal justice intervention on sanction threat perceptions. According to deterrence theory, however, for formal intervention to successfully deter crime, a process of perceptual updating is required. In the current study, we used panel data from German adolescents to extend the research on differential updating. We applied fixed effects regressions to analyze whether people with weaker or stronger morals update their perceptions of detection risk differently following experiences of police detection. Our findings suggest that they do: Risk perceptions increased more in adolescents with weak morals than in adolescents with strong morals when they experienced a higher certainty of detection. Combined with previous findings on differential deterrence (by personal morality), our results indicate that deterrence processes may—at least for individuals with weak morals—play a more critical role in the prevention of crime than previous research has suggested.

KEYWORDS

perceptual deterrence theory, differential deterrability, differential updating, risk perceptions, personal morals or morality

6.1 | Introduction

The foundations of deterrence theory were laid in the eighteenth century by philosophers Cesare Beccaria (1764/1872) and Jeremy Bentham (1789/2000). The deterrence doctrine that developed out of their work assumed that legal punishment can prevent crime (by eliciting fear of punishment) if it is applied with sufficient certainty, severity, and celerity. Deterrence theory has evolved substantially since that time (see Loughran et al., 2018; Piquero et al., 2011): It has been reframed as a perceptual theory, with sanction threat perceptions (i.e., perceptions of punishment certainty, severity, or celerity) as the central transmitter of punishment threats (Geerken & Gove, 1975), and has since been reconceptualized to include experiential and observational learning processes triggered by personal and vicarious punishment (avoidance) experiences (Stafford & Warr, 1993).

A more recent refinement to deterrence theory is the idea of *differential deterrability*, which suggests that the likelihood of deterrence differs across situations and individuals (Loughran et al., 2018; Piquero et al., 2011).[111] Most differential deterrability research has investigated whether individuals vary in their likelihood of being deterred from criminal behavior by their perceptions of the certainty of arrest or detection[112] (referred to as *risk perceptions* in the following).[113] To study this varying likelihood, the research has analyzed potential moderators of the deterrence process, including delinquent peer associations, emotional and pharmacological arousal, prosocial bonds, and self-control abilities (for reviews, see Hirtenlehner, 2020; Loughran et al., 2018; Piquero et al., 2011).

Much of this differential deterrability research has concentrated on personal morals or morality (views of what behavior is right or wrong, or good or bad)[114] as a moderator of deterrence effects. The interest in morality may have originated from Talcott Parson's (1937/1968) early interpretation of Émile Durkheim that personal morals may make deterrence considerations irrelevant in some circumstances, operating as a sort of a "filtering mechanism" (Herman & Pogarsky, 2020; see also Grasmick & Green, 1981; Wright et al., 2004). In modern criminology, this filtering was

[111] Although the basic idea behind differential deterrability was introduced long ago (e.g., Toby, 1957), it was the systematic review of Piquero et al. (2011) that brought it into the spotlight.
[112] In the following, we generally refer to experiences and perceptions of *detection*, as these are the subject of our analysis. However, we also think that our discussion and results could be applied to the phenomenon of *arrest*, which has been examined more often in previous research.
[113] Less attention has been paid to how differential deterrence affects perceptions of the severity of punishment, and the perceived celerity component of deterrence has been almost entirely neglected in the (differential) deterrence research (for a review of this literature, see Loughran et al., 2018; Piquero et al., 2011).
[114] This *thin* understanding of morality has been applied in most of the previous deterrability research (but see Herman & Pogarsky, 2020). It does not take a moralistic stance towards particular behaviors but instead simply uses information about personal views on specific (here: criminal) behaviors to explain why some people engage in such activities while others do not (see also Wikström, 2019a).

formalized most thoroughly by Per-Olof Wikström. His Situational Action Theory (SAT) considers personal morals part of a moral filter that excludes some behavioral alternatives from the perceived range of possible actions in a given situation while including others (Wikström, 2019a; Wikström et al., 2012). The weaker a person's morals against delinquency (the weaker their moral opposition to delinquency), the more likely that person's filter is to include crime as an action alternative. According to SAT, deterrence processes will only be relevant when the person's filter includes criminal behavior as an option and when the individual deliberates over committing a crime. Deterrence will therefore rarely affect the behavior of individuals with strong morals since their moral filter will typically prevent them from seeing crime as an action alternative in the first place. In line with this reasoning, a number of empirical studies have found that sanction threat perceptions deter crime only or especially among individuals with weak morals. Those with stronger morals, in contrast, generally commit fewer crimes and are typically affected less or not at all by their perceptions of sanction threat (e.g., Bachman et al., 1992; Hirtenlehner & Reinecke, 2018a; Kroneberg et al., 2010; Paternoster & Simpson, 1996; Svensson, 2015).[115]

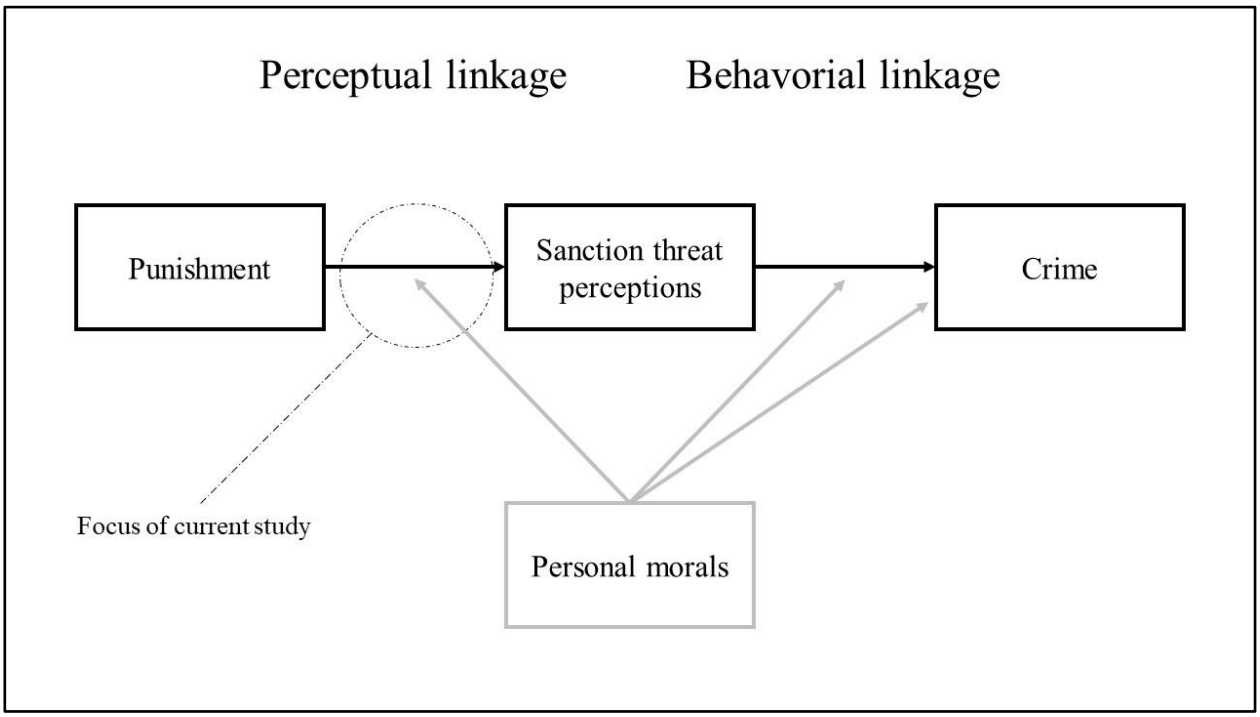

Figure 6.1: Differential deterrability by personal morals in perceptual deterrence theory

However, even if sanction threat perceptions affect individuals with weaker morals, this does not provide sufficient evidence to conclude that criminal justice intervention can prevent future crimes

[115] Hirtenlehner and Reinecke (2018a) relied on data from the German sample used in the current study. For some mixed or even contradictory results, see Gallupe and Baron (2014).

through deterrence. This is because perceptual deterrence theory encompasses not one but two processes or linkages (Paternoster, 2018; Pogarsky et al., 2004; see Figure 6.1), both of which are required to deter criminal behavior. Formal intervention can prevent crime only if it, first, alters the perceptions of sanction threat (*perceptual linkage*), which, second, deter people from committing crimes (*behavioral linkage*).

The first of these, the perceptual linkage, is often neglected in deterrence research (Pogarsky et al., 2004), and this is particularly true for the study of differential deterrability (Loughran et al., 2018). So far, only a handful of studies have investigated whether individuals vary in how they update their risk perceptions after being arrested or detected by the police. This differential updating research has concentrated mainly on individuals' self-control abilities and criminal history as potential moderators of the updating process (e.g., Pogarsky et al., 2004, 2005; Schulz, 2014; K. J. Thomas et al., 2013).[116] Personal morality, in contrast, has been almost entirely ignored as a moderating force in the study of perceptual updating. To date only Pogarsky et al. (2005) have studied whether updating differs between individuals who vary in their morals, producing mixed evidence of such differential learning. With this dearth of research, it remains questionable whether those with weak personal morals can be deterred from crime through criminal justice intervention, even if the available evidence suggests that the behavioral linkage may be active.

The current study supplements the scarce research on differential updating. It explores whether individuals who differ in their morals update their risk perceptions differently after being detected by the police. For this purpose, the article first derives hypotheses from two perspectives on this (potentially differential) updating process. It then empirically evaluates the validity of the hypotheses using panel data from German adolescents.

6.2 | Personal morals and differential updating of risk perceptions

The deterrence literature offers two perspectives on how personal morals may (or may not) moderate the updating process. The first perspective is taken by the classical school of criminology around Beccaria (1764/1872) and Bentham (1789/2000). It assumes that all people are equally deterrable through (threats of) punishment (see also Loughran et al., 2018; K. J. Thomas et al., 2013). Accordingly, personal morals should not moderate either the perceptual or the behavioral

[116] To our knowledge, differential updating research has so far focused only on perceptions of punishment certainty, leaving out the study of changes in the perceptions of punishment severity or celerity.

deterrence linkage. Thus, people should increase their risk perceptions after detection experiences irrespective of their morality. This gives rise to our first hypothesis:

H_0: All individuals increase their risk perceptions similarly after experiences of police detection.

The second perspective was introduced by Pogarsky et al. (2005, p. 8), who "hypothesize[d] that moral inhibition [in our notion: strong personal morals] will reduce the degree to which offending and any consequences affect perceptions of sanction certainty." To explain why personal morals should moderate the updating process in the proposed way, Pogarsky and colleagues referred back to Etzioni (1988). Etzioni argued that strong morals towards particular behaviors may make them "nonmarket." Individuals who are not "in the market" for a specific behavior do not commit it because they see it as morally wrong.[117] Instrumental calculations should play no (or a lesser) role in their decisions to act (or not to act). Benefit- and cost-related information of the nonmarket behavior should be largely irrelevant to those individuals. They consequently have no incentive to invest energy to update their perceptions of these benefits and costs (Apel, 2013). Pogarsky and colleagues thus argue that information about the benefits and costs of crime is (largely) irrelevant for people with strong moral opposition to delinquency. These individuals should be unlikely, or at least less likely than individuals with a weaker moral opposition to delinquency, to process personal (or vicarious) criminal and detection experiences[118] (which include information about benefits and costs of crime) to update their risk perceptions. This gives rise to our second hypothesis:

H_1: Only or especially individuals with weaker morals against delinquency increase their risk perceptions after experiences of police detection.

As reported above, so far only Pogarsky et al. (2005) have conducted an empirical investigation of whether personal morality moderates the updating process. For this purpose, they analyzed differential changes in perceptions of the risk of arrest for theft or assault in a nationally representative sample of juveniles in the United States. In line with implications of the "nonmarket argument,"

[117] As mentioned in the introduction, this is akin to the moral filtering proposed in other sociological or criminological theories: Strong personal moral opposition to a particular behavior (strongly internalized social norms) will, in many circumstances, contribute to or work as a moral filter that prevents seeing such behavior as a proper action alternative (e.g., Kroneberg et al., 2010; Wikström, 2019a).

[118] The question of why individuals with overall high morals may still commit crimes can be answered with the help of SAT in at least two ways. First, they may be driven by environmental factors to break the law. Deviant peers, for example, may provide a moral context that pressures them to engage in deviant activity (e.g., Beier, 2018; Kaiser, 2021). Second, personal morals are *specific to the circumstances* individuals encounter (Wikström, 2019a). As such, it may be that a person with otherwise strong morals against theft may still find such an action acceptable when it is needed to provide food for a starving loved one.

their results indicate that changes in the number of arrests were more strongly related to increases in risk perceptions among those with weak morals against delinquency than among those with strong morals. However, their moderation estimates were relatively uncertain and, as a result, statistically insignificant. This estimation uncertainty can be attributed to their relatively small database (approx. 1,725 observations). The current study strives to overcome this statistical power issue by using a larger data pool (see methods section). Since these data were collected from German adolescents, our study also examines whether the results of Pogarsky and colleagues translate to another national context.

6.3 | An alternative explanation of the proposed differential updating

Consistent with the "nonmarket argument" and SAT's moral filter, a growing body of research suggests that individuals with strong morals commit fewer crimes than individuals with weaker morals (e.g., Antonaccio & Tittle, 2008; Brauer & Tittle, 2016; Gallupe & Baron, 2014; Wikström & Butterworth, 2006). This information must be considered when investigating whether personal morals moderate the effects of crime detection on risk perceptions. Bayesian updating models suggest that the individual's experienced certainty of arrest (i.e., the ratio of arrests to crimes committed) affects risk perceptions differently depending on how many offenses the individual has committed (Anwar & Loughran, 2011; see also Pogarsky et al., 2004). A high number of criminal experiences should provide more reliable information (i.e., a more informative signal) to a person about how likely it is that they will be caught or arrested. The experienced certainty of detection thus should be more strongly related to increased risk perceptions when an individual has committed many crimes.

Against this backdrop, only comparing how individuals with weak and strong morals update their risk perceptions following detection experiences may lead to false conclusions. There are (at least) two explanations for the finding that individuals with weaker morals update their risk perceptions more substantially than individuals with stronger morals. A first explanation of such differential updating, which is in line with Pogarsky et al. (2005), is that detection information may be of little relevance to individuals with strong morals because they are not "in the market," and instrumental considerations (including risk perceptions) do not guide their actions. Consequently, they will not invest energy to process detection information and update their perceptions accordingly. A second explanation of the differential updating is that those with weak morals commit more crimes on average. Their detection (certainty) knowledge is hence based on more experiences and is thus

more informative. According to this explanation, it is not the irrelevance of the information that diminishes the updating of risk perceptions among individuals with strong morals, but rather the fact that their detection information is based on fewer criminal experiences and is thus less informative. No study to date has investigated this latter explanation, making the current study the first to test the following hypothesis:

H_2: If one controls for the frequency of criminal offending, personal morality loses its moderating impact on the relationship between experiences of police detection and risk perceptions.

6.4 | Methods

6.4.1 | Data

The data for our analysis stems from the panel study *Crime in the modern City* (CrimoC; Boers et al., 2010). In its initial wave in 2002, CrimoC tried to sample all seventh graders in Duisburg, an industrial city in the west of Germany. With 3,411 on average 13-year-old juveniles, 61% of the student population participated in the first panel wave. These participants were asked to complete self-administered questionnaires on a regular basis—first annually, later biennially—up to the year 2020, encompassing a range of topics including delinquent behavior, routine activities, and normative attitudes.

For our analysis, we used only data from panel waves 2 to 5 (i.e., during the respondents' adolescent years) and included only observations from participants that met two conditions. First, the juveniles had to have participated in at least two of the four panel waves. Second, all data had to be complete for each observation to be included in the analysis. Furthermore, the analysis sample includes only observations in which individuals had reported at least one criminal offense. This offender-only stratification has been used more often recently to study the perceptual effects of arrests or detections (e.g., Anwar & Loughran, 2011; Schulz, 2014). Due to these selection criteria, our final analysis sample consists of 2,231 observations from 1,385 adolescents. If not otherwise mentioned, we included the relevant measures as time-variant concepts in our analyses (for more information on the measures and descriptive statistics, see online supplementary material, Table S1).

6.4.2 | Measures

Perceptions of detection risk. Our dependent variable measures individual perceptions (assessments) of the risk of detection when committing crimes.[119] More specifically, the juveniles were asked how likely they thought it would be for them to get caught if they committed the following types of delinquency: assault, bicycle theft, burglary, extortion, provocation or intimidation, shoplifting, car theft, and vandalism. The response categories were (0) very unlikely, (1) unlikely, (2) neither/nor, (3) likely, and (4) very likely. To generate a score of the *general perception of detection risk*, we calculated a mean score across the eight offenses (range: 0-4).

Self-reported criminal offending. Juveniles were asked whether and how frequently they had been involved in various delinquent behaviors over the last year. We used frequency reports on the commission of assault, shoplifting, graffitiing, scratching, and (other forms of) vandalism to generate a criminal offending variable.[120] To calculate such a variable, we first summed up the reported frequencies on the different criminal offenses. As the sum score is highly skewed to the right, we categorized it to diminish the effects of outliers (for a similar approach, see Matsueda et al., 2006; Schulz, 2014). The resulting ordinal variable has the following categories: (0) 1-2 offenses, (1) 3-9 offenses, and (2) 10 or more offenses.

Detection-crime ratio. To include detection information as our key independent variable, we followed previous research by calculating a detection-crime ratio (e.g., Anwar & Loughran, 2011; Matsueda et al., 2006; Schulz, 2014; K. J. Thomas et al., 2013). A detection-crime ratio is argued to be more closely related to the perceived detection risk than the pure number of times a juvenile was detected for committing a crime (Horney & Marshall, 1992). In addition to the offending information on the five criminal offenses mentioned above, we therefore also relied on reports on the number of crimes the police were aware of. We summed up the crime (C_f) and detection (D_f) frequencies and finally generated a *detection-crime ratio* for each respondent by dividing the total number of police detections by the total number of crimes: $DCR = Sum(D_f)/Sum(C_f)$.

[119] Wikström (2008) criticized deterrence researchers for talking about studying risk perceptions, but only measuring relatively abstract risk assessments. He argued that these rather contextless assessments cannot serve as a proper operationalization of risk perceptions, which have a situational (context-specific) nature. However, Wikström also acknowledged that respondents' general risk assessments are likely indicative of their sensitivity to deterrence and thus related to the perceptions of risk they form in real life.

[120] We selected these particular offenses from the larger pool of delinquency items available in the CrimoC survey as these were the ones the respondents reported having committed and being detected for most frequently. This selection thus increases the variance in detections (and the detection-crime ratio) and maintains as many observations as possible in our offender-only sample.

Personal morals. Our personal morals scale is based on the juveniles' reports on whether they approved or disapproved of eight different delinquent behaviors (assault, bicycle theft, burglary, car theft, extortion, provocation/intimidation, shoplifting, vandalism). The participants could respond that they thought committing the offense in question was (-2) completely harmless, (-1) relatively harmless, (0) neither/nor, (1) relatively bad, or (2) very bad. We then generated a personal morals score (range: -2 to 2) by taking the mean across the different criminal behaviors *and over the multiple panel waves*. We, thus, followed previous research in including our moderator as a time-invariant variable (e.g., Schulz, 2014; K. J. Thomas et al., 2013; van Veen & Sattler, 2018).[121]

Other covariates. Our selection of other covariates was based on Stafford and Warr's (1993) reconceptualized deterrence theory, in which they suggest that risk perceptions are learned not only through personal but also through vicarious experiences. In particular, we included the following variables that provide information on the latter type of experiences: First, we considered data on juveniles' *exposure to a deviant peer group*. This peer group measure is ordinal and consists of the following categories: (0) spending no or little time with a peer group, (1) spending a large amount of time in low-deviant peer group activities, (2) spending a large amount of time in medium-deviant peer group activities, and (3) spending a large amount of time in high-deviant peer group activities. Second, we assessed the individuals' *perceptions of neighborhood disorder*. The perceived disorder variable is continuous, ranging from -2 to 2, with higher values indicating that a person perceived more disorder. Finally, we used reports about how often the respondents watched crime movies to assess the influence of media consumption on risk perceptions. More precisely, the respondents could indicate that they (0) never, (1) rarely, (2) sometimes, (3) often, or (4) very often watched crime movies.

6.4.3 | Analytical Procedure

To study the updating processes outlined in our hypotheses, we relied on a series of fixed effects regression models (Allison, 2009). These models allow the updating process to be modelled in an intraindividual way by focusing on how individual risk perceptions change over time on average.

[121] Our main reason for including personal morals as a time-invariant variable is that we could not determine a causal time order between risk perceptions and time-variant measures of personal morals. Our analysis, thus, implicitly assumes that personal morality is, as a result of previous socialization processes, relatively stable over time and between people. Calculated correlation coefficients between morality indicators of adjacent panel waves (range: 0.49 to 0.56) bolster our stability assumption to some extent, as do standardized stability estimates (range: 0.48 to 0.65) from structural equation models reported in a previous CrimoC publication (Seddig, 2014).

Since learning or updating processes operate within individuals over time, capitalizing on intraindividual variation seems more appropriate than resorting to perceptual variation between individuals. Furthermore, beyond being an intuitive choice for studying learning processes, fixed effects models have the advantage of automatically accounting for all of the respondents' (unobserved) heterogeneity due to time-invariant factors by estimating pure within-effects (Wooldridge, 2010).

We specified our fixed effects regression models in such a way that differences from the within-person mean of general risk perceptions $\overline{Y_i}$ at time point t are regressed on differences from the within-person mean in the covariates $\overline{X_i}$:

$$(Y_{it} - \bar{Y}_i) = (X_{it} - \bar{X}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i)$$

Our first model (Model 1) only includes the detection-crime ratio as a predictor variable and the general risk perceptions score as the dependent variable. This model gives a first impression of how changes in the experienced detection certainty are associated with changes in individuals' risk perceptions. The second model (Model 2) extends the first one by including the other predictors (i.e., the vicarious crime-related information). This inclusion should allow for a more unbiased estimation of the perceptual effect of the detection-crime ratio.[122]

The third model (Model 3) allows for an assessment of hypotheses H_0 and H_1 by including an interaction term between the personal morals score and the detection-crime ratio. Although we treat the information on personal morals as a time-invariant factor, which is automatically eliminated in fixed effects models, unit-level differences can nonetheless be considered in these models via interaction terms. It is thus possible to examine the extent to which individuals with weaker morals adjust their risk perception differently than individuals with stronger morals after experiences of detection (see Schulz, 2014 for a similar approach but with self-control capabilities as the moderator).

Finally, the fourth model extends the former model specification by including a three-way interaction between personal morals, criminal experience, and the detection-crime ratio. This three-way interaction allows for an accurate assessment of hypothesis H_2, which suggests that the moderation of the updating process by personal morality can be (largely) explained by the variation in criminal involvement among individuals with different morals.

[122] Additionally, we included the current panel wave as predictor in this and the following model specifications. Although this variable does not allow any substantive statements to be made, it picks up potential year shocks resulting from underlying unobservable systematic differences between observed time units (period effects) and therefore prevents corresponding distortions.

6.5 | Results

This section presents the results of our fixed effects models (see Table 6.1).[123] As outlined above, Model 1 predicts the intraindividual changes in risk perceptions using only the detection-crime ratio as predictor. The estimate of the ratio variable indicates that the higher the individual's experienced certainty (or rate) of detection, the higher their subsequent perceived risk of getting caught (β = 0.31 [0.02 0.60]). More precisely, when a person's detection certainty increased by 0.1, or ten percentage points (e.g., they were detected in 2 out of 10 instead of 1 out of 10 crimes), their risk perceptions rose on average by just 0.03 units.

In Model 2, which encompasses the other covariates in addition to the detection-crime ratio, the effect estimate of the detection certainty decreases and loses its statistical significance. However, the direction of the estimate remains the same (β = 0.21 [-0.08 0.51]). It indicates that a person's general risk perception increased on average by 0.02 units when their experienced detection rate rose by ten percentage points.

Beyond this small, nonsignificant effect of the experienced detection certainty, our model estimated a more precise influence of criminal offending on risk perceptions. If the adolescents committed crimes repeatedly instead of only once or twice in a given period, they reported reduced risk perceptions (3-9 offenses: β = -0.12 [-0.24 0.00]; ≥ 10 offenses: β = -0.20 [-0.33 -0.07]). Like in previous research, the commission of more crimes thus was related to somewhat lower risk perceptions (e.g., Hirtenlehner & Wikström, 2017; Matsueda et al., 2006; Schulz, 2014).[124] For all other covariates that include vicarious information about criminal experience, the model instead estimates small and statistically insignificant effects. This lack of impact is also consistent with previous research, which suggests that vicarious information is less relevant for updating risk perceptions among individuals who have had personal experiences of committing crimes in a given period (which all individuals in our offender-only sample have done; Paternoster & Piquero, 1995; Pogarsky et al., 2004; van Veen & Sattler, 2018).

[123] We, additionally, calculated the intraclass correlation coefficient (ICC = 0.40) of a null model (not shown), suggesting that risk perceptions vary to a similar extent within and between individuals.
[124] This result was also found in previous studies using the data at hand (Kaiser et al., 2021; Seddig et al., 2017).

Table 6.1: Modelling the updating process: Changes in general risk perceptions

	Model 1			Model 2			Model 3			Model 4		
Predictor	**β**	**95%-CI**		**β**	**95%-CI**		**β**	**95%-CI**		**β**	**95%-CI**	
Detection-crime ratio (DCR)	0.31	[0.02	0.60]	0.21	[-0.08	0.51]	0.50	[0.14	0.87]	0.73	[0.30	1.16]
DCR*Personal morals (PM)							-0.46	[-0.91	-0.01]	-0.80	[-1.32	-0.28]
Criminal experience (CrEx) *(ref.: 1-2 offenses)*												
3-9 offenses				-0.12	[-0.24	0.00]	-0.12	[-0.24	0.00]	-0.07	[-0.24	0.11]
≥ 10 offenses				-0.20	[-0.33	-0.07]	-0.20	[-0.33	-0.07]	-0.23	[-0.39	-0.06]
DCR*CrEx *(ref. CrEx: 1-2 offenses)*												
*DCR*CrEx: 3-9 offenses*										-1.19	[-2.04	-0.33]
*DCR*CrEx: ≥ 10 offenses*										-0.36	[-1.42	0.69]
CrEx*PM *(ref. CrEx: 1-2 off.)*												
*CrEx: 3-9 off.*PM*										-0.10	[-0.32	0.13]
*CrEx: ≥ 10 off.*PM*										0.12	[-0.13	0.36]
DCR*CrEx*PM *(ref. CrEx: 1-2 off.)*												
*DCR*CrEx: 3-9 off.*PM*										2.25	[1.17	3.33]
*DCR*CrEx: ≥ 10 off.*PM*										-0.20	[-2.05	1.64]
Peer group exposure *(ref.: no or little exposure)*												
Low-deviant				0.13	[-0.02	0.27]	0.12	[-0.02	0.27]	0.12	[-0.02	0.27]
Medium-deviant				0.05	[-0.09	0.19]	0.05	[-0.09	0.19]	0.06	[-0.08	0.20]
High-deviant				-0.11	[-0.28	0.06]	-0.11	[-0.28	0.06]	-0.10	[-0.27	0.07]
Neighborhood disorder				-0.06	[-0.14	0.03]	-0.06	[-0.14	0.02]	-0.06	[-0.14	0.02]
Watch crime movies *(ref.: never)*												
Rarely				0.06	[-0.09	0.21]	0.05	[-0.09	0.20]	0.06	[-0.09	0.21]
Sometimes				0.07	[-0.10	0.24]	0.07	[-0.10	0.24]	0.08	[-0.09	0.25]
Often				0.02	[-0.18	0.21]	0.01	[-0.18	0.20]	0.01	[-0.18	0.21]
Very often				0.04	[-0.19	0.27]	0.03	[-0.20	0.26]	0.02	[-0.21	0.26]
Constant	1.96	[1.94	1.97]	2.08	[1.91	2.26]	2.08	[1.90	2.26]	2.07	[1.89	2.25]
Panel waves					x			x			x	
Persons		1,385			1,385			1,385			1,385	
Observations		2,231			2,231			2,231			2,231	

Notes: Unstandardized regression coefficients of fixed effects models with cluster-robust confidence intervals (CI).

Model 3 includes personal morals as a moderator of the updating process and thus produces some estimates for assessing hypotheses H_0 and H_1. While the effect estimates of all other covariates remain basically the same in this model specification, the inclusion of the personal morals variable as a moderator affects the relationship between the detection-crime ratio and the risk perceptions. To present the results of this moderation more intuitively, we used the regression estimates to compute average marginal effects (AMEs) across the dimension of personal morals (see Figure 6.2 and Table 6.2).[125]

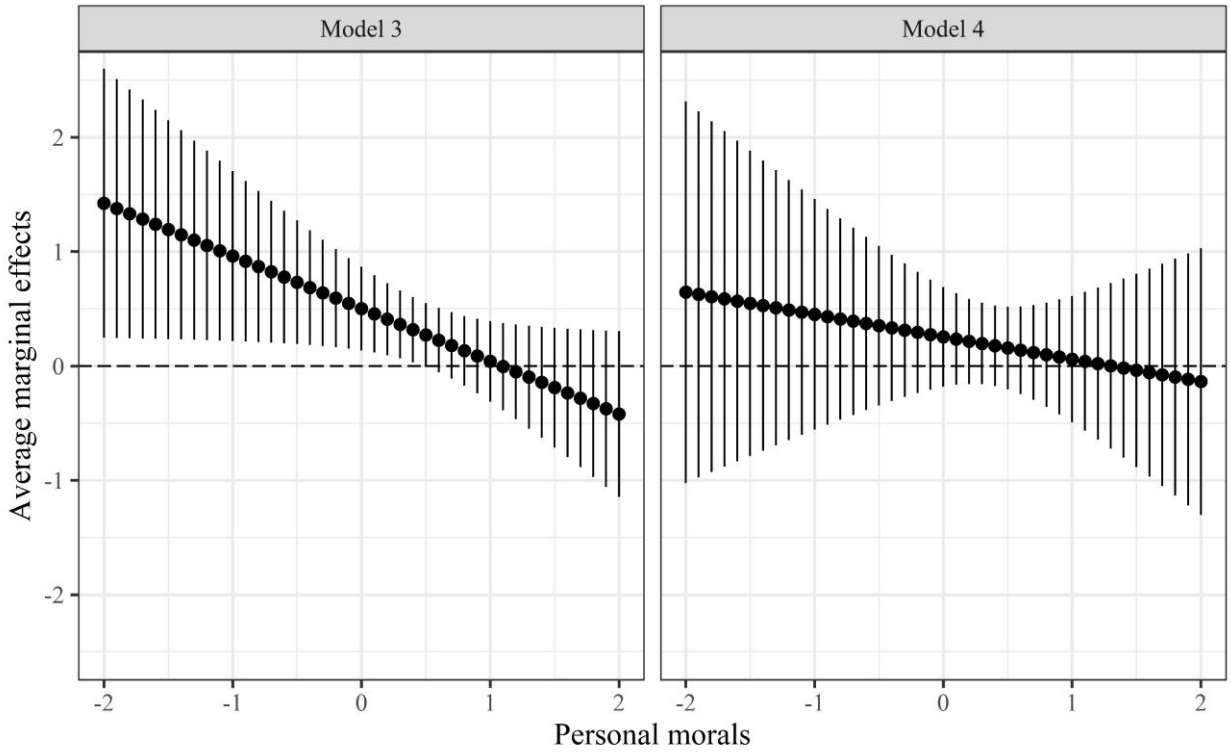

Figure 6.2: Average marginal effects of the detection-crime ratio on risk perceptions by personal morals

The AME estimates are consistent with hypothesis H_1 and the direction of the differential effects reported by Pogarsky et al. (2005). They indicate that only those with weak morals showed a substantial increase in risk perceptions after experiencing a somewhat higher detection certainty (e.g., $AME_{PMorals=-2.0} = 1.42$ [0.25 2.60]). More precisely, when the detection ratio of an individual with a personal morals score of -2.0 (i.e., with very weak morals) increased by ten percentage points, their risk perceptions rose by 0.14 units on average. The risk perceptions of individuals with stronger morals, in contrast, were not substantially (and only insignificantly) affected by an increased detection certainty (e.g., $AME_{PMorals=1.0} = 0.04$ [-0.31 0.39]). However, as a note of caution:

[125] For more information on AMEs and their advantages in linear regression models with interactive terms, see Mize (2019).

The interaction effect between personal morals and the detection-crime ratio was estimated relatively imprecisely. The actual strength of the moderation in the juvenile population thus remains relatively uncertain ($\beta = AME_{PMorals} - AME_{PMorals+1} = -0.46$ [-0.91 -0.01]).

Table 6.2: Average marginal effects of the detection-crime ratio on risk perceptions by personal morals

	Model 3			Model 4		
Personal morals (PM)	**AME [95%-CI]**			**AME [95%-CI]**		
-2.0	1.42	[0.25	2.60]	0.65	[-1.02	2.31]
-1.5	1.19	[0.23	2.15]	0.55	[-0.79	1.88]
-1.0	0.96	[0.22	1.71]	0.45	[-0.56	1.46]
-0.5	0.73	[0.19	1.27]	0.35	[-0.34	1.05]
0.0	0.50	[0.14	0.87]	0.25	[-0.18	0.69]
0.5	0.27	[-0.01	0.55]	0.16	[-0.20	0.52]
1.0	0.04	[-0.31	0.39]	0.06	[-0.49	0.61]
1.5	-0.19	[-0.71	0.33]	-0.04	[-0.88	0.81]
2.0	-0.42	[-1.15	0.31]	-0.14	[-1.30	1.03]
	Second difference [95%-CI]			**Second difference [95%-CI]**		
$AME_{PM} - AME_{PM+1}$	-0.46	[-0.91	-0.01]	-0.20	[-0.88	0.49]

Notes: AMEs are calculated based on the fixed effects models (see Table 6.1, Models 3 and 4).

Finally, to test hypothesis H_2, Model 4 additionally specifies a three-way interaction between the detection-crime ratio, criminal experience, and personal morals. This specification allows for the investigation of whether the moderation of the updating process by personal morals is due to the variation in criminal involvement of individuals with different personal morals. A first inspection of descriptive statistics for three subsamples stratified by personal morals provides some initial support for hypothesis H_2 (see Table 6.3).[126] Individuals with stronger morals committed, on average, far fewer crimes than those with weaker morals (Pearson's $r = -0.25$; Spearman's $\rho = -0.35$; not shown in a table). The moderation may thus indeed be explained by differential offending among those with different morals.

Our Model 4 corroborates this idea to some extent. Since the regression coefficients of (three-way) interactions themselves are difficult to interpret, we again use these estimates to calculate AMEs of the detection-crime ratio across the dimension of personal morals (see Figure 6.2 and Table 6.2). The results show that the strength of the moderation of the updating process by personal morals is somewhat reduced, and estimates are more uncertain than in the previous model specification. When only considering the point estimates, the model still indicates (although less

[126] We split the personal morals score into three unequally spaced categories (weak: -2 to 0; medium: 0 to 1; strong: 1 to 2) to consider the skewed nature of the variable and to communicate the differences in criminal activity in a relatively efficient way (Gelman & Park, 2009; for a similar approach, see also Kaiser, 2021).

markedly) that those with weaker morals (e.g., $AME_{PMorals=-2.0}$ = 0.65 [-1.02 2.31]) update their risk perceptions more strongly due to an increased experienced detection certainty than individuals with stronger morals (e.g., $AME_{PMorals=1.0}$ = 0.06 [-0.49 0.61]). However, the differences in the point estimates are diminished, and the widened confidence intervals reflect an increased estimation uncertainty. Consequently, differences in the AMEs of individuals with weak and strong morals are no longer statistically significant in this model specification ($AME_{PMorals}$ - $AME_{PMorals+1}$ = -0.20 [-0.88 0.49]). However, due to the estimation uncertainty, it would be premature to conclude that this finding unequivocally supports the hypothesis that the variation in criminal involvement across the morality dimension can explain the differential updating.

Table 6.3: Criminal experience by personal morals

	Personal morals					
	Weak (-2 to 0)		Medium (0 to 1)		Strong (1 to 2)	
Criminal experience	**n**	**%**	**n**	**%**	**n**	**%**
1-2 offenses	67	16.5	462	35.2	277	54.3
3-9 offenses	111	27.3	416	31.7	139	27.3
≥ 10 offenses	229	56.3	436	33.2	94	18.4
Mean (number of offenses)	27.8		13.0		7.6	
SD (number of offenses)	48.7		25.6		22.3	

Notes: The descriptive statistics are based on the pooled data from the offender-only sample.

6.6 | Discussion

The current study supplements the small body of research investigating the differential updating of risk perceptions. It revisits the question of whether people learn differently from police detection depending on their personal morals. Studying this question with a sample of German juveniles, our longitudinal models produced two main findings.

The first finding suggests that when juveniles experienced a higher certainty of police detection, their general perception of the risk of getting caught increased somewhat on average. This result is in line with aspects of perceptual deterrence theory and particularly Bayesian updating models, and also consistent with previous empirical research (e.g., Anwar & Loughran, 2011; Horney & Marshall, 1992; Matsueda et al., 2006). People seem to be rational in the sense that they update their risk perceptions when confronted with relevant experiences. However, the average updating effects found in the current and in previous studies are typically relatively small. Combined with the relatively modest support for the behavioral linkage (see Figure 6.1; for a review, see Paternoster, 2018), this low explanatory power casts severe doubts on whether (specific) deterrence is

an appropriate goal of criminal justice intervention (for critical perspectives, see Kleck & Sever, 2017; Pratt & Turanovic, 2018).

Confronted with this outlook, some scholars suggest that deterrence may only work for some people or in some situations, and highlight the need to study processes of differential deterrability (e.g., Hirtenlehner, 2020; Loughran et al., 2018; Piquero et al., 2011). The second finding of the current study bolsters their claims. It suggests, in line with hypothesis H_1, that the relationship between experienced detection certainty and risk perceptions varies by personal morality. According to our estimates, only individuals with weak morals updated their risk perceptions substantially after having experienced an increased detection certainty. Risk perceptions of individuals with stronger morals, in contrast, were relatively unaffected by detection experiences. Combined with previous findings that sanction threat perceptions deter criminal behavior only (or primarily) among individuals with weaker morals (e.g., Hirtenlehner & Reinecke, 2018a; Kroneberg et al., 2010; Svensson, 2015), this result has interesting implications for deterrence research. It suggests that criminal justice intervention may have more power to deter people from committing crimes than indicated by non-differential analysis, but that this power applies only or primarily to a subset of individuals. More precisely, in line with the reasoning of Pogarsky et al. (2005), substantial deterrence processes may be restricted to those with weak morals who are generally more likely to contemplate committing crimes.

While our study cannot give a definite answer as to why individuals with weaker morals update their risk perceptions more substantially when exposed to a higher detection certainty, it offers two possible explanations for the differential updating. First, as individuals with weaker morality contemplate committing crimes more frequently than individuals with stronger morals, they need to put more energy into calibrating their perceptions of potential benefits and costs (Pogarsky et al., 2005). Second, individuals with weaker morals commit more crimes on average, and their experiences of detection certainty are thus more informative than the experiences of individuals with stronger morals. We could not test the first explanation since no data on the motivation for such calculations were available. Regarding the second explanation, our point estimates indicate that the moderation of the updating process by personal morals was substantially reduced (more than halved) when adequately controlling for the amount of criminal experience with a three-way interaction. However, the estimates of this more complex model specification were too imprecise to demonstrate with certainty that differences in criminal involvement can really explain the differential updating. Future research should utilize larger samples to overcome this problem and consider other explanations for the differential updating.

Beyond replicating and further exploring the differential updating process, future research should also tackle some of the other issues not fully addressed in the current paper. First, it should investigate the updating process in an offense-specific manner. Most studies on deterrence (including the current one) examine the impact of the *total* number of arrests or the *general* arrest-crime ratio on (general or offense-specific) risk perceptions (e.g., Pogarsky et al., 2004, 2005; Schulz, 2014). They conduct this "global" analysis because arrests are a rare phenomenon (see Kaiser et al., 2021; Lochner, 2007), and the power to analyze their effects can be increased by aggregating arrests across different offense types.[127] A problem with this aggregation is, however, that deterrence theory and the underlying rational choice theories assume that deterrence processes operate (primarily) in an offense-specific manner (Anwar & Loughran, 2011; Paternoster, 1989; but see Stafford & Warr, 1993). Future research should thus be conducted with larger (stratified) samples to enable offense-specific analyses. Such analyses may, at least according to deterrence theory, find even stronger evidence for perceptual deterrence effects among those with weak morals than the global effects reported in the current and previous studies.

Second, a "dirty little secret" of deterrence theory is that research typically only explains a small fraction of the variation in sanction threat perceptions (Paternoster, 2010, p. 808). This observation also applied to the current study: Our fixed effects models account only for 0.6 (Model 1) to 7.4 percent (Model 4) of the intra-individual perceptual variation. This low explanatory power results from the fact that the current and most previous studies restrict their pool of independent variables to experiential determinants. They include only covariates with information about personal or vicarious experiences with criminal behavior and its consequences (including punishment). And even among those experiential determinants, they typically lack some relevant measures, such as indicators on the arrests or detections of relevant others (e.g., peers; see Matsueda et al., 2006; Pogarsky et al., 2004). Moreover, experts have highlighted that most updating models do not account for mental shortcuts (cognitive heuristics) that people use to form their risk perceptions (e.g., Kreager & Matsueda, 2014; Pickett & Roche, 2016; Piquero et al., 2011). Recent research, however, has shown that such shortcuts may play a crucial role in how individuals assess their detection risk (Pogarsky et al., 2017; K. J. Thomas et al., 2018). Future research should consider these

[127] Offense-specific analyses were not feasible with our data for the same reason: Few of our study participants (< 2%) reported having been detected for any specific offense (with less than 15% reporting having committed any specific crime). This low number of (offense-specific) detections would lead to little variation in our crucial predictor, the (offense-specific) detection-crime ratio. Lacking variation in the key predictor, in turn, renders an accurate estimation of the complex differential perceptual effects impossible.

cognitive heuristics in updating models and explore how they relate to differential experiential learning.

Finally, the current and most previous studies focused either on the perceptual or the behavioral linkage. Because of this, they could not assess the hypothesis of perceptual deterrence theory that getting caught should indirectly lead to less criminal behavior via a change in sanction threat perceptions (see Figure 6.1). This lack of a complete analysis is particularly true for research on the moderation of the deterrence process by personal morals. So far, one line of research has investigated whether people with different morals update their risk perceptions differently after arrest (the current study and Pogarsky et al., 2005). The other line analyzed whether the impact of sanction threat perceptions on criminal behavior varies by personal morality (e.g., Hirtenlehner & Reinecke, 2018a; Kroneberg et al., 2010; Svensson, 2015). Although both lines of research have produced promising findings, suggesting that personal morality may indeed moderate the deterrence process, no study to date has investigated the moderation of the perceptual and behavioral processes simultaneously. Only such a complete analysis can ultimately show that deterrence is, at least for those with weak morals, a more critical process than indicated by previous non-differential research.

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