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Victim and Offender Ratings of Mediations and Restorative Justice Conferences: Findings from a Danish Randomized Controlled Trial

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ABSTRACT

This article presents initial survey findings from the first randomized controlled trial to compare two forms of restorative justice. Cases referred to the Danish police's restorative justice program (*konfliktråd*) were randomly allocated to either victim-offender mediation ($n = 101$) or a restorative justice conference ($n = 99$), and follow-up surveys were sent to victims and offenders 1 and 6 months after all convened meetings. Based on responses from 135 victims and 121 offenders, we explore group differences for four measures capturing participants' overall assessments of the fairness of their meeting, the extent to which it repaired harm, helped them move on, and was an overall success. Analyses revealed generally positive ratings for all outcomes and both interventions, with some decreases observed over time. For offenders, no significant differences emerged between the experimental conditions for any outcomes, whereas victims assessed mediations significantly more positively than conferences for overall success and harm repair at 6 months. While these findings fail to support our theoretical hypothesis that conferences would be rated more favorably for all outcomes, respondents' positive assessments of both meeting types generally support the use of both mediation and conferencing methods, offering new insights for future research and practice.

KEYWORDS

Restorative justice; victim-offender mediation; conferencing; harm; fairness; satisfaction; randomized controlled trial; Konfliktråd Impact Project

Introduction

Restorative justice practices that bring crime victims and offenders together for a dialogue about the crime and what can be done to repair the harm, are increasingly being used around the world (Dünkel et al., 2015; Gavrielides, 2021; McCold, 2006). These practices, which can be diversionary or supplementary to traditional criminal justice systems, are broadly defined as processes "...whereby all the parties with a stake in a particular offense come together to resolve collectively how to deal with the aftermath of the offense and its implications for the future" (Marshall, 1996, p. 37). While still somewhat marginalized, such

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approaches have gained political traction in recent years, as evidenced by new international policy instruments encouraging wider access to high-quality restorative justice services (see e.g., Council of Europe, 2021).

However, identifying practices that reasonably fall under the “restorative justice” umbrella is contested, and the processes labeled as such can take quite varied forms (Gade, 2018; Storgaard, 2015; Wood & Suzuki, 2016). As Ashworth (2001, p. 347) has noted, “vessels of widely differing shapes, sizes, and modes of propulsion sail under this particular flag.” Most commonly, these include victim-offender mediation (VOM), restorative justice conferences (RJC), and circles (McCold, 2006; UNODC, 2020, pp. 24–36). Both mediations and conferences, the types of meetings we focus on in this article, include facilitated dialogue and typically address what has happened, how the meeting participants have been affected, and what should be done to move forward. However, while mediations often include only victims and offenders, conferences tend to extend further to include victims’ and offenders’ support networks in the meeting. Furthermore, in contrast to mediations, conference facilitation typically follows a “script” and has a greater focus on developing a written, future-oriented outcome agreement in the final step of the meeting (for a discussion of the core design features of RJs and mediations see Shapland, 2012; Strang, 2012).

Recognizing that definitional boundaries can be fuzzy and that not all mediations and conferences take these forms, varied practice models clearly exist. Historically, different restorative traditions have developed geographically, such that mediation is more standard in the European context and conferencing more common in e.g., Australia and New Zealand (Zinsstag, 2012). However, the global proliferation of restorative justice means that, today, many different methods operate side by side, even within a single setting. This variety complicates both definition, evaluation, and theoretical development in the field, as it remains somewhat unclear how different methods vary in practice, which approaches should be promoted, and on what grounds. Some scholars suggest that focus should be on manifesting shared restorative principles such as human dignity, empowerment, accountability, and inclusion *across* methods, rather than promoting specific process models (Bazemore & Schiff, 2005; Chapman et al., 2021). Others have called for more transparency regarding methodological choices and variations, urging systematic explorations of the respective effects of different approaches (Walgrave, 2012). To ensure the best possible experience and outcomes for all victims and offenders offered restorative justice services, it is our position that robust empirical evidence should guide the development, implementation, and improvement of these processes. Generating such evidence is crucial, both to informing local practice and policy, and to more general theoretical discussions in the field. As Shapland (2012, p. 48) has argued, “if we want to know what works well or works well for the particular population it serves, we need to start unpacking those terms and working out what is or is not different between them, and the extent to which it matters.”

As we elaborate later, empirical research describing and directly comparing different restorative justice practices is lacking. In this paper, we contribute the first-ever evidence from a randomized controlled trial comparing outcomes for two face-to-face methods: victim-offender mediation (VOM) and restorative justice conferences (RJC). Specifically, drawing on surveys distributed to victims and offenders whose cases were randomized to receive either a mediation or a conference in the Danish police’s victim-offender program (*konfliktråd*), we explore how participants rate the overall success of these meetings, as well

as the extent to which the process was perceived as fair, helped the parties to move on, and repaired the harm associated with the offense. To contextualize this work, we start with a brief review of relevant theory and research on different restorative justice practices and outline the design and implementation of the randomized trial.

Are some methods more “restorative” than others?

Although rooted in different historical, cultural, and political contexts, many restorative justice supporters argue that regardless of the specific approach adopted, core values like procedural fairness, inclusion, participation, dialogue, accountability, redress, and safety are central to good practice (Aertsen, 2024; Chapman et al., 2021). However, some methods have either explicitly or implicitly been framed in previous literature as potentially better equipped to fulfill such restorative ideals. Specifically, several scholars suggest that more inclusive methods, such as RJC, may be particularly well positioned to repair past damage and prevent future harm by more systematically including supporters, posing standard questions to all participants, and/or actively working with forward-looking restorative agreements (e.g., Kyvsgaard et al., 2018; O’Mahoney & Doak, 2017, p. 184; Shapland, 2012, p. 55; Shapland et al., 2007, p. 5; Sherman et al., 2015; Strang, 2012). In relation to the self-reported outcomes explored in this article, we limit our current discussion to how participants’ assessments of mediations and conferences (when implemented according to design ideals) may be theorized to diverge with respect to overall ratings of success, perceived fairness, harm repair, and capacity to move on. While theory may also suggest outcome differences in other areas, we concentrate on these commonly noted goals of restorative justice, as they were measured as key overall indicators in our evaluation.

First, with respect to assessments of fairness, conference processes that actively engage a broader circle of support and systematically give voice to a greater number of parties affected by the crime, might be hypothesized to be perceived as fairer by participants than less inclusive mediation processes. The RJC model tested in this study and several other jurisdictions makes use of a three-staged “script” aimed at ensuring that all meeting participants, including supporters, are given equal opportunity to share their view of what happened, how they were affected, and how the harm can be repaired going forward (McDonald, 2012). When reflecting design ideals of inclusion and impartiality, such meetings may have the potential to promote an increased sense of fairness for both victims and offenders by fostering procedural justice through fair decision-making procedures (Tyler, 2003). Additionally, they may increase participants’ sense of interactional justice – the fairness of the information and interpersonal treatment they receive during the process (Bies, 2005; Scheuerman et al., 2021). Although the number of supporters participating in actual restorative justice conferences rarely lives up to the theoretical ideal (that all those affected should participate), and some may even hinder the process (Barnes et al., 2015; Kenny & Clairmont, 2009; Prichard, 2002), there is ample empirical evidence that supporters typically play a productive role in the dialogue and can serve to moderate power imbalances and domination by single participants (Shapland et al., 2007; Strang, 2002).

Second, by consistently working with a structured “script” and concrete written restorative outcome agreements, conferences might be in a better position than mediations to identify and repair harm. In addition to offenders being presented with the personal consequences of their actions for the victim – which would be expected to occur in both

mediations and conferences – RJC^s additionally aim to systematically outline any harms experienced by the participating supporters of both victims and offenders. To the extent that this results in a more thorough identification of the harms resulting from the offense, RJC^s may be better able to ensure that all relevant damages are addressed and repaired to the extent possible. In addition, previous research suggests that most RJC^s produce a written account of meeting participants' agreements on how best to repair past harm and avoid future damage, while victim-offender mediations in, for example, Denmark and England, more often rely on potentially less obligating oral agreements or may not include any agreements at all (Kyvsgaard, 2016; Shapland et al., 2011). As Chapman (2012, p. 79) has noted, "a commitment to an agreed course of action is strengthened if it is made to the person who you have harmed and in the presence of people who are significant to you." Confirming this commitment in writing might be expected to further promote offenders' accountability to repair harm (O'Mahoney & Doak, 2017).

Third, engaging the main parties' "micro-communities" (Braithwaite, 1993) or "communities of care" (McCald, 2004) in future-oriented conference dialogs may more readily help RJC participants to move on after the meeting, especially if their relationships with these supporters are positive and close (Scheuerman & Keith, 2015; Suzuki, 2023). Ideally, victim supporters would be expected to contribute to a sense of safety during and after the meeting for those harmed by the crime, enabling closure. Meanwhile, offender supporters have the potential to act as a source of informal social control and capital (Cullen, 1994; Warner et al., 2010), helping offenders to make good (Maruna, 2000), reintegrate (Braithwaite, 1989), and potentially desist (Robinson & Shapland, 2008). Empirically, there is evidence that closure for victims can also be promoted by contributing to positive outcomes for the offender (Shapland et al., 2011) through a process that may be facilitated by shared feelings of "collective effervescence" (Collins, 2004) and consensus decision-making when drafting forward-looking RJC^s agreements. Some descriptive research from the UK suggests that whereas discussion of the future is a central topic in most RJC^s, there is greater variability in whether this is addressed in mediations (Shapland et al., 2007, p. 3), further supporting the hypothesis that RJC^s may be especially well-equipped to help the involved parties move on.

It is important to emphasize that what we outline here are *theoretical* predictions based on the framing of mediations and RJC^s found in some extant literature, and not a claim that one method is, *in fact*, experienced as more restorative or successful than another. It is the lack of precisely such comparative evidence that motivates this article.

Victim and offender ratings of restorative justice

A substantial body of international work conducted in the UK, Australia, and North America indicates that victims and offenders are generally very satisfied with their participation in varied restorative justice processes and find them beneficial, whether offered as a diversion or supplement to traditional criminal justice procedures (for reviews see e.g., Hansen & Umbreit, 2018; Islam et al., 2023; Latimer et al., 2005; Nascimento et al., 2023; Shapland et al., 2011; Strang et al., 2013; Umbreit et al., 2001). Even so, a small minority of both victims and offenders report negative experiences, which are typically attributed to poorly planned or implemented meetings, resulting in insufficient preparation of participants, unfulfilled expectations, and/or

lack of agreed follow-up (Choi et al., 2012; Daly, 2003; Shapland et al., 2011, 2024; Strang, 2002). Other research points to the possibility of negative interpersonal dynamics developing in some restorative justice processes, depending on combinations of who is present, the gender of the offender, and the nature of the perpetrated harm. For example, violent offenders may experience more disrespect than offenders of less serious crimes, parents may dominate or undermine the active participation of their children or be shamed themselves, and stereotypes may negatively affect participants who violate gender norms (see Barnes et al., 2015; Kenny & Clairmont, 2009; Prichard, 2002; Scheuerman & Keith, 2015, 2023).

While less published research has systematically explored victims' and offenders' experiences of restorative justice in mainland Europe, similar patterns appear to emerge here (see Dünkel et al., 2015; Nascimento et al., 2023). In the Danish context specifically, two commissioned reports (Hansen, 2012; Henriksen, 2003) indicate high levels of satisfaction amongst victims and offenders participating in both an early pilot and later formalization of the victim-offender *konfliktråd* program, which was established as a nationwide and permanent restorative justice offering in 2010.¹ Like in many other European countries, this scheme primarily employs a mediation, as opposed to RJC, model and operates as a supplement, rather than alternative, to traditional criminal justice processing. In Denmark, victim-offender meetings are organized by the police but carried out by lay mediators, can be held at any stage in the criminal justice process, and are available to both juvenile and adult offenders across a wide range of crime types, including serious violence (for further background about the Danish context, see Kyvsgaard, 2016, Kyvsgaard & Øland Ribe, 2018; Rambøll, 2006; Rasmussen, 2020, 2024; Sandbye et al., 2023; Storgaard, 2015; Vindeløv, 2012). In the 2012 report, 85% percent of victims and 90% of offenders rated their mediations as "very successful" or "successful" immediately after the meeting (Hansen, pp. 86–90). However, some critiques of these meetings have also been raised. For example, based on observations of 12 mediations and subsequent interviews with all parties, Asmussen (2015) found that some participants felt pressured to perform stereotypical roles of the remorseful offender/forgiving victim, which was detrimental to their experience of the process. A Danish governmental report that failed to identify a crime prevention effect of offenders' participation in the mediation program has also prompted calls for practice developments, possibly in the direction of more inclusive RJC methods (Kyvsgaard, 2016; Kyvsgaard & Øland Ribe, 2018).

While the evidence reviewed above generally paints a positive picture of participants' ratings of their restorative justice experiences in Denmark and abroad, this is not exclusively the case and more work is needed to identify the factors that predict positive outcomes, including whether particular practice models generate better self-reported outcomes for victims and offenders as suggested above. Although some non-experimental attempts have been made to compare different approaches and schemes, such contrasts are inadequate because varied practices are often anchored in – and therefore confounded by – local cultural and organizational contexts, where selection bias can also play a role (Bouffard et al., 2017; Shapland, 2012, p. 47). Isolating the effects of specific restorative justice approaches thus requires a randomized controlled test of different methods implemented in a shared setting. The Konfliktråd Impact Project (KIP) is the first to do this.

The current study

KIP is a collaborative development and evaluation project led by academic researchers and anchored in the Danish police's victim-offender program. The overarching objective of the project is to contribute the first experimental evidence on the relative effects of mediation and restorative justice conferencing approaches, thereby addressing theoretical claims regarding the superiority of more inclusive approaches. The trial also has a more pragmatic aim: to promote local evidence-based practice and policy development by introducing and testing the RJC model as a possible alternative to the Danish mediation approach typically employed in the *konfliktråd* program (see description of treatments below).

In late 2017, the leading police commissioners in all 12 Danish police districts were informed about the KIP project and invited to enroll; seven districts chose to join the randomized experiment.² These districts, each of which has a local program manager responsible for coordinating victim-offender meetings in their region, are geographically spread across the country, encompassing the four largest Danish cities and a variety of rural areas. Informed by the literature and theory outlined above, the general hypothesis tested in the experiment is that:

Restorative justice conferences, generally conducted with supporters of both victims and offenders present, will be a more effective form of restorative justice practice in the Danish *konfliktråd* program than current victim-offender mediations.

The data collected to test this hypothesis include comprehensive case flow information, observations of meetings, register data on offenders' recidivism, interviews, and follow-up surveys sent to victims and offenders both 1 month and 6 months after convened meetings. It is these survey responses that form the empirical basis for the current article. As previous research has demonstrated, measuring participants' own assessments of their meetings is crucial to understanding the effects and lived consequences of restorative justice. It is in this context that we test our experimental hypothesis in relation to victims' and offenders' overall ratings of their meetings. Before reporting on these tests, it is important to first anchor the analyses and our later interpretation of the results in a more thorough description of the two interventions and the methodology used to compare them.

Description of treatments

The practitioners who convened the restorative meetings in the project were all trained mediators employed in the Danish victim-offender *konfliktråd* program at the time.³ To minimize the risk of treatment cross-over, these practitioners were assigned to deliver only one type of treatment in the experiment: either restorative justice conferences or mediations. All meetings of both types were conducted as supplements and not diversions from the traditional court system, in line with the legal framework for the Danish program.

Mediations

Mediations were carried out using a treatment-as-usual approach following current practices within the victim-offender program. As noted earlier, Danish mediations are generally designed as face-to-face meetings bringing together at least one victim and one offender for a dialogue about the crime and its effects. By law, supporters (*bisiddere*) can

be included in these meetings at the request of participants and the discretion of the mediator, but their role is framed as secondary, and past research suggests their inclusion is not standard but is most common in cases involving juveniles, where guardians sometimes participate (see Danish Ministry of Justice's Committee on Konfliktråd, 2008; Permin Berger et al., 2015).

Mediation practitioners are free to guide the meeting dialogue as they see fit, with some taking a fairly directive role in the discussion, while others remain more withdrawn. This variation likely reflects both the individual mediator's personality and the timing of their training. When the national program was launched in 2010, the standard training was based on the six-phase reflexive mediation model (Lappi-Seppälä & Storgaard, 2015; Vindeløv, 2012), in which the mediator played quite an active role, and the meeting was intended to result in a written agreement. However, since 2014, mediators have been trained in an alternative facilitative model (Riskin, 2003), partly inspired by the reflexive model, but with a simpler "facts-feelings-needs" structure, and with greater focus on the mediator being less active and withdrawing from the dialogue to increase participants' ownership (Friis Pedersen, 2023). The mediators participating in the Konfliktråd Impact Project were originally educated in the program between 2010 and 2019, and several also have mediation training from elsewhere. They therefore have rather varied backgrounds and methodological standpoints. However, most will have been introduced to at least some elements of the more recent training approach via program workshops, collegial interactions, or supervision. We therefore focus on describing this model, in which the majority of the project mediators were trained during a 5-day workshop organized by the national secretariat for the victim-offender program:

(1) Preparation before the meeting

The mediator or a program coordinator contacts the offender and victim (and initially their guardians, in the case of minors) to inquire whether they are interested in participating in a meeting and to inform them about the process. If they express interest, the mediator makes practical arrangements for the meeting. If deemed relevant, the mediator can inquire whether victims and offenders would like to bring anyone with them to the meeting, but this is not mandatory. The meeting preparations are conducted over the telephone and sometimes involve physical pre-meetings with the parties separately. On the day of the meeting, the mediator prepares the mediation room, positioning themselves at the end of a table with the parties on each side. Paper for notetaking, refreshments, and candles are placed on the table to create a calm atmosphere.

(2) The meeting itself

Opening: The mediator welcomes the participants and may choose to say a few words about his/her role and the "rules" that guide the meeting (confidentiality, etc.). The mediator asks each of the main parties, i.e., the victim and the offender, what their hopes are for the meeting, normally starting with the victim. To promote victim empowerment, the model starts with the victim in all phases, unless the victim prefers that the offender starts, which is sometimes the case. The mediator repeats the hopes expressed by the parties and ensures that they are correctly understood and heard by the other party.

Facts: The mediator initiates a dialogue about what has happened, first asking the victim. The offender is typically asked similar questions to those asked of the victim, and the mediator asks follow-up questions based on the responses from both parties. This approach also applies to the subsequent phases.

Feelings: The mediator initiates a dialogue about how the participants have been/are affected, starting with the victim.

Needs: Based on the expressed feelings, the mediator initiates a dialogue about the participants' needs to move on positively, starting with the victim. An oral or written agreement may be formulated if the mediator offers it, and the main parties wish to include this.

Closing: The mediator repeats the hopes expressed at the beginning of the meeting and asks the main parties whether their hopes for the meeting have been met.

(3) Follow-up after the meeting

The mediator has no formal requirement to follow-up, but some may choose to contact victims and offenders shortly after the meeting to debrief.

While this process was introduced as a general starting point, the training emphasized structural flexibility, meaning that mediators are free to adjust the process according to their professional judgment, should they assess that a different structure would be better for the participants in a concrete case. Similarly, if supporters are present, it is up to the mediator to determine when and to what extent they should be involved in the dialogue. Against the background of significant flexibility and variability in the Danish mediation model, a “fully” implemented mediation was defined in the experimental protocol using only two criteria: that (i) at least one offender and (ii) at least one victim, is present with a mediator. No further restrictions were placed on this treatment-as-usual group, beyond the prohibitions and requirements of Danish law.

Restorative justice conferences

The restorative justice conference model adopted in this project mirrors that employed in previous tests of the method in the Australian Reintegrative Shaming Experiments (RISE, Strang et al., 2011) and subsequent examinations in England and Wales (Shapland et al., 2011), with two important variations. First, whereas the conferences in some of those experiments were carried out by police officers, this was not the case in Denmark, where the legal framework establishes lay mediators (coined facilitators in the RJC model), as meeting conveners. Second, whereas standard follow-up procedures were employed after the meeting in some other jurisdictions, this was unfortunately not possible in our case due to structural and resource constraints within the Danish victim-offender program (we return to this point later).

All practitioners assigned to carry out RJC in the experiment were already active mediators in the victim-offender program, as noted above. For the purposes of the project, half of the mediators working in the seven test districts at the time received three intensive days of supplementary RJC training and were instructed to exclusively use the RJC model in all randomized cases. To ensure that the providers delivering the two different treatments were as similar as possible, random assignment to be trained in the RJC approach was employed in the districts where the number of practitioners available to be trained in the new method exceeded the number of training spots available. Eight of the 14 individuals

ultimately trained in the RJC method were randomly assigned. The remaining six were allocated to the role by their supervisors, based on practical considerations such as their availability on the training dates and ability to take on the necessary case flow during the project period. As we argue elsewhere, the partial randomization of service providers provides a pragmatic solution to increasing the external validity of criminological field experiments, while also recognizing the practical constraints of real-world research settings (see Sherman et al., 2021).

The RJC training was manualized and comprised theoretical and practical instruction from Australian restorative justice expert John McDonald, who also trained the facilitators involved in the earlier international trials. In line with the theoretical basis for the model, facilitators were trained to include as many relevant supporters to both victims and offenders as possible in the meeting, to follow a standard meeting “script,” and to draft a written outcome agreement aimed at repairing past harm and avoiding future offending at the end of each meeting. In addition, they were taught several verbal and non-verbal techniques aimed at limiting their own involvement, interruption, and interpretation of the dialogue, by encouraging the meeting participants themselves to speak (for further descriptions see McDonald, 2012; Sherman et al., 2015). Overall, compared to the relative flexibility of the mediation model, the RJC model employed a more tightly structured approach as outlined below:

(1) Preparation before the meeting

Like with mediations, the facilitator or a program coordinator contacts the offender and victim (and initially their guardians, in the case of minors) to inquire whether they are interested in participating in a meeting and to inform them about the process. If they express interest, the facilitator makes practical arrangements for the meeting. As all RJC victims and offenders are actively encouraged to bring supporters with them to the meeting, identifying additional relevant participants is a standard part of the preparatory process. As with mediations, these preparations can take place in person or over the telephone. Prior to the meeting itself, the facilitator has responsibility for preparing the room by arranging chairs in a circle with no table, creating a seating plan according to a conference “map,” and setting up refreshments to be shared toward the end of the meeting.

(2) The meeting itself

Opening: The facilitator welcomes/introduces the participants and reiterates the meeting process and “rules” (confidentiality, etc.) explained during the preparatory phase.

Stage 1- What happened?: The facilitator initiates a dialogue about what happened, by posing scripted questions to the offender asking them to first describe in detail what occurred, and then what they were thinking at the time, and what effects they think their actions had. In contrast to the mediation model, offenders are asked to start, to promote accountability.

Stage 2- How were people affected?: The facilitator initiates dialogue about how all of the participants have been affected, posing scripted questions starting with the victim, followed by victim supporters, then offender supporters. Finally, the offender is asked if they would like to respond to what he/she has heard.

Stage 3- What should be done?: The facilitator helps participants decide what can be done to repair the harm and prevent re-offending by drafting a restorative agreement. The facilitator initiates the dialogue by first asking the victim and then the other participants, what they would like to see happen as a result of the conference. For every suggestion, follow-up questions query whether the proposal is realistic and fair, and who can help ensure that these things will be done, and when. Agreed items are then written down by the facilitator while the participants take a small break and/or interact informally over refreshments. This stage ends with the facilitator reading the agreement back to the participants, who subsequently sign it.

Closing: The facilitator asks each participant how they are feeling now, carefully considering who should speak last.

(3) *Follow-up after the meeting*

As noted above, the facilitator is not formally required to follow-up (unless a second meeting is agreed upon). However, some may choose to contact victims and offenders shortly after the meeting to debrief if they deem it necessary. Facilitators were trained not to become engaged in implementing or ensuring compliance with restorative agreements, as the police considered this both infeasible and beyond the remit of the Danish victim-offender program's status as a supplementary and voluntary offer.⁴ When drafting agreements, facilitators were encouraged to discuss with the parties and their supporters how they, themselves, could help to ensure compliance and feedback.

As conferencing was introduced as a new practice, a pilot period followed the RJC training, during which a total of 25 non-randomized RJCs were carried out and each facilitator was observed in the field and given implementation feedback by a member of the research team at least once before the randomized trial was launched. Based on the theoretical literature, a “fully” implemented RJC was defined as a meeting that met five criteria: (i) at least one offender, (ii) at least one victim, (iii) at least one victim supporter, (iv) at least one offender supporter, and (v) a written agreement.

Methods

Trial recruitment, enrollment, and randomization

Prior to commencing randomization, the project was reported to the Danish Health Research Ethics Committee for Central Jutland (Forespørgsel 36/2018) and a locked study protocol was published on the institutional websites of the primary investigators. Formal recruitment into the trial opened in November 2018 and closed upon randomization of the 200th case in February 2022.⁵ During this period, the police’s victim-offender coordinators were asked to record all cases referred or otherwise coming to their attention in a systematic case registration system designed by the research team. As illustrated in the CONSORT diagram provided in [Figure 1](#), 2921 potential cases were recorded during this period and assessed for eligibility one case at a time in a trickle-flow process.⁶

Eligibility for randomization was determined using a three-step process. Cases had to first meet the standard organizational criteria for inclusion in the Danish victim-offender

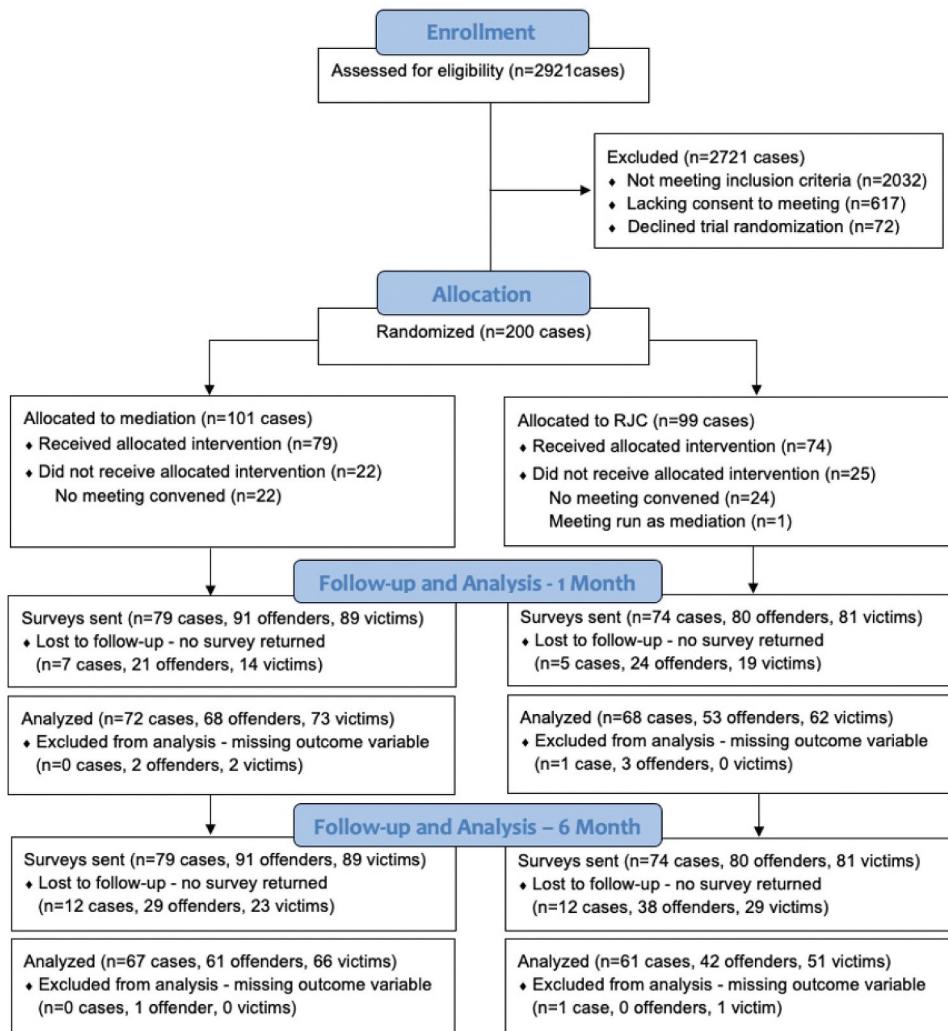


Figure 1. CONSORT flow diagram. Note. Surveys were not sent in n = 1 convened RJC due to a distribution error. Counts of surveys available for follow-up/analysis are based on condition that ≥ 1 response was received and that data was available for all outcomes. Partial analysis was possible for n = 1 additional RJC case and n = 1 additional RJC victim.

program, including documentation of one or more identifiable offender(s) and personal victim(s), sufficient admission on the part of the offender to the factual elements of the case, and the absence of e.g., serious mental instability that could put meeting participants at risk.

For cases meeting the basic program requirements, a second screening step assessed whether the additional inclusion/exclusion requirements for the experiment were met. Given the crime-prevention focus of the project, only criminal code violations were eligible (to the exclusion of murder, sexual crimes, violations of eviction/restraining orders, and all cases with indications of domestic violence or neighborhood disputes).⁷ For practical and measurement reasons, cases requiring a translator, cases with counter charges, and cases involving parties who had participated in a previous mediation within the last two years,

were excluded. Finally, some cases were excluded at the discretion of district coordinators e.g., based on knowledge that an offender would shortly be relocating or sent to a closed treatment facility.

For the cases meeting all the above eligibility requirements, the final inclusion criteria for case enrollment were: (i) willingness on the part of at least one victim and at least one offender associated with the case to meet in the victim-offender program and (ii) informed consent to random assignment. As shown in [Figure 1](#), a large number of cases were excluded from the trial because one or more parties were not interested in meeting or couldn't be reached. Of the cases where initial consent to a meeting was secured, 74% ($n = 200$) met the final criterion that at least one offender and one victim provided informed consent to trial randomization. Upon receiving documentation of this consent and basic case details from the districts, a member of the research team performed a final eligibility check before entering these cases into a secure REDCap project database for randomization (Harris et al., [2019](#)).⁸

Cases were assigned to mediation or RJC according to a computerized random number list generated for two treatment arms using a 1:1 allocation ratio. To ensure that approximately equal numbers of cases would be allocated to mediation and RJC in each of the seven participating districts, the random allocation lists were stratified by district using permuted block randomization with random varying block sizes of four and six. Protected randomization sequences were generated and uploaded to REDCap by an independent service provider (Clinical Trials Unit, Department of Clinical Medicine, Aarhus University) and were not visible to the research team, thereby ensuring proper allocation concealment. The randomization procedure rendered a final sample of 101 mediations and 99 RJCs spread across the seven districts. Immediately following randomization, the allocation result was returned to the district coordinators, who assigned one of their lay mediators/facilitators to prepare the participants and convene the meeting itself. As mediations and RJCs were carried out by separate practitioners and required different preparatory and meeting processes, it was not possible to blind the districts, convenors, or research team to treatment assignment.

As illustrated in [Table 1](#), the randomization allocation was successful in establishing baseline equivalence across a range of individual and case characteristics for the two intervention groups.

Implementation fidelity

As shown in [Figure 1](#), 154 of the 200 randomized cases resulted in a convened meeting. The proportion of cases receiving a meeting was balanced for the two intervention groups (VOM = 78%, RJC = 76%). The reasons underlying drop-out included inability to get in contact with one or both parties after randomization, victims or offenders changing their mind regarding participation,⁹ cancellations due to repeated COVID-19 restrictions imposed during the project, or no-shows on the day.

Key meeting characteristics for the two groups are shown in [Table 2](#). As compared to convened mediations, restorative justice conferences were significantly longer, involved a greater number of supporting participants, and were much more likely to end in a written outcome agreement. While three out of four convened conferences included at least one victim/offender supporter, it is noteworthy that a relatively small number of supporters participated in these conferences – on average just over one on each side. Most commonly, these were family and/or friends, and only occasionally professionals like a teacher or case

Table 1. Balance between intervention groups at baseline.

	Mediations		Conferences		<i>p</i>
	Mean (SD)	Mdn	Mean (SD)	Mdn	
Case-level variables					
Days since offense at randomization	91.8 (99.0)	53.0	107.7 (192.8)	51.0	.23
Number of registered victims	1.10 (.36)	1.0	1.25 (1.45)	1.0	.30
Number of registered offenders	1.23 (.93)	1.0	1.16 (.51)	1.0	.53
Crime type					.54
Violence (%)	73.3		77.8		
Property (%)	23.8		21.2		
Other (%)	3.0		1.0		
Individual-level variables					
Age					
Offender age at offense	28.0 (13.9)	22.2	26.9 (15.4)	19.1	.61
Victim age at offense	32.5 (16.7)	26.3	29.4 (16.7)	23.2	.17
Offender <15 (%)	14.0		21.6		.12
Victim <15 (%)	12.4		19.4		.15
Gender					
Male offender (%)	88.5		86.5		.50
Male victim (%)	67.5		64.8		.67

Note. Case-level comparisons based on $n = 101$ mediations and $n = 99$ conferences.

Individual-level comparisons based on $n = 233$ offenders and $n = 222$ victims.

Differences tested with chi-squared and independent t-tests. No significant differences at $p \leq .05$.

Table 2. Meeting characteristics by intervention type.

	Mediations		Conferences		<i>p</i>
	Mean (SD)	Mdn	Mean (SD)	Mdn	
Timing					
Days between randomization and meeting	19.0 (93.1)	18.0	28.8 (35.8)	21.0	.40
Meeting duration in minutes	58.4 (30.1)	53.5	88.9 (36.2)	85.0	<.001
Meeting terminated early (%)	3.8		1.3		.33
Meeting participants					
Total number excluding convenor	3.3 (2.6)	3.0	5.0 (2.7)	4.0	<.001
Number of victims	1.1 (.44)	1.0	1.1 (.37)	1.0	.60
Number of offenders	1.2 (.85)	1.0	1.1 (.48)	1.0	.38
Number of victim supporters	0.4 (.66)	0.0	1.4 (1.2)	1.0	<.001
Number of offender supporters	0.5 (1.2)	0.0	1.3 (1.4)	1.0	<.001
≥1 victim present (%)	100.0		98.7		.31
≥1 offender present (%)	100.0		97.3		.15
≥1 victim supporters present (%)	35.9		76.0		<.001
≥1 offender supporters present (%)	26.9		74.7		<.001
Agreement					
Made written agreement (%)	10.3		96.0		<.001

Note. Based on $n = 79$ convened mediations and $n = 75$ convened conferences.

Differences tested with chi-squared and independent t-tests. Significant differences identified in bold.

worker. This finding is consistent with implementation patterns seen in the English trials (Shapland et al., 2007, p. 20). Furthermore, in one out of four mediations held in this project -often those involving young people – supporters were also present. The statistically significant differences illustrated here nonetheless clearly indicate the existence of two divergent delivered treatments.¹⁰

This impression is further supported by the implementation scores shown in Table 3. Using objective meeting data, a count-based implementation score was calculated for all meetings as a sum value indicating the number of RJC criteria met (where meetings that failed to take place score 0, and a score of 5 indicates full implementation as described earlier). Based on these criteria, 43% of all randomized RJCs and 57% of all convened RJCs

Table 3. Implementation scores by intervention type.

	Mediations		Conferences	
	Mean (SD)	Mdn	Mean (SD)	Mdn
RJC implementation score (out of 5)				
All randomized cases	2.1 (1.4)	2.0	3.4 (2.0)	4.0
“Fully” implemented (%)	0.0		43.4	
Convened meetings	2.7 (.82)	2.5	4.4 (.77)	5.0
“Fully” implemented (%)	0.0		57.3	
Observer rating (out of 5)				
To what extent did the mediation resemble the RJC model as designed?	1.4 (.52)	1.0	n/a	n/a
Rated ≥3 (%)	1.6			
To what extent was the RJC conference implemented as designed?	n/a	n/a	3.96 (.95)	4.0
Rated ≥3 (%)			92.3	

Note. n/a = rating not applicable for this group.

Mediations: $n = 101$ randomized, $n = 79$ convened, $n = 61$ observed.

Conferences (RJC): $n = 99$ randomized, $n = 75$ convened, $n = 52$ observed.

were “fully” implemented, in that they included at least one victim, one offender, a supporter on each side, and a written agreement. An additional 23% of randomized cases and 31% of convened RJC meetings scored four out of five. Where full implementation was not achieved, this was most often due to one or both parties failing to bring supporters to the meeting. The sum scores shown for mediations are, as expected, significantly lower than those for RJC, reflecting the fact that they included supporters and written agreements far less often. As illustrated in Figure 1, there was one formal protocol deviation in the RJC group, such that a case allocated to receive RJC was purposely carried out as a mediation, which was assessed as more appropriate by the facilitator in the case.¹¹

In addition to the objective count-based implementation score described above, a second measure was coded for the 113 meetings that were observed by at least one member of the research team (73% of all convened meetings). For the 52 observed conferences, this took the form of a global rating indicating the extent to which the RJC was implemented as trained and met the overall (qualitative) standards of the model, including adherence to the conference manual and “script.” As established in the study protocol, a rating of three or more on this 5-point rating scale was considered acceptable (corresponding to a “moderate” implementation rating); this standard was met in 92% of observed conferences.

In order to measure the extent to which the treatment-as-usual (mediation) approach resembled the new RJC model in practice, the 61 observed mediations were instead rated with respect to the extent to which they resembled the RJC model as trained (also on a scale from 1 to 5). Higher scores on this measure identify mediations with greater similarity to the RJC model, such as those including supporters, a written agreement, or a more facilitative convener style. Just under 2% of mediations were rated three or higher on this scale, again indicating that the two models diverge in practice. Taken together, these implementation measures indicate satisfactory (if imperfect) implementation of the theoretical RJC model, and support for our design expectation that the actual conduct of RJC would differ from the conduct of mediations on several important dimensions.¹²

Having established the basic design and implementation status of the trial, we now turn our attention to the collection and analysis of the specific survey outcomes explored in this paper.

Survey measures and procedures

One month and 6-month follow-up surveys were developed by the research team in collaboration with the participating districts. Questions aimed at measuring victims' and offenders' experiences before, during, and after their restorative meetings were constructed with inspiration from previous national and international evaluations and the stated aims of the local Danish victim-offender program (e.g., Hansen, 2012; Shapland et al., 2007; Strang, 2002). For our current purposes, we focus on four general outcome questions that were presented at the end of the survey under the heading "Overall experience of the meeting." In this section, participants were asked to provide a global assessment of their meeting, measured in relation to several theoretically relevant dimensions. To allow for comparisons across respondent groups and time, the same questions were posed to both victims and offenders at both follow-ups.

For the first three outcomes, respondents were instructed to: "indicate for each of the following statements the extent to which it reflects your overall experience of the meeting:"

- "The meeting was fair"
- "The meeting has helped me move on"
- "The harm that was done has been repaired"

A forced-choice 4-point ordinal rating scale was employed for these items, allowing respondents to check one of the following response categories: 1 = "no, not at all;" 2 = "no, not really;" 3 = "yes, to some extent; 4 = "yes, to a large extent."

The fourth and final question, "How would you rate the meeting overall?" was included to capture respondents' perception of the overall success of their meeting, as in Hansen's (2012) previous Danish evaluation of victim-offender mediations. This item was rated on a 5-point Likert scale including a neutral mid-point: 1 = "very unsuccessful;" 2 = "unsuccessful;" 3 = "neither successful nor unsuccessful;" 4 = "successful;" 5 = "very successful").¹³ While these overall measures are relatively rough and do not allow for specific theoretical analysis of, for example, procedural vs. interactional justice assessments of fairness (Scheuerman et al., 2021), they have the advantage of capturing participants' general impressions of their meetings, an important starting point for future work unpacking these assessments using more fine-grained data also collected in the project.

Auto-generated survey links were sent from the research team via REDCap to all victims and offenders 1 month (standardized to 29 days) and 6 months (standardized to 174 days) after their convened meeting dates. In the case of non-response after the first week, an autogenerated e-mail nudge was automatically sent 7 days after dispatch, and personal phone calls and SMS reminders were employed after 2 weeks and 3 weeks. As remuneration for their time, respondents were sent a 150 kroner gift card (approximately 20 dollars USD) that could be used in a variety of online and physical stores. Neither the police nor the mediators/facilitators were involved in the data collection or informed regarding the responses of individual participants.

Table 4. Survey response rates at 1 and 6 months by intervention type.

	1 month			6 months		
	VOM % (n)	RJC % (n)	All % (n)	VOM % (n)	RJC % (n)	All % (n)
Case level^a						
Share of all randomized cases	71.3 (72)	68.7 (68)	70.0 (128)	66.3 (67)	61.6 (61)	64.0 (128)
Share of convened meetings	91.1 (72)	90.7 (68)	90.1 (128)	84.8 (67)	81.3 (61)	83.1 (128)
Respondent-level^b						
Share of victims	82.0 (73)	76.5 (62)	79.4 (135)	74.2 (66)	63.0 (51)	68.8 (117)
Share of offenders	74.7 (68)	66.3 (53)	70.8 (121)	67.0 (61)	52.5 (42)	60.2 (103)

Note. Mediations (VOM): $n = 101$ randomized, $n = 79$ convened. Conferences (RJC): $n = 99$ randomized, $n = 75$ convened.

^aPercentage of cases represented with ≥ 1 survey response for all analyses.

^bPercentage of distributed surveys with a response for all analyses.

Final response rates at the case and individual levels are provided in [Table 4](#). Between 0–9 survey responses ($M = 1.72$, $SD = 1.02$) were returned per case. At 1 month, 70% of all randomized cases (whether a meeting was held or not) and wholly 90% of cases resulting in a convened meeting are represented in the data with at least one survey response, decreasing to 64% of randomized cases and 83% of convened meetings at 6 months. Some response imbalance was observed for the two intervention arms, such that a lower share of RJC participants returned a survey, especially at the second follow-up. This difference is statistically significant for offenders at 6 months ($p = .037$). A series of non-response analyses indicated that for meeting participants, there were no significant differences between survey responders and non-responders for gender, age at offense, or crime type at either follow-up point. In addition, there were no significant differences in these distributions for responders in the RJC and mediation groups, indicating that the baseline equivalence of the two treatment arms was maintained in the survey data.

On average, surveys were returned 45 days ($SD = 20.3$) after respondents' meeting date for the 1-month follow-up and 201 days ($SD = 23.4$) for 6-month follow-up. In both conditions, most surveys were filled out online, with a small number completed over the phone or on paper (most often by incarcerated offenders).

Analytic strategy

The study analyses were carried out in two analytical steps using STATA version 18.0 (StataCorp., 2023), treating participants' data according to the original group assignment of their case and pooling responses from all seven districts. All analyses were unblinded but carried out by a contracted statistician unconnected to the design and implementation of the trial (the fourth author).

First, basic descriptive analyses exploring the response distributions for each of the four ordinal outcomes were carried out, split by intervention group and follow-up period. Counts and percentages were calculated both as totals combining victims and offenders in each treatment group, as well as separately for victims and offenders within each respective intervention. These analyses provide an initial descriptive picture of how each meeting type was experienced by participants.

Second, to test the inferential hypothesis that RJC would evidence higher ratings of fairness, harm repair, capacity to move on, and overall success as compared to mediations,

a series of regression models were constructed. We initially planned to analyze all four outcomes using ordinal logistic regression. However, due to generally high ratings across the four outcomes and very small cell counts for some of the response options, it was necessary to collapse the outcome data into fewer response categories to run the models. To allow for comparisons across models, all four outcomes were dichotomized, combining the two “yes” categories and the two “no” categories into a binary yes/no variable for the fairness, move on, and repair variables. The overall satisfaction variable was collapsed to capture ratings indicating “very successful” and “successful” meetings in one category vs. meeting rated as “neither/nor,” “unsuccessful” or “very unsuccessful” in the other category.¹⁴

For each of these binary outcomes, simple logistic regressions pooling victims and offenders were first constructed, with intervention (binary, 0 = mediation, 1 = RJC) as the sole predictor. These models address the question of whether participants generally rate RJC s differently than mediations; that is, whether there is an overall intervention effect. Expanded models were then constructed in a second step, including both intervention type, respondent group (binary, 0 = offender, 1 = victim), and their interaction as predictors, allowing us to extract focused comparisons that identify differences in intervention effects for victims and offenders specifically. Given the demographic equivalence between the two intervention groups illustrated in [Table 1](#) and the survey response data, no additional controls were included. Due to power concerns, separate models were constructed using the 1-month and 6-month follow-up data.

For all analyses, cluster robust standard errors were used to account for victim(s) and offender(s) being nested within the same crime. Model fit was evaluated using only area under the curve (AUC), as the main estimated models were saturated. Prediction was considered significantly better than chance if AUC was greater than .50 at $p < .05$. In addition to testing for statistical significance, odds ratios (OR) and their confidence intervals were calculated to estimate effect size, where OR values above 1 favor RJC, and values below 1 favor mediation.

Results

Descriptive analysis

Original response distributions and within group percentages for each outcome variable are illustrated in [Figures 2](#) and [3](#). Disaggregated distributions for victims and offenders are additionally presented in Appendix [Table A1](#) and [Table A2](#). For all outcomes, the large majority of respondents used the two highest rating categories, indicating that both interventions were evaluated positively by most victims and offenders at 1 month follow-up.

At 6-month follow-up, most respondents again used the two most positive answer categories, but the minority of respondents reporting more neutral or negative ratings had grown in both conditions. At both follow-ups, a larger share of RJC participants rated the overall success of their meeting neutrally.

As noted earlier, very small cell-counts were observed for some of the negative response options at both 1 month and 6 months, especially for the fair and overall success outcomes. For this reason, dichotomized outcomes indicating negative and neutral vs. positive response options are reported for all subsequent analyses.

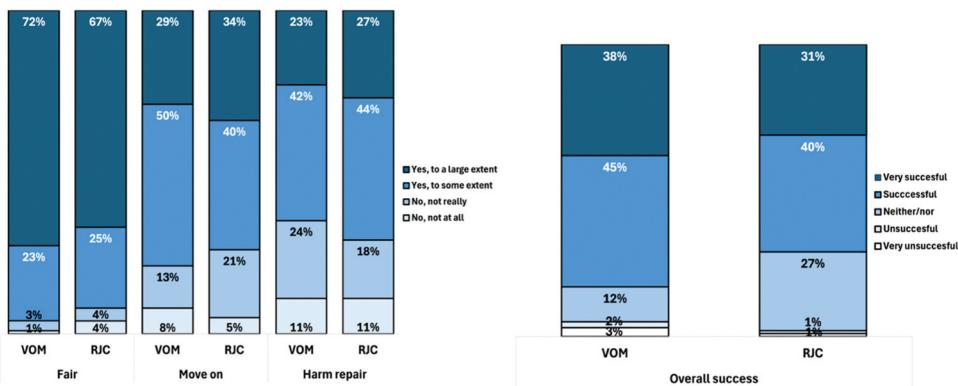


Figure 2. Response distributions by intervention type – 1 month.

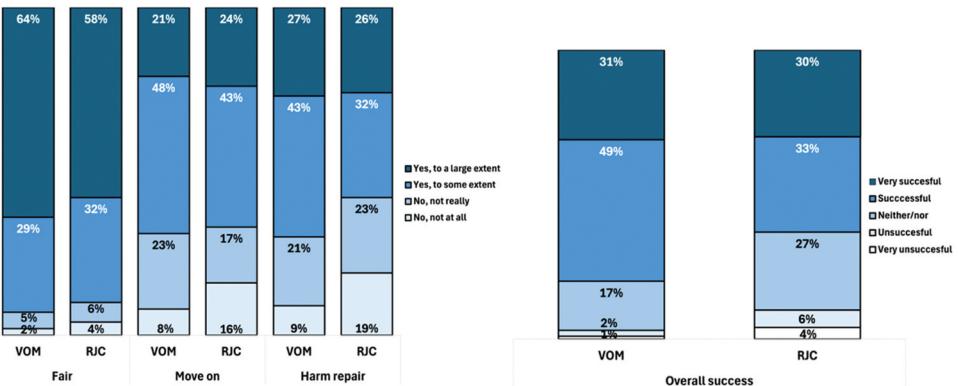


Figure 3. Response distributions by intervention type – 6 months.

Inferential tests of intervention effects

Tables 5 and 6 present results from logistic regression analyses for the four outcomes at 1-month and 6-month follow-up, respectively.

The results of the simple logistic regression models including intervention type as the sole predictor are shown in Panels A of both tables. At both 1-month and 6-month follow-ups, the only statistically significant difference between conditions was observed for the overall success variable, with RJC demonstrating *lower* odds for positive evaluation (1-month: OR = .51, 95% CI (.27, .96); 6-month: OR = .43, 95% CI (.25, .77)). Comparing a participant in RJC with a participant in mediation, the participant in RJC has 49% lower odds of providing a successful overall evaluation than a neutral or unsuccessful rating relative to the participant in the mediation group at 1 month, and 57% lower odds at 6 months. In other words, mediations were rated as significantly more successful at both follow-ups. Measures of perceived fairness and capacity to move on showed the same

Table 5. Association between binary outcomes, intervention type, and respondent group at 1 month.

	Fair binary		Move on binary		Repair binary		Success binary	
	OR (95% CI)	p	OR (95% CI)	p	OR (95% CI)	p	OR (95% CI)	p
Panel A								
RJC overall/VOM overall ^a	.52 (.17, 1.62)	.261	.73 (.40, 1.34)	.313	1.37 (.77, 2.42)	.286	.51 (.27, .96)	.038
Overall model fit ^a (AUC) ^c	.58	.114	.54	.149	.54	.126	.58	.013
Panel B								
RJC victim/VOM victim ^b	.66 (.17, 2.61)	.554	.74 (.34, 1.63)	.458	1.36 (.65, 2.86)	.413	.57 (.25, 1.30)	.182
RJC offender/VOM offender ^b	.37 (.07, 1.92)	.238	.73 (.32, 1.64)	.442	1.42 (.63, 3.22)	.396	.45 (.19, 1.04)	.061
Overall model fit ^b (AUC) ^c	.60	.090	.55	.121	.58	.012	.59	.015

Note. N = 256. Odds ratios (OR) above 1 favor conferencing (RJC), odds ratios below 1 favor mediation (VOM).

OR is significant at $p < .05$ if 95% CI does not overlap with 1. Significant comparisons identified in bold.

^aEstimate is based on crude logistic regression model with Intervention type as sole predictor.

^bEstimate is based on logistic regression analysis with intervention type, respondent group, and intervention*respondent group interaction as predictors.

^cp-value for AUC > .50.

Table 6. Association between binary outcomes, intervention type, and respondent group at 6 months.

	Fair binary		Move on binary		Repair binary		Success binary	
	OR (95% CI)	p	OR (95% CI)	p	OR (95% CI)	p	OR (95% CI)	p
Panel A								
RJC overall/VOM overall ^a	.64 (.22, 1.84)	.409	.89 (.49, 1.59)	.686	.57 (.30, 1.09)	.091	.43 (.25, .77)	.004
Overall model fit ^a (AUC) ^c	.56	.180	.51	.340	.57	.026	.60	.003
Panel B								
RJC victim/VOM victim ^b	.77 (.21, 2.77)	.685	.76 (.32, 1.83)	.538	.37 (.17, .82)	.015	.39 (.17, .89)	.026
RJC offender/VOM offender ^b	.49 (.10, 2.34)	.373	1.02 (.46, 2.25)	.970	1.09 (.46, 2.60)	.848	.49 (.21, 1.12)	.090
Overall model fit ^b (AUC) ^c	.58	.108	.55	.092	.62	.001	.61	.005

Note. N = 221 (N = 220 for move on). Odds ratios (OR) above 1 favor conferencing (RJC), odds ratios below 1 favor mediation (VOM).

OR is significant at $p < .05$ if 95% CI does not overlap with 1. Significant comparisons identified in bold.

^aEstimate is based on crude logistic regression model with intervention type as sole predictor.

^bEstimate is based on logistic regression analysis with intervention type, respondent group, and intervention*respondent group interaction as predictors.

^cp-value for AUC > .50.

direction of effect but were not significant. A different pattern emerged for the harm repair variable, indicating *higher* odds for positive evaluation for RJC at 1 month, although this difference was also non-significant.

To explore whether the general intervention effects outlined above were similar for victims and offenders, a second set of logistic regressions were run for both 1-month and 6-month follow-ups, including an additional predictor for respondent group (victim/offender), and an intervention by respondent group interaction term. Focused comparisons extracted from these models are shown in Panels B of Tables 5 and 6. Once again, no significant differences between conditions emerged for the fair and move on variables at either follow up. In addition, no significant differences were observed for the success or

repair outcomes at 1 month in either respondent group. However, at 6 months, the intervention effects for victims and offenders diverged for the overall success and harm repair measures. Specifically, whereas RJC victims had significantly *lower* odds of a positive success rating compared to mediation victims at 6 months ($OR = .39$, 95% CI (.17, .89)), no such difference was observed for offenders. Similarly, while victims in the RJC condition had significantly *lower* odds of positive evaluation for the repair outcome as compared to victims in the mediation condition ($OR = .37$, 95% CI (.17, .82)), offender ratings of harm repair were more positive and did not differ by intervention type. Comparing a victim in RJC with a victim in mediation, the victim in RJC thus has 63% *lower* odds of positive evaluation of the repair outcome than the victim in mediation, whereas for a similar comparison between an offender in RJC with an offender in mediation, the odds of positive evaluation of harm repair is 9% *higher* for the offender in the RJC group (but non-significant). Taken together, these patterns indicate that the few observed differences favoring mediation are primarily driven by victims' ratings.

Discussion

As the first of its kind worldwide, the Konfliktråd Impact Project (KIP) randomized controlled trial provides the first experimental evidence comparing outcomes of direct victim-offender mediations and restorative justice conferences. In this paper specifically, we tested the hypothesis that victims and offenders participating in conferences would provide higher ratings of their meeting's fairness, the extent to which it repaired harm, helped them to move on, and was deemed an overall success. Three main conclusions can be drawn from our analyses.

First, with respect to *absolute* ratings of mediations and conferences, our descriptive results show that victims and offenders participating in both kinds of meetings generally rated them positively at both 1 and 6 months, with some minor decreases observed over time. Even at 6 months, 91% of all survey respondents found their meeting fair, 68% reported that the meeting had helped them move on, and 65% indicated that the harm had to a large or some extent been repaired. Furthermore, 72% of all respondents assessed their meeting as successful or very successful. These results are consistent with other international research on restorative justice, which also shows high overall satisfaction with most face-to-face meetings (Hansen & Umbreit, 2018; Shapland et al., 2007, 2011; Sherman et al., 2015; Umbreit et al., 2001; Strang et al., 2013; Nascimento et al., 2023). The results also confirm previous positive Danish evaluations based on questionnaire data (Hansen, 2012; Henriksen, 2003), indicating that the scheme largely meets key objectives as described in the legislation and by the police themselves (Danish Ministry of Justice, 2008, 2009; Danish National Police, 2012, 2017). A small minority of respondents nonetheless rated their meetings as unsuccessful, and these assessments should not be ignored. Whether the negative experiences in these meetings can be attributed to the same factors underlying the failures observed in previous research is an issue we are currently exploring using other data collected in the study. Identifying what can mitigate the risk of unproductive meetings is paramount to ensuring the credibility and value of restorative justice processes for all (Choi et al., 2012; Shapland et al., 2024).

Second, in relation to respondents' *relative* ratings of mediations and RJCs, our regression results indicate only a few statistically significant differences between the two treatment types.

These were relatively small in effect size terms and should be interpreted against the backdrop of generally high ratings in both conditions. Whereas no significant differences were observed for the binary outcomes assessing meeting fairness or help in moving on at either follow-up, mediations were evaluated significantly more positively than conferences for the overall success outcome at both 1 and 6-month follow-up, as well as the harm repair measure at 6 months, for victims specifically. As we discuss later, these findings fail to support our theoretically driven study hypothesis that RJC would be rated more positively for all outcomes.

With respect to the overall success outcome, it is important to note that the binary categorization for this variable combined neutral and unsuccessful/very unsuccessful ratings in one category, and we compared the odds of these ratings with a successful/very successful assessment. Due to the very limited number of negative ratings in both groups, it was not possible to exclude or separate neutral ratings for the inferential analyses. An inspection of the original response distributions for this variable ([Tables A1](#) and [A2](#)), suggests that the neither/nor ratings are the primary driver of the observed intervention effect, as the share of both RJC and mediation respondents who report decidedly unsuccessful meetings is small.

A particularly interesting pattern of results emerged in relation to the assessment of harm repair. The direction of the odds ratios for this variable indicates that RJC were rated more positively than mediations for repair by both victims and offenders at 1 month, and by offenders but not victims at 6 months. These results seem to suggest that while RJC may have promise to repair harm to a greater extent than mediations shortly after the meeting, something happens for victims in our context that produces a significant and reverse pattern at 6 months. Other data collected in the project points to the possibility that uncompleted outcome agreements may play an important role in explaining this finding. Although a thorough discussion of this issue is beyond the scope of this paper, we suspect that forming written outcome agreements in restorative justice conferences may introduce more concrete and potentially higher expectations for these victims as compared to victims receiving mediation, where written agreements were far less common. Danish conference victims may thus face an additional source of potential dissatisfaction if these agreements are later abandoned or victims are not informed as to whether they were upheld, which was often the case in this trial. As noted earlier, resource constraints in the Danish system precluded systematic monitoring of RJC agreements, which would otherwise have been desirable on both theoretical and practical grounds. Future work would benefit from further exploring the nature of these agreements and whether knowledge of their fulfillment moderates assessments of harm repair for conferences in particular.

A third conclusion that can be drawn from the study is that our findings generally suggest positive effects for both victims and offenders in both models. No significant differences between VOM and RJC emerge for offenders on any of our outcomes at either follow-up, whereas victims rate mediation even more positively than RJC for two of the outcomes at 6 months, as noted above. Given the victim-centered approach adopted in both Danish legislation and many international policy guidelines (Chapman et al., [2021](#); Rasmussen, [2024](#)), exploring whether varied restorative justice processes are met with similar or different outcomes for victims and offenders is important, as is considering their relative importance for theory and policy when they diverge (Gade, [2024](#)). Our primary analysis comparing 2-year offender recidivism for conferences and mediations is

still outstanding, and it will be interesting to see whether the direction of effect for that outcome mirrors or departs from the patterns observed here. Based on the self-reported outcomes explored in this article, however, our results as a whole support the claim of other restorative justice proponents that these processes are generally appreciated and experienced as beneficial by both victims and offenders, offering a win-win approach for the types of cases included here (Strang, 2002, 2012).

Returning to the general hypothesis motivating the study, our findings fail to support the theoretical proposition that RJC_s, as compared to mediations, would be experienced as more successful, fair, and better able to promote harm repair and closure. One possible explanation is that the restorative potential of the RJC model was hampered by real-world implementation challenges. Despite heroic efforts on the part of facilitators, it proved difficult to get victim and offender supporters in the room. While three quarters of conferences included at least one victim *or* one offender supporter, there were rarely more than two supporters present, and fewer conferences included supporters on both sides. The RJC process and “script” are designed with supporter presence in mind and may thus function sub optimally when supporters are absent. In this study, some victims and offenders actively resisted the idea of supporter involvement, while others lacked a social network from which to draw or couldn’t bring others due to practical or scheduling challenges. The mean number of supporters included in our conferences is slightly lower than in the Australian RISE trials (Strang et al., 2011), but mirrors averages in the UK experiments (Shapland et al., 2007). Difficulty including supporters poses both fidelity and feasibility issues for the RJC model that must be addressed. That this is a challenge observed in multiple settings suggests that it is not unique to the Danish context. More nuanced work exploring the various roles and impacts that supporters have in shaping restorative justice perceptions and outcomes is clearly needed (see Scheuerman & Keith, 2015).

A second implementation challenge, that may be more specific to the Danish setting, relates to the local organizational barriers that limited the ability for consistent monitoring of written agreements as discussed above. If not followed-up, outcome agreements that are meant to increase accountability and reparation in theory may have the opposite effect in practice. Challenges with establishing good follow-up procedures have also been noted in some other studies (see e.g., Shapland et al., 2007, 2024), which points to the importance of considering whether this element of the model may need to be adjusted, or the lack of follow-up clarified for participants, in some implementation contexts.

Another possible explanation for the results favoring mediation is that the relative flexibility afforded by the Danish mediation model provides an additional benefit to victims and offenders. As outlined earlier, Danish mediators operate with a high degree of methodological discretion to tailor their specific approach to each meeting. In comparison, the RJC model as it was implemented here, was considerably more standardized in its approach (not least to ensure internal validity in the experiment). Feedback from our project facilitators indicates that in cases lacking supporters or including young people who had communication difficulties, it could be challenging to fully adhere to the RJC model as trained (also see Riley & Hayes, 2018).

Finally, in relation to the majority of comparisons which indicated no significant differences between VOM and RJC, our null findings may reflect the possibility that the methodological differences between conferences and direct mediations are simply of less practical import for victims and offenders’ evaluations than theory suggests. Although

several key characteristics of the two models diverged as expected (see [Table 2](#)), there was still a considerable degree of overlap in the general design of the two interventions, in that both involved a face-to-face meeting and raised similar topics for discussion. It may thus be that the most important active ingredients driving these victim and offender ratings are present in both models. In line with other research ([Shapland et al., 2011](#); [Strang, 2002](#)), the victims and offenders in this study were primarily motivated to meet with the other party to receive/give an explanation, ask/answer questions, get/give an apology, and express how they were affected. Both interventions provided this possibility, albeit in slightly different ways. In short, when comparing two active restorative justice treatments, as opposed to such processes vs. court for example, shared underlying restorative goals and values may be more central to understanding process and outcomes than whether supporters are present or a “script” is followed (see e.g., [Chapman, 2012](#)). However, it is important to emphasize that we have tested these two approaches in a specific context, and it is not certain that the practices that we label “mediation” and “conferencing” correspond with those labels in other settings. In fact, [Zinsstag \(2012\)](#) has suggested that Nordic mediation may approximate conferencing to a greater extent than in some other jurisdictions, and the relatively facilitative nature of the more recently trained Danish mediation model may make this particularly so. As [Shapland \(2012, p. 48\)](#) has argued, “it is not possible to simply rely on the names of the restorative practices to delineate difference.” Additional descriptive research and randomized tests are needed in other settings to further clarify these issues. We would suggest that the aim of such work should not be to identify any single best practice model or to establish universal methodological standards, but rather to contribute evidence that can inform a suitable and safeguarded toolbox of services appropriate for the local context in which they are delivered ([Chapman et al., 2021](#); [Shapland et al., 2024](#)).

While we believe that the current study makes important strides toward this goal, indicating that both mediation and RJC processes offer useful tools in the Danish context, we recognize some limitations and caveats to the evidence produced in this study. One concern is the relatively small sample of cases included in the experiment. Recruitment into the study was severely hampered by a series of enforced COVID-19 lockdowns, during which no new enrollments or physical meetings could take place. This resulted in a smaller-than-intended final sample of 200 randomized cases, and a larger-than-hoped share of these in which a meeting was never held; in part, due to delays associated with the pandemic and a general sense of fatigue that led some participants to change their mind regarding participation. While this loss was balanced for the two treatment arms, it has considerable implications for the current analyses, as we were only able to assess participants’ satisfaction with their meetings for cases where a meeting, in fact, took place (77% of all randomized cases). We therefore can not explore potential dissatisfaction amongst participants who were randomized to receive a meeting which never materialized. The views of these potential participants are also important, but rarely captured (for an exception see [Strang, 2002](#)).

Our analyses are further limited to those who returned a survey response. As outlined in [Table 4](#), we achieved a very high case-level response rate with respect to the share of convened meetings represented with at least one survey response at 1 and 6 months (90% and 83% respectively), and our respondent-level rates at 1 month were also very satisfactory (79% of victims and 71% of offenders). However, these rates declined somewhat at 6 months for both victims and offenders, to between 53–74%, where RJC offenders had significantly lower

response rates than mediation offenders. Whether this attrition is likely to have affected our results is difficult to judge, but we cannot rule out the possibility of a self-selection bias, if the non-responders were systematically more satisfied or unsatisfied with their meetings than those who replied. In any case, it is an important caveat that the estimated treatment effects presented in this article are based on a limited sample of Danish criminal code cases, in which the parties agreed to meet and be randomized, later took part in a meeting, and returned a survey response.

It is similarly important to underline that our analysis is limited to exploring only four overall outcome ratings provided by these respondents approximately 1.5 and 7 months after their meetings. More work is needed to unpack these and other outcomes, explore statistical moderation and mediation effects, and extend follow-up times. In short, additional analysis is required before ultimate conclusions can be made regarding the relative values of mediation and conferencing. This also applies to drawing final policy conclusions from the KIP study itself. Importantly, the primary recidivism analyses for the experiment are still outstanding. Those results will provide an important additional dimension to the comparative analyses provided here, and they may not align with the present findings' directions or size of effect. We therefore urge readers not to draw premature conclusions based on the current analyses alone.

Finally, it is worth stressing yet again that future research is needed to establish the external validity of our findings. This relates not only to international contexts, as discussed above, but also to the wider Danish restorative justice landscape. Locally, the KIP experiment is anchored in seven of Denmark's 12 police districts and does not include all types of cases handled in the *konfliktråd* program (e.g., neighbor disputes). Given that other research indicates that there are considerable variations in how the victim-offender program operates across districts (Rasmussen, 2024), we cannot be sure that our results would fully replicate in the nonparticipating districts. However, the predominance of violence cases in our data reflects a general pattern in the national program, which makes it probable that at least some of our results would apply more broadly (Kyvsgaard, 2016). Restorative justice practices of varied types are also being used in other Danish settings, like workplaces and schools. While our criminal justice focus may not easily transfer to these contexts, our hope is that the general description of treatments and implementation findings presented here may nonetheless be useful in inspiring and clarifying shared practices and challenges that could help to advance the broader field, both nationally and internationally.

In conclusion, this analysis shows strong evidence of both parties receiving important benefits from the restorative processes tested here. With all the above caveats in mind, we feel confident in concluding that the generally positive ratings of both mediations and conferences demonstrated in this study support ongoing calls for more systematic access to high-quality and safeguarded restorative justice opportunities. That both victims *and* offenders in this and other studies largely find these encounters to be fair and helpful – a rare circumstance in criminal justice contexts – provides an especially compelling argument for dedicating the necessary resources to promote additional evidence-based practice, theory, and policy development in the field.

Notes

1. See Danish Ministry of Justice (2008, 2012) and Lov om konfliktråd (2009).

2. The participating police districts include Copenhagen, Northern Zealand, Southern Zealand and Lolland Falster, Funen, Northern Jutland, Eastern Jutland, and Southeastern Jutland.
3. These individuals are employed on a contract basis by their local police district and paid a small fee per meeting. They are not police officers and do not have state or legal authority in their function as meeting conveners. They have a variety of educational and employment backgrounds (e.g., social workers, teachers, retired pedagogues), and many convene victim-offender meetings in addition to their regular jobs.
4. It was the original intention of the researchers that a standard follow-up phase would be implemented in the RJC model. However, this proved impossible due to resource-constraints in the Danish program which precluded the facilitators' ability to dedicate the necessary additional time to such follow-ups, paired with the police's position that this was not their role. As the experiment was designed as a pragmatic effectiveness trial testing the real-world application of mediations and RJCs in the Danish context (rather than a highly constrained/idealized test of efficacy), a decision was made to proceed without this requirement in the RJC approach.
5. Based on power estimates for the experiment's primary recidivism outcome, we initially aimed to recruit a minimum 300 cases into the trial. Due to recruitment stops associated with multiple COVID-19 shutdowns, fewer cases could be enrolled, and the trial was not able to attain its intended size.
6. The 2010 Consolidated Standards of Reporting Trials (CONSORT) statement provides guidelines for transparent reporting of randomized controlled trials, including both a checklist and a flow diagram template illustrating case enrollment and attrition (see Schultz et al., 2010).
7. These specific case types were excluded in agreement with police, due to their potentially sensitive nature and unique dynamics. In a small number of cases, it became apparent post-randomization or during the meeting itself that partners or neighbors were, in fact, involved. In these situations, it was up to the meeting convenor to determine how best to manage the meeting and protect the participants.
8. The Research Electronic Data Capture (REDCap) platform is a secure online software application designed for constructing and managing research surveys and databases.
9. In three RJC cases, informed consent was formally withdrawn following randomization. In two cases, this occurred prior to convening the meeting itself. In accordance with local data protection requirements, all identifying information associated with these cases was deleted, and the individuals associated with these cases are therefore excluded from analysis.
10. In one mediation and one conference, a second meeting was arranged at the request of the parties. These follow-up meetings applied the same method as the first. Given the rarity of these cases and the balance between the two treatments, data was only collected regarding the first of these meetings.
11. In addition, three mediations were carried out online during COVID-19 shutdowns. Because a mediation was still held in these cases, they are retained in the count of cases receiving their allocated intervention. The small number of meetings that were held, but terminated early, are also included in this category.
12. Further analysis of processual differences based on additional survey and structured observation data is underway but beyond the scope of this article.
13. The four outcome variables were significantly but not perfectly correlated for both victim and offender respondent groups at 1 and 6 months, with Tau correlation coefficients ranging between .27–.61. The largest correlations were observed between the overall success rating and the three more specific ratings, while there was lower correspondence between the variables capturing fairness, repair, and moving on. This suggests that these variables (at least partly) measure different constructs, calling for separate analyses.
14. The decision to combine the mid-point neither/nor category together with the unsatisfied and very unsatisfied response categories was statistical and pragmatic, as attempts to exclude the neutral responses from the analysis or combine them with the successful/very successful responses either rendered the models unable to run due to low cell counts, or generated confidence intervals too wide to interpret.

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The first author has been paid for occasional teaching at the Danish National Police College. The last author is a mediator in the Danish victim-offender program but did not convene any meetings included in the project.

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Data availability statement

Due to ethical and legal restrictions governing our data collection and distribution, no supporting data is publicly available.

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Appendix

Table A1. Item-wise distribution of response categories for victims and offenders at 1 month by intervention type.

	Mediations			Conferences			All		
	Victim %	Offender %	Total %	Victim %	Offender %	Total %	Victim %	Offender %	Total %
The meeting was fair									
Yes, to a large extent	79.5	64.7	72.3	74.2	58.5	67.0	77.0	62.0	69.9
Yes, to some extent	15.1	32.4	23.4	17.7	34.0	25.2	16.3	33.1	24.2
No, not really	2.7	2.9	2.8	3.2	5.7	4.4	3.0	4.1	3.5
No, not at all	2.7	0.0	1.4	4.8	1.9	3.5	3.7	0.8	2.3
(n)	(73)	(68)	(141)	(62)	(53)	(115)	(135)	(121)	(256)
The meeting helped me move on									
Yes, to a large extent	30.1	27.9	29.1	32.3	35.8	33.9	31.1	31.4	31.3
Yes, to some extent	48.0	52.9	50.4	40.3	39.6	40.0	44.4	47.1	45.7
No, not really	12.3	13.2	12.8	22.6	18.9	20.9	17.0	15.7	16.4
No, not at all	9.6	5.9	7.8	4.8	5.7	5.2	7.4	5.8	6.6
(n)	(73)	(68)	(141)	(62)	(53)	(115)	(135)	(121)	(256)
The harm has been repaired									
Yes, to a large extent	19.2	26.5	22.7	25.8	28.3	27.0	22.2	27.3	24.6
Yes, to some extent	39.7	44.1	41.8	40.3	49.1	44.4	40.0	46.3	43.0
No, not really	27.4	20.6	24.1	17.7	18.9	18.3	23.0	19.8	21.5
No, not at all	13.7	8.8	11.4	16.1	3.8	10.4	14.8	6.6	10.9
(n)	(73)	(68)	(141)	(62)	(53)	(115)	(135)	(121)	(256)
How would you rate the meeting overall?									
Very successful	34.2	42.7	38.3	27.4	35.9	31.3	31.1	39.7	35.2
Successful	48.0	41.2	44.7	45.2	34.0	40.0	46.7	38.0	42.6
Neither/nor	11.0	13.2	12.1	24.2	30.2	27.0	17.0	20.7	18.8
Unsuccessful	2.7	1.5	2.1	1.6	0.0	0.9	2.2	0.8	1.6
Very unsuccessful	4.1	1.5	2.8	1.6	0.0	0.9	3.0	0.8	2.0
(n)	(73)	(68)	(141)	(62)	(53)	(115)	(135)	(121)	(256)



Table A2. Item-wise distribution of response categories for victims and offenders at 6 months by intervention type.

	Mediations			Conferences			All		
	Victim %	Offender %	Total %	Victim %	Offender %	Total %	Victim %	Offender %	Total %
The meeting was fair									
Yes, to a large extent	66.7	60.7	63.8	53.8	61.9	57.5	61.0	61.2	61.1
Yes, to some extent	24.2	34.4	29.1	34.6	28.6	31.9	28.8	32.0	30.3
No, not really	6.1	3.3	4.7	7.7	4.8	6.4	6.8	3.9	5.4
No, not at all	3.0	1.6	2.4	3.8	4.8	4.3	3.4	2.9	3.2
(n)	(66)	(61)	(127)	(52)	(42)	(94)	(118)	(103)	(221)
The meeting helped me move on									
Yes, to a large extent	31.8	9.8	21.3	25.5	21.4	23.7	29.1	14.6	22.3
Yes, to some extent	42.4	54.1	48.0	43.1	42.9	43.0	42.7	49.5	45.9
No, not really	13.6	32.8	22.8	15.7	19.0	17.2	14.5	27.2	20.5
No, not at all	12.1	3.3	7.9	15.7	16.7	16.1	13.7	8.7	11.4
(n)	(66)	(61)	(127)	(51)	(42)	(93)	(117)	(103)	(220)
The harm has been repaired									
Yes, to a large extent	27.3	26.2	26.8	23.1	28.6	25.5	25.4	27.2	26.2
Yes, to some extent	40.9	45.9	43.3	21.2	45.2	31.9	32.2	45.6	38.5
No, not really	18.2	24.6	21.3	26.9	19.0	23.4	22.0	22.3	22.2
No, not at all	13.6	3.3	8.7	28.8	7.1	19.2	20.3	4.9	13.1
(n)	(66)	(61)	(127)	(52)	(42)	(94)	(118)	(103)	(221)
How would you rate the meeting overall?									
Very successful	31.8	29.5	30.7	25.0	35.7	29.8	28.8	32.0	30.3
Successful	48.5	49.2	48.8	36.5	28.6	33.0	43.2	40.8	42.1
Neither/nor	15.2	19.7	17.3	25.0	28.6	26.6	19.5	23.3	21.3
Unsuccessful	3.0	1.6	2.4	9.6	2.4	6.4	5.9	1.9	4.1
Very unsuccessful	1.5	0.0	0.8	3.8	4.8	4.3	2.5	1.9	2.3
(n)	(66)	(61)	(127)	(52)	(42)	(94)	(118)	(103)	(221)