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Sentencing in the Netherlands. Taking risk-related offender characteristics into account

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Citation

Wingerden, S. G. C. van. (2014, September 25). *Sentencing in the Netherlands. Taking risk-related offender characteristics into account*. Meijers-reeks. Eleven International Publishing, Den Haag. Retrieved from <https://hdl.handle.net/1887/28918>

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Title: Sentencing in the Netherlands : taking risk-related offender characteristics into account

Issue Date: 2014-09-25

Pre-sentence reports and punishment

A quasi-experiment assessing the effects of risk-based pre-sentence reports on sentencing¹

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ABSTRACT

The current study investigates the effects of structured risk-based pre-sentence reports on sentencing outcomes in the Netherlands by means of a quasi-natural experiment. Defendants with such a report are compared with similar defendants without such a report, based on propensity score matching and synchronization on nine additional criteria relevant to penal decision-making ($N = 6,118$). Although structured risk-based pre-sentence reports are a textbook example of 'new penological' accounts, high-risk defendants with such a report are not sentenced to more 'controlling' and less 'diverting' sentencing outcomes than are high-risk defendants without such a report. Instead, these reports overall relate to less 'controlling' and more 'diverting' sentencing outcomes, indicating that the penal welfarism account is still prevalent in penal decision-making in the Netherlands.

3.1 INTRODUCTION

'The pre-sentence report touches a corner-stone of any nation's penal structure – namely how we regard the individual person being sentenced' (Wandall, 2010, p. 331). Over recent decades, pre-sentence reports have become a big feature of criminal justice practices. Annually, about 246,000 pre-sentence reports are provided by the National Probation Service in the United Kingdom (Scott, 2008), and over 100,000 pre-trial services reports are prepared by Pretrial Services Officers for the United States courts (Duff, 2009). In the Netherlands, 11,000 pre-sentence reports are produced by the probation agencies every year (Reclassering Nederland, 2012). The goal of these reports is to inform judges

¹ A slightly different version of this chapter has been published in the *European Journal of Criminology* (2014, DOI: 10.1177/1477370814525937).

about a defendant's social background, (criminogenic) circumstances, risk of reoffending and eligibility for certain types of punishment, enabling judges to take this into account when making sentencing decisions.

The importance of pre-sentence reports is reflected in the rising interest in these reports among researchers (Persson & Svensson, 2012). Previous research has concentrated for example on the use of pre-sentence reports, by examining the correspondence between the reports' sentencing recommendations and the sentencing outcome (for example Deane, 2000; Downing & Lynch, 1997; Gelsthorpe & Raynor, 1995). However, correspondence between a recommendation and the sentence does not have to imply that judges follow the recommendations: probation officers might also *anticipate* on sentencing outcomes when they recommend sentences (Halliday, Burns, Hutton, McNeill, & Tata, 2009; Tata, Burns, Halliday, Hutton, & McNeill, 2008).

Although previous studies have contributed to knowledge about the role of pre-sentence reports in penal decision-making, they suffer from a key methodological limitation: they lack a proper control group. To fully assert the effect of pre-sentence reports on penal decision-making, sentencing outcomes for defendants with a pre-sentence report should ideally be compared with outcomes for similar defendants without such a report. However, such research is absent to date. The current study fills this gap by using large-scale, Dutch sentencing data to compare sentencing outcomes for an experimental and a control group – carefully matched on a range of relevant characteristics.

3.2 FROM PENAL WELFARISM TO ACTUARIAL JUSTICE: CHANGING TASKS OF PROBATION AGENCIES

To formulate our expectations about the effects of a risk-based pre-sentence report on sentencing outcomes, it is important to consider the changes in the penological climate and related developments in probation agency tasks over recent decades. After the Second World War, imprisonment rates in the Netherlands dropped to an all-time low and the Dutch penal system was characterized by the belief in the 'improvement' of the delinquent, with rehabilitation as the hallmark of sentencing philosophy. This period of 'old' penology can be typified by concern for individuals (Feeley & Simon, 1994), an attitude more generally reflected in a society characterized by welfarism.

However, since the 1970s, when offense rates kept rising and Martinson's (1974) 'What Works' research was interpreted as 'Nothing Works', people lost faith in the rehabilitative ideal, and penal welfarism was gradually replaced by a more punitive system, with a focus not on 'improving' offenders but on protecting the public by eliminating risks (Downes & Van Swaaningen, 2007). This transformation of the welfare state into a society characterized by a culture of control is analyzed by Garland (2001), and is assumed to exist in the Netherlands as well (Van der Woude, 2010; Van Swaaningen, 1996). Feeley and Simon

(1994) refer to this development in the field of criminal justice as actuarial justice. In this 'new penology', crime is seen no longer as a pathological problem that needs fixing but as a normal phenomenon that can be managed. To do this efficiently, different techniques, such as risk assessments, are used to identify, classify and control groups based on their expected danger to society (Feeley & Simon, 1992, 1994).

Ultimately, the consequence of this actuarial justice is that society is protected by maintaining long-term control over high-risk defendants, with prison as 'a warehouse for the highest risk classes of offenders' (Feeley & Simon, 1992: 460). On the one hand, this 'risk-based sentencing' increases the punitiveness of the criminal justice system by placing high-risk defendants under long-term control, but, on the other hand, scarce penal resources are not used for low-risk offenders; these defendants are diverted from prison (for example by sentencing them to more rehabilitative-orientated types of punishment such as community service), decreasing the system's punitiveness. This corresponds to the notions of scholars who argue that the welfare/risk binary is overstated because the emergence of the new penology has not simply replaced penal welfarism, but has instead resulted in 'mixed models' and 'hybrid formations' (Hannah-Moffat, 2005) or 'complex and contradictory interweaving' (Field & Nelken, 2010; see also Wandall, 2010) combining risk with welfarism accounts.

New penological discourses – suggesting that risks can be identified and managed – affect the tasks of probation officers who write pre-sentence reports. In the era of the old penology, probation agencies used to act exclusively in the interests of the defendant. Now, they have evolved into output-driven organizations with the objective of assisting judicial authorities by advising the public prosecutor and the judges, and by supervising community services and conditions of suspended sentences, with the interests of *society* at heart. As a consequence, for report writers the focus has shifted from assessing defendants' needs to assessing defendants' risks. To assess this risk, they use a clinical structured risk assessment tool: RISC (Recidivism Assessment Scales).

3.3 PRE-SENTENCE REPORTS IN THE NETHERLANDS

A pre-sentence report is the key piece of information about an individual defendant. It is normally about five pages long, depending on the case and the defendant, and is requested from the probation agency by the prosecutor, although it may also be ordered by the judge. There are no clear rules about which cases require a pre-sentence report but, in general, a report is requested when the defendant is held in preventive custody or when the case demands special attention owing to the severity of the crime or the harm to the victim. Conversely, pre-sentence reports are least common in standard cases – to which most criminal cases belong – such as driving while intoxicated. Furthermore, for practical reasons, no pre-sentence report is requested when the court session

is scheduled to be held within 10 weeks, or when the defendant has already had his/her risk assessed within the last year (Adviesbureau Van Montfoort & Reclassering Nederland, 2004).

Before 2004, pre-sentence reports were based on the professional judgement of the probation officer, without the assistance of a structured, clinical risk assessment tool. The subjects described in the reports depended heavily on the individual probation officer's approach. However, since 2004, the probation agency uses RiSc as the foundation of the pre-sentence report: a structured clinical tool to assess a defendant's risk of reoffending.² The introduction of RiSc as central to the advisory work of the probation agency signifies a much more explicit focus on risk assessment; whereas risk assessment by the probation agency used to be more a clinical and non-standardized assessment of *needs*, RiSc clearly aims at the standardized assessment of *risk*. However, focusing on risk does not mean that needs are neglected: to assess a defendant's risk to society RiSc also touches upon a defendant's needs. But, in contrast to the assessment of needs in the era of penal welfarism, RiSc covers only a defendant's *criminogenic* needs, since these needs are the mirror image of risk.

When assessing risk, the probation officer relies on information about the offense from the police case file, as well as on the criminal record of the defendant. In addition, one or more interviews with the defendant are held, and often the probation officer talks to the defendant's family or employer as well. The probation officer assessing the RiSc maps out the defendant's criminogenic factors categorized into 12 sections, such as accommodation, education and work, relationships, drug or alcohol misuse and thinking and behavior. Each scale contains several items to assess whether the section is a point of risk for reoffending. The (weighted) scale scores together add up to the total RiSc score (see Van der Knaap et al., 2012). Based on this total score, delinquents are categorized as having either a low, medium or high risk of reoffending.

The pre-sentence report for the public prosecutor and the judge is based on this risk assessment and follows the structure of the RiSc, but does not contain the scores on the separate items or scales of the RiSc. Instead, the defendant's overall risk of reoffending is reported explicitly in terms of either a low, medium or high risk of reoffending, while criminogenic issues on the different scales are only narratively described in the report.³

2 RiSc is derived from the Offender Assessment System (OAsys) developed in the United Kingdom (Howard, Clark & Garnham, 2003), which is based on the Canadian instrument Level of Service Inventory – Revised (LSI-R) (Andrews and Bonta, 1995) and on the Assessment Case management and Evaluation System (ACE) (Gibbs, 1999; van der Knaap et al., 2007).

3 Report writers are free to divert from the RiSc-outcome if they feel that defendant's risk is actually lower or higher than RiSc indicated. Prior research shows that deviation takes place in only 4% of the cases, mostly due to defendant's psychological problems or addictions (Van Wingerden, Moerings, & Van Wilsem, 2011).

The conclusion of a pre-sentence report contains the probation officer's evaluation of the defendant's social background, risk-related social circumstances and risk of reoffending, as well as his/her suitability for a suspended sentence, for special conditions accompanying a suspended sentence, or for other punishments requiring the involvement of the probation agency. Hence, it contains important information for judges when making their sentencing decisions: it enables them to fit the punishment to the crime as well as to the defendant.

Judges have broad discretionary powers to do this, because a key feature of the Dutch criminal justice system is that judges' sentencing decisions are constrained only by the Dutch Penal Code, which sets a uniform minimum penalty (for example, imprisonment should last at least one day) and crime-specific maximum penalties (for instance, 4 years for ordinary theft and 12 years for violent theft). The discretionary power of the judge is further broadened by the different sanction types and modalities (suspended or unsuspended) the judge can choose from. These sanction types can be independently or jointly imposed, either unsuspended or (partially) suspended. Examples of sanction types are community services (performing unpaid work for the benefit of society, for example cleaning public areas), fines and – in certain circumstances – additional measures, such as placement in an institution for mentally ill offenders or deprivation of the proceeds of crime. Finally, for suspended punishments, various special conditions can be specified, which have to be met by the offender during the operational period of the suspended sentence, such as alcohol treatment or aggression regulation therapy. The National Consultation on Criminal Content (LOVS, 2013) has provided judges with orientation points for common offenses, but these are non-binding and judges are free to deviate from it. Moreover, judges are not restricted in the sentencing goals they pursue either, since these are not explicated in the Dutch law. Previous research suggests that there is no dominant sentencing goal in Dutch sentencing *practices* either: different judges pursue different sentencing goals (De Keijser, 2001). Hence, especially compared with other countries, Dutch judges enjoy broad discretionary powers: they are at liberty to impose any sentence they want within the boundaries of the law; there are no binding guidelines, nor are there rules for when to take factors into account as mitigating or aggravating circumstances.

3.4 RISK-BASED PRE-SENTENCE REPORTS: FRAMING THE PERSON OF THE OFFENDER

The goal of this study is to investigate the effects of a structured risk-based pre-sentence report on sentencing outcomes. These outcomes are likely to be affected by a defendant's risk of reoffending because, in line with the ideas of the new penology, risk is a key factor for judges who aim to protect society

with the sentences they impose. Since crime is considered to be a risk that needs to be managed and resources are scarce, resources are expected to be employed in the most effective way: to control high-risk defendants and divert low-risk defendants from imprisonment. There are two types of punishment that judges can impose to place high-risk defendants under long-term control. First, imprisonment physically prevents the defendant from committing crime – at least as long as he/she is incarcerated. Second, suspended sentences with special conditions spread a net of control over defendants.⁴ These special conditions might include interventions such as training in lifestyle, cognitive skills or aggression regulation, or treatment of addictions, all under the supervision of the probation agency. Therefore, we assume that judges who want to protect society from future crimes either eliminate high-risk defendants from society by imposing unsuspended imprisonment terms, or place them in a net of control by imposing suspended sentences with special conditions. Low-risk defendants, on the other hand, are more likely to be diverted from imprisonment by sentencing them to non-custodial punishment types such as solely suspended imprisonment or community service.

We expect that a structured risk-based pre-sentence report enhances these effects, because the narrative of the report is likely to create *framing effects*: judges' sentencing decisions are made dependent on how the situation of the defendant is presented, or 'framed', in the pre-sentence reports (Baron, 2008; Isaacs, 2011).⁵ These framing effects are stronger for defendants from the experimental group, since structured risk-based pre-sentence reports 'impart a sense of moral certainty and legitimacy into the classifications they produce' (Hannah-Moffat, 2013, p. 277), framing the defendant as a certain risk. Confirmation bias causes information consistent with the level of risk stated in the pre-sentence report to be overweighted and inconsistent information to be underweighted. Judges are therefore more likely to attribute the risk label to the defendant as well. We therefore expect that the narrative of the pre-sentence report both consciously and unconsciously (through framing and confirmation bias) enhances the 'new penological' mechanisms: high-risk defendants *with* a structured risk-based pre-sentence report are more likely to be sentenced to 'controlling' types of punishment and less likely to be sentenced to 'diverting' types of punishment than comparable high-risk defendants *without* such a report. For low-risk defendants, we expect the opposite effect, since their pre-sentence reports underline the virtuous, non-criminogenic

4 Suspended imprisonment sentences, either with or without special conditions, cannot be imposed when the length of the unsuspended prison term exceeds four years (art. 14c Penal Code).

5 After all, judges make decisions under uncertain, time-pressured conditions that encourage reliance on cognitive shortcuts. These mental shortcuts not only help judges to efficiently judge cases, but may also create cognitive illusions that produce erroneous judgments (Guthrie et al., 2001), for example because important information is overlooked or even ignored by judges (Isaacs, 2011; Ten Velden & De Dreu, 2012).

aspects of defendants' lives, such as being employed or owning a house, thereby framing defendants *explicitly* as having a low risk of causing future harm to society.

As a side-effect of the dispersive impact of 'new penological' mechanisms (increasing the sentencing gap between low-risk and high-risk defendants), differences in sentencing outcomes between low-risk and high-risk defendants are likely to be greater for defendants with a structured risk-based pre-sentence report than for defendants without such a report.

3.5 DATA AND METHOD

3.5.1 Quasi-natural experiment

To study the effects of a structured risk-based pre-sentence report on sentencing outcomes, the sentences of defendants with such a report need to be compared with the sentences of defendants who were tried in the absence of such a report. We make use of a unique opportunity to investigate these effects by employing a quasi-natural experiment. Subjects are not randomly assigned to either the experimental or the control group, but instead the control group is found in a natural setting: we compare the sentencing outcomes of defendants whose risk of reoffending was assessed by the risk assessment tool RISC *before* trial – and who thus have a risk-based pre-sentence report – with those of defendants who had a RISC assessment *after* trial. Hence, we distinguish two groups that differ in the availability of a structured, risk-based pre-sentence report but that both offer detailed offender information because, in the end, all members of the two groups had their risks assessed via RISC. This enables the creation of an experimental group and a control group in which cases can be matched at a detailed level, thereby increasing the potential for valid comparison between the groups.

This is possible because delinquents' risks of reoffending can be assessed at several stages in the criminal justice processes. Often, RISC is used *before* trial to provide a pre-sentence report, but sometimes it is also used *after* trial, either to determine the defendant's reintegration trajectory from prison⁶ or to determine the kind of supervision by the probation agency, insofar as RISC has not been assessed already *before* trial – which frequently occurred during the introduction period of RISC (2005-2009). As such, this transitional period when RISC was gradually introduced into the Dutch penal system, and in which comparable defendants were sentenced either with or without a structured risk-based pre-sentence report, presents a unique opportunity to determine the impact of these reports on judicial decision-making.

6 This is only done when the defendant has at least three to four months of imprisonment left.

3.5.2 Dataset

The current study utilizes a combination of two datasets: the registry of the Public Prosecutor's Office (OM data⁷) and the RISC database of the Dutch Probation Service over the period 2005-2007. The registry of the Public Prosecutor's Office contains information on the prosecution and conviction of defendants. For each criminal case, information is registered on the type of crime and the decisions of the prosecutor and the judges, including the imposed sentences. Next, the RISC-database contains the scores on the separate items as well as the final risk classification of the defendant.

The combination of the verdict date and the date of the RISC assessment makes it possible to indicate whether the RISC was assessed *before* or *after* trial. Of the 30,565 cases with a verdict date, we deleted cases in which the outcome of the RISC was unknown ($N = 6,019$) (generally first-offenders who deny having committed the crime), cases in which defendants had their risk assessed more than once ($N = 1,594$), and cases in which information on the defendant's social circumstances ($N = 527$) or on the verdict ($N = 789$) was missing. Further, we deleted cases in which defendants were acquitted, dismissed or declared guilty while no punishment is imposed ($N = 312$) and cases in which only a fine is imposed ($N = 86$), because these case outcomes are not available to defendants in the control group. This leaves 21,238 cases: 16,318 defendants in the experimental group and 4,920 defendants in the control group. The defendants in the latter group do not have a structured risk-based pre-sentence report, but they might have an 'old style' unstructured needs-based pre-sentence report.⁸ Because the experimental group and the control group are not readily suitable for comparison owing to potential selectivity biases, the next section outlines the matching strategy to secure comparable groups.

7 OM data are obtained from the Research and Documentation Centre (WODC) of the Dutch Ministry of Security and Justice. This Centre cannot be held responsible for the completeness, correctness and use of the data provided.

8 The control group exists of defendants with an unstructured needs-based pre-sentence report 'old style' written for the current or for a prior case. They might also have a different kind of report in their case files, such as a report on the execution of prior sentences (*advies-en maatregelrapportages*). And finally, they might have none of the above mentioned reports in their case files. Since the Dutch Probation Service only registers the presence of pre-sentence reports for prior cases from the year 2002 on, we were not able to distinguish these different subgroups properly in the control group. However, we are certain that they did *not* have a *structured risk-based* pre-sentence report. Hence, for the current research we compare sentencing outcomes for defendants with such a structured risk-based pre-sentence report to those of defendants without such a report.

3.5.3 Matching procedure

Since defendants are not randomly selected to have a pre-sentence report, defendants in the control group are likely to differ in various ways from the defendants with a risk-based pre-sentence report, for example with regard to the severity of the offense and to criminogenic social circumstances. Thus, to make a fair comparison, it is important to select the cases in such a way that defendants with a risk-based pre-sentence report are as similar as possible to defendants without such a report in terms of available offense, case-processing and defendant characteristics.

To make sure the control group mirrors the experimental group, we match individual cases from one group to individual cases from the other group with the purpose of controlling for the differences between the two groups. To match cases, they need to be identical on several characteristics. We employ 10 of these matching criteria.

Our first matching criterion is the score on the propensity to have a structured risk-based pre-sentence report.⁹ Propensity score matching offers a useful analytical approach for establishing equivalency in observed covariates between groups (Johnson & Kurlychek, 2012). It is a practical solution to take many variables at once into account for the matching, because a collection of confounding covariates is replaced by one function of these covariates: the propensity score. This score is then used as if it were the only confounding covariate. It is derived from a logistic regression analysis with a range of observable factors as independent variables, including offense, case-processing and defendant characteristics, and a variable on the timing of the RISC (*before* or *after* trial) as the dependent variable. The results for this regression analysis are presented in Table 3.1. Based on this analysis, the predicted probability of having a structured risk-based pre-sentence report is derived to be each defendant's propensity score.

9 Propensity score matching has long received little attention in the criminological research field, but from 2004 on, it has been employed in several criminological studies (Jones et al., 2004; e.g. Wermink et al., 2010), also with regard to sentencing outcomes (Johnson and Kurlychek, 2012; Kurlychek and Johnson, 2010).

Table 3.1. Results of logistic regression analysis on the chance of having a structured risk-based pre-sentence report (N=21,238 of which 16,318 coded 'RISc before trial' and 4,920 coded 'RISc after trial')

	Chance of RISc assessment before trial		
	B	S.E.	Exp(B)
<i>Constant</i>	3.01 ***	.11	20.20
<i>Offense characteristics</i>			
Maximum penalty	-.05 ***	.01	.95
Offense type of most serious offense (Ref=Assault)			
Intimidation	-.21 *	.09	.81
Violent theft	-.44 ***	.08	.64
Vices	-.57 ***	.09	.56
Homicide	-.27 **	.10	.77
Other violent crimes	-.54 ***	.16	.58
Forgery	-.37 *	.15	.69
Theft	-.72 ***	.09	.49
Aggravated theft	-.48 ***	.07	.62
Other property crimes	-.32 **	.11	.73
Destruction of property	-.36 *	.15	.70
Violation of public order	-.27 **	.08	.76
Drugs	-.44 ***	.08	.64
Traffic	-.16	.14	.85
Other crimes	-.33 ***	.09	.72
Number of offenses	-.03	.02	.97
<i>Case processing characteristics</i>			
Length of preventive custody (in months)	-.06 ***	.01	.94
Number of prior convictions as a minor			
1-2	-.31 ***	.05	.73
3 or more	-.47 ***	.06	.63
Unknown	-.75 ***	.05	.47
Number of prior convictions as an adult			
1-2	-1.06 ***	.05	.35
3 or more	-1.19 ***	.05	.30
Unknown	-.95 **	.36	.39
<i>Offender characteristics</i>			
Sex			
Female	.05	.06	1.05
Age			
Age 12-20	-.62 ***	.05	.54
Age 31-40	.07	.05	1.07
Age 41-50	.18 **	.06	1.20
Age > 50	.37 ***	.08	1.44
Origin			
Western	-.20 **	.07	.82
Non-Western	-.16 **	.05	.86
Origin unknown	.12	.13	1.13

(Continued)

Table 3.1. – Continued

	Chance of RISC assessment before trial		
	B	S.E.	Exp(B)
<i>Offender social circumstances</i>			
Accommodation	.11 *	.04	1.12
Education and employment	-.26 ***	.04	.77
Financial management and income	.10 **	.04	1.11
Relationships with partner, family and relatives	-.06	.04	.94
Relationships with friends	.05	.05	1.05
Drug misuse	.11 **	.04	1.12
Alcohol misuse	.02	.03	1.02
Emotional well-being	-.12 **	.04	.89
Thinking and behavior	.20 **	.07	1.22
Attitude	.04	.05	1.04
Nagelkerke R ²	.17		

NOTE: Model includes dummy variables for unknown maximum penalty, unknown accommodation and for court districts (not presented). Complete results are available from the authors.

* $p < .05$; ** $p < .01$; *** $p < .001$.

Figure 3.1 shows that the majority of defendants with the highest propensity scores indeed had a risk-based pre-sentence report, but also that there is a large amount of overlap between the propensity score distributions of the experimental and control groups. The defendants within the overlapping propensity score area (shaded grey in Figure 3.1) are eligible for matching.

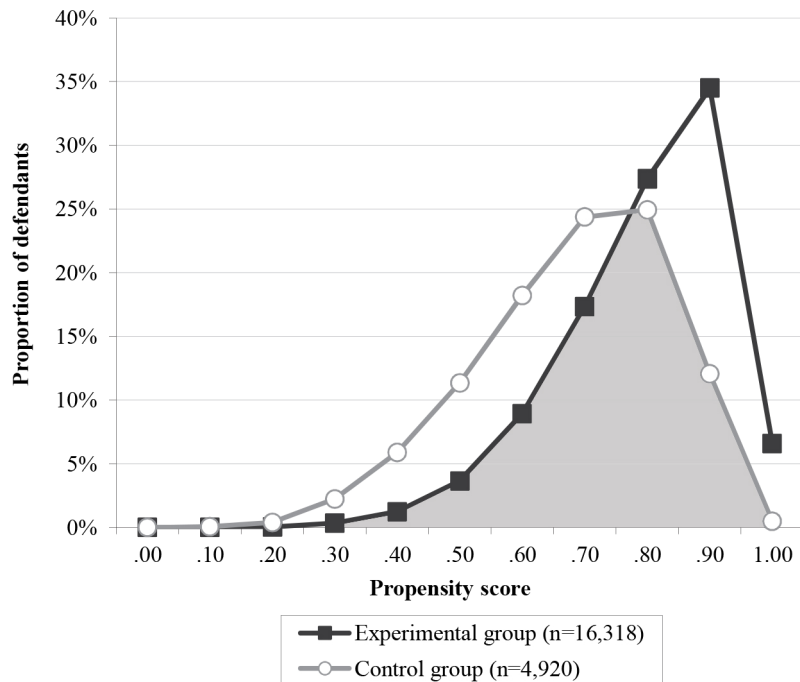


Figure 3.1. Probability densities of estimated scores of the propensity to have the RISC assessed *before* trial

For the matching, we employ a nearest-neighbor matching technique with non-replacement, allowing for a maximum deviation in propensity scores of 0.05. By including the propensity to have a pre-sentence report as a matching criterion, defendants from the control group have the same predisposition to have a pre-sentence report as defendants from the experimental group.

Yet the propensity of a RISC assessment is not the only characteristic relevant for matching defendants. To investigate the effects of a pre-sentence report on sentencing outcomes, characteristics known to be relevant for judicial decision-making also need to be taken into account. These factors include offense, defendant, case-processing and risk characteristics. So, besides the propensity score, we use nine of these characteristics for the matching.

First, the *type of crime* is considered. The type of crime of the most severe offense consists of 16 categories, such as violence, theft, drugs, etc. Furthermore, for cases to be comparable, we take into account whether the offense was an *attempted or a completed crime*. Next, the defendant's *sex* (male or female), *age* (in five categories: 12-20, 21-30, 31-40, 41-50, 50+) and *country of birth* (in four categories: the Netherlands, other Western country, non-Western country, or unknown country of birth) have to be equal for cases to be eligible for matching. The next matching criterion is the defendant's *criminal history*. This is a variable indicating whether the defendant has previous convictions as a youth and/or as an adult (the categories are: none, as a youth, as an adult, both as a youth and as an adult). Besides offense and defendant characteristics, case-processing characteristics are taken into account when matching. We use a variable indicating whether the defendant was taken into *pre-trial detention* (yes or no); and whether he/she was *present at court hearings* (yes, no, or unknown). The final matching criterion is the *outcome of the RISC* (low, medium or high risk of reoffending).

In summary, to match a defendant from the experimental group to one in the control group, they need to be similar in their propensity to have a pre-sentence report (having a difference in propensity score of 0.05 at most), as well as on all nine of the above-mentioned offense, defendant, case-processing and risk characteristics. As such, our requirements for matching a defendant from the control group to one in the experimental group are strict: other studies often use either no additional matching criteria besides the propensity score or only two or three, such as sex and age (for example, Johnson & Kurlychek, 2012; Wermink et al., 2010). Moreover, we employ very specific measures for the offense characteristics: we distinguish 16 different offense types and also take into account whether the crime was attempted or completed, whereas categorizations in most previous research are far less detailed. Our in-depth procedure guarantees a more precise matching of cases from the experimental and the control groups, thereby increasing the comparability of cases from the two groups. Despite our strict requirements, we were able to match 3,059 of the 4,920 defendants from the control group (62 percent)

to a suspect in the experimental group. Our sample therefore consists of 6,118 (2 x 3,059) defendants.

3.5.4 Balancing tests

To test whether the experimental and the control group are comparable, we analyze the differences between both groups on several offense, case-processing and defendant characteristics. Covariate balance checks are conducted on the sample both *before* and *after* the matching. Results are presented in Table 3.2. First, means are reported for the experimental and the control group *before* matching took place. Two-sample *t*-tests show that there are many significant differences between the two groups. For example, defendants from the control group have committed more offenses and more serious ones, they are younger and they have more previous convictions.

Besides the *t*-test, another measure to assess the initial covariate imbalance between the experimental and the control groups is the standardized difference as a percentage (*D*) (Rosenbaum & Rubin, 1985), also known as the standardized bias statistic (SBS). This is the difference in sample means as a percentage of the average standard deviation (Rosenbaum & Rubin, 1985).¹⁰ According to Rosenbaum and Rubin, a *D*-value greater than 20 indicates that the two groups are out of balance. However, more recent scholarship suggests that values above 10 are problematic (Johnson & Kurlychek, 2012). As Table 3.2 shows, some characteristics have a *D*-value greater than 20, such as the severity of the offense, the length of the pre-trial detention and previous convictions, and several others have a *D*-value greater than 10. In conclusion, before the matching the control group differs significantly from the experimental group.

Table 3.2 also shows the differences between these groups *after matching*. Clearly, the experimental group and the control group are very similar now. *T*-tests show significant differences for only one variable, a defendant's housing situation, where the mean score of the experimental group is slightly higher than that of the control group (0.30 vs. 0.27, $p = .02$). Moreover, for all variables, *D*-values are below the critical value of 10, and for none of the characteristics do *D*-values exceed 6.

10 The formula for the standardized difference in percent is:

$$D = 100 \cdot \left(\frac{\bar{X}_{\text{experimental group}} - \bar{X}_{\text{control group}}}{\sqrt{\frac{S_{\text{experimental group}}^2 + S_{\text{control group}}^2}{2}}} \right)$$

where for each covariate, $\bar{X}_{\text{experimental group}}$ is the sample means for the defendants with a risk-based pre-sentence report, $\bar{X}_{\text{control group}}$ is the sample means for the defendants from the control group, and $S_{\text{experimental group}}$ and $S_{\text{control group}}$ are the corresponding standard deviations (see Rosenbaum and Rubin, 1985).

After matching, the two groups thus have highly similar characteristics. Since these offense, case-processing and defendant characteristics are similar for both groups, and the groups differ only in the experimental condition, we can be more confident that potential differences in sentencing outcomes are the result of the presence or absence of a structured risk-based pre-sentence report at sentencing.

Table 3.2. Differences between the experimental and the control group before and after matching

	Before matching				After matching			
	Means		Differences in covariate means		Means		Differences in covariate means	
	Exp. group	Contr. group	Absolute difference	<i>t</i>	Exp. group	Contr. group	Absolute difference	<i>t</i>
<i>Offense characteristics</i>								
Maximum penalty	5.77	7.30	1.53	-22.95 ***	6.71	6.80	.09	-.86
Offense type								
Assault	.26	.15	-.11	15.95 ***	.20	.20	.00	.00
Intimidation	.07	.04	-.02	5.62 ***	.05	.05	.00	.00
Violent theft	.07	.13	.06	-13.88 ***	.13	.13	.00	.00
Vices	.06	.06	.01	-1.44	.06	.06	.00	.00
Homicide	.05	.08	.03	-8.45 ***	.06	.06	.00	.00
Other violent crimes	.01	.01	.01	-3.11 **	.01	.01	.00	.00
Forgery	.02	.02	.00	.78	.00	.00	.00	.00
Theft	.05	.06	.02	-4.35 ***	.06	.06	.00	.00
Aggravated theft	.10	.14	.04	-7.53 ***	.15	.15	.00	.00
Other property crimes	.03	.03	-.01	2.50 *	.02	.02	.00	.00
Destruction of property	.02	.01	.00	1.84	.01	.01	.00	.00
Violation of public order	.07	.07	.00	-.32	.07	.07	.00	.00
Drugs	.09	.11	.02	-4.73 ***	.12	.12	.00	.00
Traffic	.04	.02	-.02	6.78 ***	.02	.02	.00	.00
Other crimes of the Penal Code	.07	.06	-.02	4.68 ***	.05	.05	.00	.00
Number of crimes	1.72	1.86	.13	-9.69 ***	1.87	1.88	.01	-.40
<i>Case processing characteristics</i>								
Length of preventive custody (months)	1.74	2.82	1.09	-24.61 ***	2.60	2.54	-.05	.72
Number of prior convictions as a minor								
0	.59	.40	-.19	23.25 ***	.45	.45	.00	-.10
1-2	.15	.21	.06	-9.53 ***	.20	.20	.00	.48
3 or more	.09	.16	.07	-14.53 ***	.15	.15	.00	-5.4
Unknown	.18	.24	.06	-9.03 ***	.20	.20	.00	.13
Number of prior convictions as an adult								
0	.41	.20	-.21	27.18 ***	.20	.20	.00	.00
1-2	.23	.31	.08	-11.62 ***	.31	.29	-.02	1.29
3 or more	.35	.48	.13	-16.05 ***	.49	.51	.02	-1.18
Unknown	.00	.00	.00	-1.78	.00	.00	.00	.00

(Continued)

Table 3.2. - Continued

	<i>Before matching</i>				<i>After matching</i>			
	Means Exp. group	Contr. group	Absolute difference	<i>t</i>	Means Exp. group	Contr. group	Absolute difference	<i>t</i>
<i>Defendant characteristics</i>								
Sex								
Female	.10	.09	-.01	2.85 **	.04	.04	.00	.00
Male	.90	.91	.01	-2.85 **	.96	.96	.00	.00
Age								
Age 12-20	.17	.26	.09	-14.51 ***	.21	.21	.00	.00
Age 21-30	.29	.31	.02	-2.23 *	.33	.33	.00	.00
Age 31-40	.27	.25	-.02	2.74 **	.27	.27	.00	.00
Age 41-50	.18	.14	-.05	7.85 ***	.14	.14	.00	.00
Age > 50	.09	.05	-.04	9.17 ***	.05	.05	.00	.00
Country of birth								
Netherlands	.75	.70	-.05	6.86 ***	.79	.79	.00	.00
Western	.07	.09	.02	-5.67 ***	.05	.05	.00	.00
Non-Western	.16	.19	.03	-4.94 ***	.15	.15	.00	.00
Origin unknown	.02	.02	.00	2.03 *	.01	.01	.00	.00
<i>Defendant social circumstances</i>								
Accommodation	.25	.28	.03	-3.65 ***	.30	.27	-.03	2.37 *
Accommodation unknown	.12	.18	.05	-9.29 ***	.18	.18	.00	.17
Education and employment	.64	.80	.16	-16.89 ***	.76	.77	.01	-.49
Financial management and income	.54	.62	.09	-9.05 ***	.65	.63	-.03	1.72
Relationships with friends	.47	.59	.12	-15.21 ***	.59	.58	-.01	.74
Drug misuse	.39	.47	.09	-9.45 ***	.49	.49	-.01	.39
Alcohol misuse	.49	.47	-.02	1.90	.52	.50	-.03	1.68
Emotional well-being	.65	.68	.03	-2.97 **	.67	.66	.00	.33
Thinking and behavior	.82	.86	.04	-5.66 ***	.87	.87	.00	.00
Attitude	.59	.64	.05	-6.10 ***	.64	.64	.00	-.33

* $p < .05$; ** $p < .01$; *** $p < .001$.

3.5.5 Analytic approach

To investigate whether the presence of a structured risk-based pre-sentence report enhances risk-based sentencing (that is, ‘controlling’ of high-risk defendants and ‘diversion’ of low-risk defendants), we assess whether high-risk offenders with a structured risk-based pre-sentence report are sentenced to more ‘controlling’ and less ‘diverting’ punishments compared with (very similar) high-risk offenders from the control group, and whether the reverse pattern can be found for low-risk offenders. We do so by comparing the sentencing outcomes of the 3,059 defendants with a structured risk-based pre-sentence report with those of the 3,059 defendants without such a report. Regarding the ‘controlling’ types of punishment, we focus on the decision to impose incarceration (imprisonment, youth detention or placement in an institution for habitual offenders), as well as on the decision to impose a suspended sentence with special conditions. Regarding the ‘diverting’ types of punishment, we focus on the decision to impose solely suspended sentences without special conditions¹¹ and on the decision to impose community service.¹² For the defendants who were sentenced to imprisonment, we also investigate the decision about the length of the unsuspended prison sentence (in days).¹³ Considering the skewed nature of prison sentence lengths, we use (non-parametric) Mann–Whitney U-tests. Thereafter we examine whether differences in sentencing outcomes between low-risk and high-risk defendants are larger for defendants with a structured risk-based pre-sentence report than for defendants without such a report.

3.6 RESULTS

3.6.1 Type of punishment

To assess the effects of a structured risk-based pre-sentence report on sentencing outcomes, we compare sentencing outcomes for low-risk defendants from the experimental group with low-risk defendants from the control group, and

11 The defendants sentenced to *solely* suspended imprisonment without special conditions are thus not sentenced to unsuspended incarceration as well. Since no special conditions are imposed, these defendants only have to meet the general condition that they will not commit another crime during their probationary period. When they breach this condition, they have to serve the suspended term in prison.

12 Defendants sentenced to community service might be sentenced to other sentence types as well. We also analyzed ‘solely’ community service (not combined with detention). Results show similar patterns (not presented).

13 Defendants sentenced to youth detention or to placement in an institution for habitual offender are thus excluded for the analyses on sentence length, since both types of sanctions have a maximum length of two years, while imprisonment has a maximum length of 30 years or life.

vice versa for high-risk defendants. Table 3.3 demonstrates, in line with our expectations, that low-risk defendants from the experimental group are *less likely* than low-risk defendants from the control group to be sentenced to 'controlling' types of punishment: 42 percent of the experimental group is incarcerated compared with 49 percent of the control group ($\chi^2(1) = 8.35$, $p < .01$). For suspended sentences with special conditions, these percentages are, respectively, 41 and 75 ($\chi^2(1) = 187.93$, $p < .001$). Moreover, as expected, low-risk defendants with a structured pre-sentence report are *more* likely to be sentenced to 'diverting' types of punishment. Of the experimental group, 16 percent are sentenced to solely suspended imprisonment, compared with 5 percent of the control group ($\chi^2(1) = 47.04$, $p < .001$). For community service, these percentages are 57 and 52, respectively ($\chi^2(1) = 3.92$, $p < .05$).

However, for high-risk defendants, our findings are not consistent with predictions from a 'new penological' perspective. Contrary to our expectations, the chance of being incarcerated does not differ significantly from the experimental to the control group ($\chi^2(1) = 2.08$, n.s.), and high-risk defendants from the experimental group are not more but *less* likely than high-risk defendants from the control group to be sentenced to special conditions with a suspended sentence (respectively 45 percent and 61 percent; $\chi^2(1) = 32.63$, $p < .001$). Moreover, regarding the 'diverting' types of punishment, high-risk defendants from the experimental group are not less but *more* likely than high-risk defendants from the control group to be sentenced to suspended sentences without special conditions (respectively 3 percent and 0 percent; $\chi^2(1) = 13.76$, $p < .001$). The chance of being sentenced to community service does not differ significantly for high-risk defendants from the experimental and control groups ($\chi^2(1) = 0.01$, n.s.).

As a side-effect of the dispersive 'new penological' mechanisms for low-risk and high-risk defendants, we expect sentencing disparities between low-risk and high-risk defendants to be greater for defendants with a structured risk-based pre-sentence report than for the control group. Overall, risk-based sentencing disparity appears to be somewhat larger in the experimental group, at least as far as detention, solely suspended sentences and community service are concerned. This can mainly be ascribed to low-risk defendants who are even more often steered away from 'controlling' types of punishment (detention) to 'diverting' types of punishment (solely suspended sentence or community service) than low-risk defendants from the control group. To illustrate, community service was assigned to 52 percent of the defendants in the low-risk control condition and to 16 percent of the defendants in the high-risk control condition: a difference of 36 percentage points. In the experimental condition, these percentages were 57 and 16, respectively: a difference of 41 percentage points. 'Controlling' types of punishment involving special conditions proved to be an exception to this rule: differences between high-risk and low-risk offenders seem to be smaller in the experimental group than in the control group.

3.6.2 Sentence length

Next, for the 1,748 defendants from the experimental group and 1,711 defendants from the control group who were sentenced to unsuspended imprisonment, we investigate the differences in average length of the unsuspended prison term within the levels of risk. Table 3.4 demonstrates, as expected, that low-risk defendants from the experimental group are on average sentenced to significantly *shorter* prison terms than are low-risk defendants from the control group (376 compared with 494 days, a difference of 119 days; medians differ by 120 days, with lengths of 300 and 180 days respectively; $U = 46,358$, $p < .01$). However, contrary to predictions from a 'new penological' perspective, sentence lengths for high-risk defendants do not differ significantly between the experimental and the control group ($U = 98,673$, n.s.).

To assess whether the sentencing disparity between low-risk and high-risk defendants is greater for the experimental than for the control group – as expected because of dispersive 'new penological' mechanisms – we compare sentencing differences within the experimental group with those within the control group. Notably, the average prison term is longer for low-risk than for high-risk defendants. Additional analyses (not presented) point out that high-risk defendants are more often sentenced to prison for relatively minor crimes (for example, theft) involving short durations of imprisonment, as compared with low-risk defendants, who are more frequently imprisoned for more severe crimes (for example, certain sexual offenses). Differences in sentence length between low-risk and high-risk defendants are smaller in the experimental group (a difference of 41 days, with equal median sentence lengths), than in the control group (a difference of 98 days in mean length and 120 days in median length). Similar to the disparity we found in type of punishment, the larger risk-based disparity in sentence length for the experimental group can be mainly ascribed to more lenient punishment for low-risk defendants. We conclude that, contrary to 'new penological' expectations, sentencing disparities between low-risk and high-risk defendants are not enlarged when a structured risk-based pre-sentence report is present, but instead diminished.

3.7 DISCUSSION AND CONCLUSION

Over recent decades, the emergence of the risk society and of the corresponding actuarial justice has gained much attention in academic research and debate. These 'new penological' discourses suggest that crime is considered a risk that needs to be managed (Feeley & Simon, 1992). Hannah-Moffat (2013, p. 271) states that punishment is progressively 'being viewed through the lens of actuarial probability' and that 'the introduction of risk into sentencing is an increasing international trend'. In the Netherlands, the risk assessment tool

Table 3.3. Chance of being sentenced to 'controlling' and to 'diverting' types of punishment for the experimental and the control group by risk of reoffending

	<i>n</i>	Exp. group	Control group	χ^2 (Df 1)
'Controlling' types of punishment				
<i>Detention</i>				
Low risk	784	42%	49%	8.35 **
Medium risk	1,635	62%	66%	6.35 *
High risk	640	82%	85%	2.08
Total	3,059	61%	66%	14.85 ***
<i>Special conditions</i>				
Low risk	784	41%	75%	187.93 ***
Medium risk	1,635	54%	70%	94.41 ***
High risk	640	45%	61%	32.63 ***
Total	3,059	48%	69%	276.65 ***
'Diverting' types of punishment				
<i>Solely suspended sentence without special conditions</i>				
Low risk	784	16%	5%	47.04 ***
Medium risk	1,635	8%	2%	45.83 ***
High risk	640	3%	0%	13.76 ***
Total	3,059	9%	3%	103.73 ***
<i>Community service</i>				
Low risk	784	57%	52%	3.92 *
Medium risk	1,635	39%	36%	3.27
High risk	640	16%	16%	0.01
Total	3,059	39%	36%	5.41 *

NOTE: the *n* of the experimental group is equal to the *n* of the control group. Punishment types are not mutually exclusive: defendants can be sentenced to multiple punishment types.

* $p < .05$; ** $p < .01$; *** $p < .001$.

Table 3.4. Mean length of imprisonment in days for low, medium and high-risk defendants

Experimental group				Control group			Mean	
	n	Mean	Median	n	Mean	Median	Diff.	U
<i>Risk of reoffending</i>								
Low	321	376	180	328	494	300	-119	46,358 **
Medium	986	362	180	923	470	300	-107	400,659***
High	441	335	180	460	396	180	-62	98,673
Total	1,748	358	180	1,711	455	252	-97	1,352,515 ***

NOTE: * $p < .05$; ** $p < .01$; *** $p < .001$

RISC is a textbook example of the emergence of risk assessment in criminal justice practices, because the RISC assessment is used as the foundation of the pre-sentence report. However, to date, the effects of risk-based pre-sentence reports on judicial decision-making are unknown.

The purpose of this paper therefore was to explore the effects of a structured risk-based pre-sentence report on sentencing outcomes. Drawing on a unique large-scale dataset ($N = 6,118$), we compare sentencing outcomes for Dutch defendants with a structured risk-based pre-sentence report with similar defendants without such a report. Each defendant in the 'experimental' condition (with such a report) was carefully matched to a defendant in the control condition, by means of propensity score matching and nine additional (defendant and case) characteristics. In line with the notions of the new penology (Feeley & Simon, 1992, 1994), we expected that – owing to framing effects and confirmation bias – risk-based sentencing would be enhanced by the presence of a structured risk-based pre-sentence report: sentencing outcomes for high-risk defendants with such a report are more 'controlling' (incarceration or suspended sentences with special conditions) and less 'diverting' (solely suspended sentences without special conditions or community service) compared with high-risk defendants without such a report. For low-risk defendants, we expected the opposite effects.

The empirical support for these expectations was mixed. Consistent with our expectations, low-risk defendants with a structured risk-based pre-sentence report are indeed less likely than low-risk defendants without such a report to be sentenced to 'controlling' types of punishment and more likely to be sentenced to 'diverting' types of punishment. Moreover, they receive shorter prison terms. However, our findings for high-risk defendants conflict with 'new penological' expectations: high-risk defendants with a structured risk-based pre-sentence report are *not* more likely than those without such a report to be sentenced to 'controlling' types of punishment; the chances of incarceration do not differ for high-risk defendants from the experimental group and the control group, and high-risk defendants with such a report are not more but *less likely* to be sentenced to suspended sentences with special conditions. In addition, the length of the prison term does not differ significantly for the high-risk experimental group and the high-risk control group. Furthermore, high-risk defendants with a structured risk-based pre-sentence report are *not* less likely than high-risk defendants without such a report to be sentenced to 'diverting' types of punishment. Instead, they are *more* likely to be sentenced to a solely suspended sentence without special conditions, and their chances of being sentenced to community service do not differ significantly from the high-risk control group, nor does their sentence length. Hence, the presence of a structured risk-based pre-sentence report does not increase the chances of high-risk defendants being sentenced to 'controlling' types of punishment, nor does it decrease their chances of being sentenced to 'diverting' types of punishment. In general, therefore, sentencing outcomes for defendants with

a structured risk-based pre-sentence report are less ‘controlling’ and more ‘diverting’ than for defendants without such a report.

A possible explanation is that a defendant’s personal circumstances, structurally presented in the pre-sentence report, are often not considered as aggravating factors, indicating the need to protect society by imposing ‘controlling’ types of punishment, but rather as mitigating factors, indicating possibilities for rehabilitation of the defendant (Mathiesen, 1998; Moerings, 2003). Differences in sentencing outcomes between defendants with and without a structured risk-based pre-sentence report might then be explained by an information effect: judges who do not have a sound grasp of defendants’ personal circumstances cannot take these into account as mitigating factors.

An important question is whether there are other factors that can explain the differences in sentencing outcomes for the experimental and the control groups. Differences in punishment might occur when the experimental and the control groups differ from each other on features relevant to penal decision-making that are not accounted for in this study. However, our matching criteria were very extensive and, as our balancing results in Table 3.2 showed, defendants from the experimental group are very similar to defendants from the control group. Nevertheless, differences in sentencing outcomes might be caused by omitted variables, such as the quality of the lawyer or the content of the criminal record (for example, the number of violent offenses). Yet we have difficulty explaining why these omitted variables would be less or more prevalent in the experimental group than in the control group. We therefore do not find it plausible that these variables cause the differences in sentencing outcomes. We can thus think of no other reason for the less ‘controlling’ and more ‘diverting’ types of punishment for defendants with a risk-based pre-sentence report than that judges (either consciously or unconsciously) take the structured content about the presented criminogenic circumstances of the defendant into account as mitigating factors, indicating an increased potential for rehabilitative efforts.

In conclusion, the current study expands the scope of contemporary sentencing research to the under-studied role of pre-sentence reports at sentencing. Our findings suggest that a pre-sentence report based on a structured clinical risk assessment tool – a hallmark of risk managerialism in the new penology – does not enhance risk-based sentencing in the Netherlands: such a report does not increase the chances of high-risk defendants being sentenced to ‘controlling’ types of punishment, nor does it decrease their chances of being sentenced to ‘diverting’ types of punishment. Instead, a structured risk-based pre-sentence report informing the judge of criminogenic factors in a defendant’s life is linked to less ‘controlling’ and more ‘diverting’ sentencing outcomes.

Therefore we conclude that the penal welfarism account is still prevalent in Dutch judicial decision-making. This corresponds to Field and Nelken’s (2010) observation that old welfarism discourses are not being replaced by new penological discourses, but instead have resulted in new complex and

contradictory interweaving (see also Wandall, 2010). Future research is needed that further investigates the underlying theoretical processes that lead to the 'diverting' effects of risk-based pre-sentence reports. To unravel these theoretical processes, future research could also benefit from studying the effects of pre-sentence reports in other national contexts.

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