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Rapport-building in multiple interviews of children

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Abstract

Rapport-building is key in child investigative interviews, however, recommendations of how to build rapport differ. Additionally, rapport in more complex situations: when a child is interviewed repeatedly or requires separate rapport building have not been studied. This research examined the UK's 'Achieving Best Evidence' guidelines for rapport-building, which recommend conducting a neutral discussion, compared with a control condition and a separate rapport-building session for first interviews on children's recall and well-being (measured by state anxiety and rapport questionnaires). For second and third interviews, additional full rapport-building sessions were compared to shortened or no rapport-building conditions. No significant differences in children's ($N = 107$) recall or well-being were found across rapport-building conditions for all interviews. We conclude that for children who have experienced non-traumatic events, the inclusion of a neutral discussion rapport-building phase may not be any more beneficial for children than conducting a friendly interview.

KEYWORDS
child victims, eyewitness testimony, investigative interviews, multiple interviewing, rapport-building

1 | INTRODUCTION

1.1 | Rapport-building and its effects

Despite the challenges with defining the psychological experience of rapport and the variety of suggested methodologies for building rapport in investigative interviewing (Bull & Baker, 2020; Gabbert et al., 2021; Saywitz et al., 2015), rapport-building has been identified as important in most child interviewing guidelines (e.g., the Achieving Best Evidence [ABE] guidelines, Ministry of Justice, 2022; the Revised NICHD, Lamb et al., 2018). One of the most influential descriptions of rapport in the investigative interviewing literature is Tickle-Degnen and Rosenthal's (1990), which splits rapport into three components: mutual attentiveness (paying attention to and being involved in what the other person is saying), positivity (mutual friendliness), and co-ordination (both parties should be able to work together). In the context of a forensic interview, rapport is thought to lead to a more comfortable relationship between the interviewer and interviewee (R. Collins et al., 2002). Importantly, although rapport is an interpersonal phenomenon, one conversational partner's intention to build rapport with the other will not necessarily result in the mutual experience of rapport (Abbe & Brandon, 2013).

There is a reasonably large body of literature demonstrating that socially supportive techniques (which are intended to build rapport) have beneficial effects on children's experiences of investigative and mock-investigative interviews and the quality and quantity of their accounts (e.g., Almerigogna et al., 2007; Davis & Bottoms, 2002;

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Hershkowitz et al., 2015; Roberts et al., 2004; Saywitz et al., 2019; Vallano & Schreiber Compo, 2015). Interviewer-provided social support in such studies vary from simple non-verbal techniques (smiling) to detailed suggestions of wording to respond to child reluctance (e.g., the Revised NICHD guidelines; Lamb et al., 2018). The benefits observed include reducing children's anxiety, increasing children's recall, increasing their resistance to misleading suggestions, and reducing their resistance to disclose details. Thus, including rapport-building techniques in child interviews in order to make them feel socially supported (and thus hopefully perceive rapport with the interviewer) is likely to result in positive outcomes for the child and the investigation.

1.2 | The abe guidelines and the neutral discussion rapport method

To reap these benefits, the ABE guidelines (Ministry of Justice, 2022) describe the first phase of the interview as a phase which establishes rapport prior to discussing the substantive issues of the investigation. To establish this rapport, interviewers are recommended to conduct a brief discussion with the child on a neutral topic that can be answered positively. Many other interviewing guidelines include similar instructions, often alongside other techniques that may be conducted during this beginning phase (such as a practice interview, Scottish Government, 2011), or throughout the interview (such as encouraging the child by telling them how well they are doing [but not in a praising/rewarding way], Revised NICHD, Lamb et al., 2018). In the ABE, this neutral discussion is where rapport-building is emphasised, and the present study focuses specifically on this, rather than being generally supportive towards a child throughout their interview (see Saywitz et al., 2015 for a more detailed discussion of supportiveness and rapport).

The majority of research examining rapport-building phases in children's interviews using neutral discussions has focused solely on outcomes associated with the quality and quantity of recall obtained, rather than children's well-being (i.e., their psychological welfare) or perceptions of rapport (e.g., Brown et al., 2013; Hardy & van Leeuwen, 2004; Roberts et al., 2004; Sternberg et al., 1997). Furthermore, these experimental studies have not included a control group in which no rapport-building phase was included and everything else kept constant, but instead have compared only 'good' to 'poor' rapport-building techniques. Thus, although these studies have found some benefits of 'good' rapport-building in comparison to 'poor', there remain some key gaps in our knowledge. Firstly, do these rapport-building techniques increase children's feelings of rapport with the interviewer or are the benefits of such rapport-building techniques caused by other variables (e.g., a better understanding of how to respond to open questions)? And second, is any rapport-building (good or poor) an improvement on conducting an interview in a friendly manner?

The very limited literature that has solely focused on neutral discussion rapport-building suggests that when it is performed well this technique has no significant impact on children's well-being (as measured by their state anxiety and heart rate, which may indicate comfort and rapport with the interviewer) nor on their recall in comparison to a control group (K. Collins, 2012). Experimental studies such as Collins' and the current study are subject to ethical constraints which may limit the application/generalisation of the findings to forensic interviews. Specifically, the children involved are not exposed to a particularly stressful to-be-remembered event and are generally interviewed in a known, safe environment with no possible negative repercussions (unlike being the victim of abuse). Thus, although they may still need rapport to engage productively with the stranger-interviewer, the requirement for this may be lower, plus creating rapport may be easier as the child has no reason to feel reluctant to talk. Therefore, the need for additional rapport-building techniques may be surplus to requirement. However, if a rapport-building technique is found that is effective in these low-trauma settings, it may be particularly beneficial for children experiencing forensic interviews. K. Collins' (2012) findings imply that although other studies have found good rapport-building to be beneficial for children's recall in comparison to poor rapport-building, it does not improve children's recall or well-being in comparison to well conducted interviewing that involves no particular rapport-building – thus this particular form of rapport-building (i.e., discussing a neutral topic) may not work in more high-stress situations.

The possibility that a poorly conducted rapport-building phase may negatively affect children's recall in comparison to no rapport-building is particularly worrying given that the rapport-building phase in investigative interviews has often been found to be poor (Westcott & Kynan, 2006; Wood et al., 1996; Yi et al., 2015 – also see Walsh & Bull, 2012 who found that poor rapport-building was associated with lower information gain in real-life interviews with suspects). Thus, if a poorly conducted rapport could have negative effects and a well conducted rapport-building phase (conducted in line with 'official' guidance documents – see above) does not benefit children's recall, it may be that the inclusion of rapport-building with children in the recommended form of chatting about a neutral event has some risks. The current research intends to address some gaps in our knowledge by including a control group and by directly measuring children's perceptions of rapport (using a novel questionnaire).

1.2.1 | Length of rapport-building

A further concern relating to a rapport-building phase preceding a child's investigative interviews is its length. Burrows and Powell (2014), for example, found some prosecutors were concerned that long rapport-building might tire children. Teoh and Lamb's (2010) examination of forensic interview transcripts found the more rapport-building prompts interviewers used, the less informative children were, which they argued could be due to tiredness. This possibility has also been mentioned in the experimental literature. Although no studies have directly examined the impact of length of rapport-building on children's recall, Roberts et al.'s (2004) study found no difference in total recall between their 'good' (using open-ended questions) rapport-building condition and 'poor' (using a direct questioning style of wh- and yes/no questions) condition. Roberts et al. (2004) suggested that this may have been because open-ended rapport-building was too long (on average 16 min in comparison to 6 min for direct) and that children were too tired to recall more. Additionally,

Davies et al. (2000) found that shorter rapport-building sessions (less than 8 min) were associated with children providing longer answers in the interview. Davies et al. (2000) suggested this could be due to children who had longer rapport-building becoming more tired. Alternatively, it may be that the interviewers noted children's reluctance and so attempted to build rapport with these children for a longer period of time. The ABE guidelines do not make any recommendations regarding the optimal length of rapport-building, other than stating it should be 'completed within a relatively short space of time' (Ministry of Justice, 2022, p. 78). However, it states that open-ended questions should be used, and some researchers have called for interviewers to use open questions and to encourage children to talk at length during the rapport-building phase in order to increase their recall in the substantive phase (e.g., Wood et al., 1996).

One way of overcoming concerns regarding over-lengthy rapport-building is to have a separate meeting with the child prior to the substantive session of the interview (an option encouraged in the ABE for complex cases, Ministry of Justice, 2022). No research has looked at the impact of a separate, pre-interview rapport-building session using the ABE. However, researchers examining the Revised NICHD protocol in field interviews have investigated a similar option in which interviewers who face intense reluctance to disclose by a child focus the first interview solely on building rapport, and then conduct a separate second interview (including further rapport-building) to attempt to reduce reluctance and increase child disclosure (Blasbalg et al., 2021; Hershkowitz et al., 2021). Although this does not create complete separation of (i) the rapport-building from (ii) the interview (and thus may still result in children tiring from the rapport-building phase in the actual interview), their results suggest that rapport-building conducted separately from the interview had beneficial impacts in reducing children's reluctance to disclose in the actual interview. This implies that separating the rapport-building from the main interview should not undo any positive impact of rapport-building on children's recall. The current study aims to examine if this is the case with the support as described in the current national ABE rapport-building guidelines (Ministry of Justice, 2022).

1.2.2 | Multiple interviews and rapport-building

Another area of practice where there is no research to aid interviewer's decision-making is whether and how to build or maintain rapport across multiple interviews (when a child is interviewed more than once about the same alleged offence). The ABE guidelines (Ministry of Justice, 2022) generally discourage multiple interviews despite the opportunity for reminiscence (recalling in second or third interviews additional details that had previously been forgotten or withheld, e.g., La Rooy et al., 2007; Waterhouse et al., 2016). However, they stipulate some situations in which they may be required, including cases where it may be planned in advance to separate the interview into multiple parts across different days for particularly vulnerable children. In these cases, they suggest that rapport is built for each separate interview. This may confuse children who could believe they are in a subsequent interview to talk about the event and not an unrelated neutral event. On the other hand, additional rapport-building may have additive benefits for decreasing children's anxiety. A second interview rapport-building session may be particularly vital in cases in which the interviews occur with large delays between them, when the child may not remember the interviewer or their previous rapport, or for anxious children. The present study, therefore, acts as a vital and innovative study of the interaction between rapport-building conditions and multiple interviews.

1.2.3 | The present study

The present study will thus add to the rapport-building literature in a number of ways. We include a no-rapport condition in order to compare this to rapport-building as currently recommended in the United Kingdom. The rapport-building section in the current ABE guidelines and in previous versions (Ministry of Justice, 2011, 2022) is based on the presumption that this particular form of rapport-building will improve children's accounts and their experience of the interview. We will test this hypothesis across first, second and third interviews with children (see Table 1 for the study conditions in detail). Furthermore, we will measure the impact of rapport-building conditions on children's recall and also on measures of their well-being (state anxiety and perceived rapport, given that rapport is likely to result in a more comfortable relationship between the interviewer and interviewee, we argue that the level of rapport the child feels will affect their well-being during the interview) to attempt to determine rapport built.

Additionally, we consider the use of a separate rapport-building meeting to compare this with both (i) no rapport (*control* condition) and (ii) rapport-building at the beginning of the interview (*combined* condition). As an exploratory aspect of the study, children's well-being and recall may differ if the rapport-building phase is conducted the day before the interview rather than immediately before, but we do not make any predictions on the direction of any differences.

We also examine different rapport durations (*brief* and *standard*) in second and third interviews and their possible effects on children's testimony. Interviewers may feel that it is not necessary to complete another full rapport-building session with a child whom they have already interviewed. Therefore, in the present study, we investigate whether a shortened rapport-building (brief) is sufficient in comparison to another full rapport-building session (standard) in these 'repeat' interviews. We expect the rapport-building conditions children experience in their second and third interviews will impact their recall and well-being in these interviews, including reminiscence (new details provided only in second and/or third interviews).

2 | METHOD

2.1 | Sample

Prior to data collection, the study received approval from the lead author's university research ethics committee. This study was not

TABLE 1 Description of group conditions.

Group (*n*)		Initial rapport condition	Interview 1	Interview 2	Interview 3
1 (*24*)	Event	*Control*: No Rapport	Recall	*Control*: No rapport + Recall	*Control*: No rapport + Recall
2 (*21*)	Event	*Combined*: Rapport during interview one	Recall	*Standard*: Standard length rapport + Recall	*Standard*: Standard length rapport + Recall
3 (*22*)	Event	*Combined*: Rapport during interview one	Recall	*Brief*: Brief rapport + Recall	*Brief*: Brief rapport + Recall
4 (*20*)	Event	*Separate*: Rapport day before interview one	Recall	*Standard*: Standard length rapport + Recall	*Standard*: Standard length rapport + Recall
5 (*20*)	Event	*Separate*: Rapport day before interview one	Recall	*Brief*: Brief rapport + Recall	*Brief*: Brief rapport + Recall
Timing	Day 1	Days 8–9	Day 9	Day 16	Day 23

preregistered. Data were collected with $N = 107$ children from seven different primary schools in London (England) and the surrounding counties. According to the 2011 census, the majority of the population in the schools' local authority areas identified as white (57.1% to 90.9%, Office for National Statistics, 2023) and 24.6% to 38.4% fell into the highest socioeconomic status category (higher and intermediate managerial, administrative, or professional occupations, Office for National Statistics, 2011). Children with special educational needs or an anxiety disorder ($n = 6$) or who reported absolutely nothing about the to-be-remembered event ($n = 1$) were not included in this final sample. This sample size ($N = 107$) is larger than previous published multiple interviewing studies (e.g., Bruck et al., 2002; La Rooy et al., 2007; Salmon & Pipe, 1997), and there are a similar number of children per condition in the present study as in previous rapport-building research (e.g., $n = 24$, Roberts et al., 2004; $n = 25$, Sternberg et al., 1997). Whilst our sample size was defined by convenience (as is often the case with difficult to access populations), a sensitivity analysis (of a 5×3 mixed ANOVA targeting 80% power) suggests that this sample is sufficient to reliably detect effects larger than $\omega^2 = 0.07$ (a medium effect). Children were recruited from two school years and this resulted in the sample's ages ranging from 87 to 128 months old ($M = 107.83$, SD = 8.29). These ages were selected as they represent groups often studied in the rapport and multiple interviewing literature and thus viewed as a key group for effective interventions (Saywitz et al., 2019; Waterhouse et al., 2020a). All participants had had multiple years of formal education and hence were accustomed to responding to questions from adults to assess their understanding. There were slightly more girls (54.2%) than boys. Due to absences, one boy only experienced one interview, and three boys and three girls were only interviewed twice, and so these children's responses were included solely in analyses relating to the first interview only.

2.2 | Materials

2.2.1 | Spielberger's State–Trait Anxiety Inventory for children (STAI-C)

With the help of a research assistant, children completed the 20-item state anxiety questionnaire from the STAI-C, measuring the child's level of anxiety at the time of questionnaire completion. This questionnaire has been found to have good validity and reliability (Spielberger et al., 1973). The state anxiety questionnaire involves the child deciding which of three options about their feelings is most appropriate at that moment. For example, one question involves choosing between 'I feel very cheerful', 'cheerful' or 'not cheerful'.

2.2.2 | Stimulus film

The to-be-remembered (TBR) event was a four-minute film depicting the non-violent theft of a lady's handbag on a street. The film had previously been used in published research as the to-be-remembered event (Dando et al., 2011). Due to the non-violent nature of the crime, the film provided an event that the police could investigate that was suitable to show children. Ethical and practical considerations (including interviewing children from multiple different schools) meant using a video was preferable to a live event for ensuring the consistency of the TBR event.

2.2.3 | Rapport-building sessions

All of the rapport-building sessions were modelled on the ABE's (Ministry of Justice, 2011) instructions (which are the same in the updated 2022 guidance); the interviewer asked the child open-ended questions about a neutral event (e.g., 'Tell me everything about your school trip'). The child was then prompted with further open-ended questions, minimal prompts (e.g., repetitions of what they have said, nodding, 'uhuh') and wh- questions until they had nothing more to say about the event or the session had taken 10 min (whichever came first). The ABE guidelines (Ministry of Justice, 2022) give no information about the optimal length of the rapport-building phase. Thus, the rationale for limiting the *combined* and *standard* rapport-building to 10 min is that Davies et al. (2000) found that the median length of rapport-building in child forensic interviews was 8 min, which suggests that 10 min is a realistic length, verging on the generous. This is therefore likely to result in a greater chance of rapport being built as the child is being given longer to share and get to know the interviewer. For children's first interviews, the rapport-building was

conducted identically regardless of whether it was conducted just before the mock-interview (*combined* condition), or the day before (*separate* condition). The average *combined* rapport-building session took 7.61 min (SD = 2.40).

Standard and brief rapport-building

For children in the rapport-building conditions, they either experienced *standard* or *brief* rapport-building sessions in their second and third interviews. Each rapport-building session the child experienced focused on a different neutral event. *Standard* rapport-building sessions took up to 10 minutes and were conducted in the same manner as the first rapport-building session they experienced. *Brief* rapport-building sessions began with an open-ended question about a neutral event, but this was followed up with only two prompts for further information. If the child provided a lot of information in response to the first question, fewer prompts were used so that this rapport-building ideally took less than 5 min. The average *standard* rapport-building session for the second interview lasted 8.06 min (SD = 2.63) and 8.35 min (SD = 2.35) for the third interview. The average *brief* rapport-building session took 3.46 min (SD = 2.48) for the second interview and 3.52 min (SD = 1.46) for the third interview. The *standard* rapport-building was significantly longer than the *brief* rapport-building in both interviews two, $t(65) = -7.39$, $p < .001$, $d = 1.80$, and three, $t(50.6) = -9.67$, $p < .001$, $d = 2.50$.

2.2.4 | Mock-investigative interviews

The mock investigative interviews began, as 'officially' recommended (see ABE), with a 'ground rules' section. The child was given information as to what to expect during the interview (e.g., free recall followed by questions), and they were advised of a number of 'rules' for the interview. These were that (a) the child should correct the interviewer if she said something incorrect or misunderstood the child, (b) the child should tell the interviewer if she asked a question the child did not understand, and (c) that responding 'I don't know' was acceptable (Ministry of Justice, 2022). Additionally, the interviewer pointed out that she was not at the event, and therefore did not know what happened. The children were also told that they should provide as much detail as possible. ABE (Ministry of Justice, 2022) advocates going through the ground rules at the same time as rapport-building, but 'ground rules' are arguably not a form of rapport-building in themselves. They provide instructions for the interview, and so the 'ground rules' were included at the beginning of the mock-interview rather than at the end of the rapport-building for those in the *separate* rapport-building conditions.

'Ground rules' were followed by clarification of the child's understanding of truth and lies. This was conducted as recommended by ABE for younger children (Ministry of Justice, 2022; see Appendix A below for details).

After the truth and lies conversation, the substantive section of the interview began. Children were initially encouraged to provide free recall of the filmed event by the interviewer saying 'Tell me everything that happened in the film'. Further disclosure was supported with minimal prompts (e.g., repetitions of what they have said, nodding, 'uhuh'). When the children had exhausted their free recall, directive prompts were used to try to obtain more detail (e.g., 'You said there was a man, tell me everything you remember about him'), until it was felt that the children had given their full recall. Suggestive questions were avoided, but option-posing and closed questions were used in some cases to clarify children's responses. At the end of the interview, their recall was summarised. This was based on the notes that the interviewer had taken. The interviewer used the child's own terms (i.e., referring to the 'man in the black cap') and covered all the key information the child mentioned. The children were then asked 'Is there anything else you remember about the film?'. If they remembered additional details, they were asked again if there was anything else until they communicated there was nothing further they remembered.

At the beginning of the second and third interviews, children were informed the interviewer would like to speak to them again about the film they watched and asked if that would be ok. They were not given a specific reason, but if they asked why, they were told this was because sometimes when people are asked to remember something again, they remember new details. The second and third interviews were conducted in an identical manner to the first, except that for the second interview only, children were asked one additional question at the end of the interview. This question was 'I heard that the man who stole the bag bumped into someone. Do you know anything about that?' The thief did bump into someone in the to-be-remembered event, and this question was asked to simulate the situation where a second interview is conducted in order to obtain information about a particular (not yet recalled) aspect of the event. Responses to this question were not further analysed (because responses to this specific question were not thought to be relevant to the key aims of this study) and if the child had already recalled the thief bumping into someone ($n = 8$), this question was not asked.

2.2.5 | Rapport questionnaire

After each interview, children completed the rapport questionnaire. This questionnaire was designed for the present study. Based on Duke et al. (2018), seven statements were created for the questionnaire related to factors that had been found to influence adults' perceptions of rapport ('professional expertise' and 'professional dedication' were removed as they were thought to be inappropriate for child perceptions). These statements also addressed Rotenberg et al.'s (2003) indicators of good rapport (children's perceptions of adult trustworthiness and likeability). The statements were reviewed by a developmental psychologist (Dr. Kim Collins) resulting in simplifications and three additional statements. Two of these addressed Tickle-Degnen and Rosenthal's (1990) components of rapport (mutual attentiveness, positivity, and co-ordination) more directly, and one acted as a dummy question to ensure the child understood the format of the questions. The resulting questionnaire (see Appendix B) included statements such as 'The interviewer listened to me during

the interview', to which the child could respond 'None of the time', 'Some of the time', or 'All of the time'.

All of the statements in the questionnaire were positive, and so children's responses were scored from one point for 'None of the time' to three points for 'All of the time'. The dummy question (I wear my school uniform at school) was not included in the final score, and so the maximum possible score (indicating very good rapport) was 27.

We conducted a two-way random effects intraclass correlation coefficient to test the reliability of the measure. The results suggest an acceptable internal reliability of ICC = 0.69, 95% CI [0.63, 0.73], $p < .001$. However, it should be noted that for all items, the modal response was the highest rating (3), with 62%–97% of the responses to individual items being '3' responses. A majority reported a high experience of rapport.

2.3 | Procedure

Children were exposed to four or five sessions over a 4-week period, depending on their rapport-building conditions (see Table 1). The children were allocated randomly (within their genders) to groups. All of the sessions took place in their primary school, during lessons, at a time convenient for their teacher. Each child completed the sessions individually. Sessions were conducted in as quiet and private an environment as possible, but these locations often changed across sessions due to school timetables.

In the first session, the child watched the filmed event with a research assistant (who did not conduct the interviews). The second session (1 week later) only took place for children who experienced *separate* rapport-building, and these children met with the interviewer for the rapport-building session. The third session (8 days after session one), which all children took part in, consisted of the first investigative interview. Depending on their group, children experienced this interview without any rapport-building (*separate* rapport-building groups, and the *control* group) or with rapport-building just prior to the interview (*combined* rapport-building groups). Immediately after the mock-interview, all children were directed to a research assistant and completed a rapport questionnaire and the state anxiety questionnaire. The research assistant was not the person who had interviewed the child, and she informed the child that the interviewer would not know the answers had come from that child. The research assistant also offered to read the questions aloud for the child and answer any questions they might have about the questionnaires.

The fourth and fifth sessions were further investigative interviews. The fourth occurred 1 week after the third session and the fifth a week after that. Children experienced *standard*, *brief* or no rapport-building (*control*) prior to these interviews (see Table 1). They completed a rapport questionnaire and the state anxiety questionnaire with a research assistant after each interview. Thus, in total, all children who were interviewed three times completed three state anxiety questionnaires and three rapport questionnaires. The two interviews were identical, so if the child experienced a *standard* rapport-building session prior to their mock-interview in session four, they received the same in session five. The same interviewer (the first author) conducted all of the interviews and the rapport-building with all of the children.

When all of the participating children in the class had completed all of the sessions necessary, verbal de-briefing was conducted with the entire class. This allowed all children an opportunity to ask questions even those who had not taken part but still wanted to find out about the research, and the class was given a group gift for their participation and help. Teachers were given a second opportunity for de-briefing during an optional workshop offered to the schools.

2.4 | Investigation-relevant coding

A list of the main details of the to-be-remembered event was made. In order to determine investigation-relevance, police officers were asked to rate the items on this list as of high or low investigation-relevance (as in Wright & Holliday, 2007). They could also add further details that they thought were important. Details were coded in this manner (rather than central vs. peripheral details) to determine the importance of the details remembered by the children for investigative practice. Although central details are often thought to be key to the investigation, asking experienced practitioners to state the details that are most likely to impact investigative decision-making and case progression ensures that they are useful in practice, not just narratively. Furthermore, details that may seem peripheral (either in terms of where they are within the physical space or in relevance to the 'story' of the event) may be of importance to the investigation and this method ensures this was not overlooked. Five police officers (three male, two female) viewed the to-be-remembered event and made decisions as to the investigation-relevance of these details. On average, these police officers had 17.2 years of service (SD = 6.22) and 16.1 years of investigative interviewing experience (SD = 5.12). Details were coded as of high investigation-relevance if at least three of the officers rated it so. All the other details were coded as of low investigation-relevance. The percentage of the details thus coded as of high investigation-relevance was 76.0% (168 of 221 details).

2.5 | Coding of interviews

The information the children provided was coded for accuracy, consistency and novelty, and investigation-relevance. A template for coding the number of details was created (as in Wright & Holliday, 2007). Details from the film (classified as of high or low investigation-relevance as described above) were listed. Each piece of information provided was classified as one detail. For example, 'the two women were walking' would result in three details being scored. Every additional detail was scored separately, so for example, 'the two women were walking down the high street' would count as four details.

2.5.1 | Accuracy

Each detail the child provided was compared to the film. Details that correctly described what happened in the film were coded as

'correct'. Details that were somewhat correct, such as saying the man was wearing a black hat, when he in fact was wearing a black hood were coded as 'incorrect' (although in this example they would also get a correct point for 'black'). Details that were completely incorrect, such as the child recalling seeing a police officer in the film (when there was none), were coded as 'confabulations'. Proportion of accurate details was calculated by dividing the number of correct details by total details (correct, incorrect and confabulated).

2.5.2 | Consistency and novelty

For children's second and third interviews, the details they provided were categorised as 'new' (i.e., the child had not mentioned the detail in his/her previous interview/s), or 'repeated' (i.e., the child had mentioned the detail in his/her previous interview/s).

2.6 | Inter-rater reliability

A second rater coded 19% of the children's interviews (i.e., all three interviews for 20 children). This sub-sample was randomly determined. During the first meeting between the two raters, the first rater explained the coding system and process to the second rater with opportunities for questions. The second rater then coded a child's interviews and the two raters met again to discuss any disparities in their codes for these interviews. This discussion resulted in some new coding rules (e.g., correct coding for the victim's hair colour was relaxed to include black and brown due to ambiguity in the film). Once the second rater had coded the remaining interviews within the sub-sample, two-way random intraclass coefficients were conducted of the two raters' coding results. This analysis found varying agreement (between ICC = 0.33 for second interview contradictions to ICC = 0.90 for first interview correct details). For best practice, a further meeting between the two coders resulted in 100% agreement on all codes for all 60 transcripts and a refined coding scheme. The agreed coding rules were used by the first rater for coding all the remaining transcripts.

2.7 | Data analysis

The critical interview variables in this study are the accuracy (proportion of correct information), correct high-relevance information, new accurate information in later interviews, new correct high-relevance information in later interviews, and correctly repeated information in later interviews. All interview variables are presented as percentages of overall interview length so as to reflect the overall usefulness of those statements and not just longer statements. Also available are two measures of participant experience in their responses to the rapport and state anxiety questionnaires.

For all dependent variables, data were cleaned based on considering atypical scores outside of an absolute z score of 3 ($z > |3.00|$) as outliers. Initial checks on data involved the assessment of the covariance between the critical variables using Pearson's r correlations. Test of hypotheses are conducted with mixed 5 × 3 ANOVA testing the effect of (i) interview grouping (five levels—Control, Combined and Brief rapport, Combined and Standard rapport, Separate and Brief rapport, and Separate and Standard rapport) and (ii) interview number (first, second and third interview) on the critical variables of interest (accuracy, high relevance accuracy, accuracy of new details in later interviews, accuracy of new high relevance details, accuracy of repetitions, rapport questionnaire scores and state anxiety scores). Due to the number of repeated tests being conducted, inference will be drawn from a conservative $p < .005$ (exact p values are reported throughout) and from estimates of effect size. For the ANOVA, this will involve interpreting omega squared (ω^2, from MOTE; Buchanan et al., 2019) with a minimal notable ω^2 of 0.04 (in line with regularly cited practical inference; Ferguson, 2009) with 95% CI of ω^2 not overlapping 0. Data, study materials, and analysis code will be made available upon reasonable request by contacting the first author.

3 | RESULTS

3.1 | Measured interview variables

Most interview variables in this study were generally normally distributed (see Table 2). The notable exception was the responses to the rapport questionnaire, where these were negatively skewed towards a maximum score. Where the possible total on the measure was 27, across all interviews and all time points the average rapport score was 25.71. In terms of general description of the interview performance, the sample were around 70% accurate (see Table 2) in terms of general details, high relevance details and in the details they repeated between interviews. Accuracy of new details provided later on was lower, with only approximately 55% accuracy (see Table 2) of new details raised in the second and third interview (regardless of high or general relevance).

Table 3 reports the correlations between the interview outcome variables across all cases (all three interviews analysed in one dataset). There was convincing evidence that the interviews which were more accurate in one type of detail were generally accurate in other domains. Whilst these correlations were generally high, the correlations were weaker for reporting for accurate new and repeated details across interviews (i.e., $r = 0.37$, see Table 3).

3.2 | Differences between interview groups on outcomes

Table 4 reports the tests of difference across the five interview conditions over the three (or two for repetition variables) interviews regarding the key interview outcomes (see supplementary materials for *M*s and SDs of individual conditions). There was no clear consistent evidence of rapport group having an impact on the accuracy of details

TABLE 2 Descriptive statistics for critical interview variables over the multiple interviews in all conditions.

Variable	Interview	*N*	Mean (SD)	Skewness	Kurtosis (S.E)
Accuracy of all details (/100)	First	107	72.16 (10.72)	−0.37	0.21 (1.04)
	Second	106	70.85 (10.29)	−0.30	0.08 (1.00)
	Third	100	70.38 (11.63)	−0.37	−0.10 (1.16)
Accuracy of high relevance details (/100)	First	107	72.37 (10.68)	−0.28	−0.25 (1.03)
	Second	106	70.77 (10.46)	0.09	−0.44 (1.02)
	Third	100	70.33 (11.50)	−0.20	−0.46 (1.15)
Accuracy of new details (/100)	Second	105	57.44 (15.59)	0.26	−0.17 (1.52)
	Third	99	52.27 (20.66)	0.24	−0.12 (2.08)
Accuracy of new high relevance details (/100)	Second	105	56.98 (16.37)	0.32	0.06 (1.60)
	Third	99	52.62 (21.72)	0.26	−0.22 (2.18)
Accuracy of repetitions (/100)	Second	106	78.84 (9.10)	−0.10	−0.11 (0.88)
	Third	99	76.96 (8.81)	−0.32	0.45 (0.89)
Rapport Questionnaire score (/27)	First	99	25.71 (1.22)	−0.69	−0.27 (0.12)
	Second	104	25.74 (1.47)	−1.19	0.57 (0.14)
	Third	98	25.67 (1.62)	−1.16	0.56 (0.16)
State Anxiety Questionnaire score (/60)	First	100	27.97 (5.08)	0.67	0.51 (0.51)
	Second	104	26.77 (5.01)	0.54	−0.09 (0.49)
	Third	97	26.82 (5.11)	0.48	−0.45 (0.52)

TABLE 3 Correlations between critical variables in the study as Pearson's *r* [*N*] (exact *p* values) across all interview cases.

Interview variables	1	2	3	4	5	6
1. Accuracy of all details	-	-	-	-	-	-
2. Accuracy of high-relevance details	**0.95 [313] (<.001)**	-	-	-	-	-
3. Accuracy of new details	**0.73 [204] (<.001)**	**0.70 [204] (<.001)**	-	-	-	-
4. Accuracy of new high-relevance details	**0.68 [204] (<.001)**	**0.71 [204] (<.001)**	**0.94 [204] (<.001)**	-	-	-
5. Accuracy of repetitions	**0.85 [205] (<.001)**	**0.81 [205] (<.001)**	**0.41 [203] (<.001)**	**0.37 [203] (<.001)**	-	-
6. Rapport Questionnaire score	0.09 [301] (.103)	0.06 [301] (.287)	0.15 [200] (.031)	0.16 [200] (.023)	0.04 [201] (.528)	-
7. State anxiety Questionnaire score	−0.08 [301] (.167)	−0.06 [301] (.292)	−0.09 [199] (.226)	−0.08 [199] (.288)	−0.12 [200] (.093)	**−0.35 [295] (<.001)**

Note: Correlations significant at a conservative $p < .005$ are highlighted in **bold.** *N* varies per correlation due to full-cases analysis where there are different numbers of applicable interviews (i.e., new details is only relevant for second and third interviews so only approx. 200 cases are relevant as opposed to, that is, rapport questionnaire scores which were measured in all three interviews). Other minor variation is due to cases being dropped in data cleaning.

reported regardless of their relevance, repetition or novelty. Supplemental Bayesian analysis further supports these conclusions, finding notably strong evidence for the null hypothesis for most main effects and interactions (see supplemental tables). There was limited evidence in favour for the null hypothesis for the effect of interview and interview*group on accuracy in repetitions, in line with results reported in Table 4. Rapport group also had no significant effect on measures of well-being (perceptions of rapport or state anxiety).

We found some evidence of the accuracy of repetitions being poorer in the third as opposed to the second interview (see Tables 2

TABLE 4 Tests of the effect of rapport group, interview number and their interaction on the key interview outcomes in this study.

Variable	Effect[a]	ANOVA effect
Accuracy of all details	Group	$F(4, 95) = 1.14$, $p = .343$, $\omega^2 = 0.00$, 95% CI [0.00, 1.00]
	Interview	$F(2, 190) = 3.29$, $p = .039$, $\omega^2 = 0.01$, 95% CI [0.00, 0.06]
	Group*Interview	$F(8, 190) = 2.32$, $p = .021$ $\omega^2 = 0.03$, 95% CI [0.00, 0.05]
Accuracy of high relevance details	Group	$F(4, 95) = 0.74$, $p = .569$, $\omega^2 = 0.00$, 95% CI [0.00, 1.00]
	Interview	$F(2, 190) = 3.26$, $p = .041$, $\omega^2 = 0.01$, 95% CI [0.00, 0.06]
	Group*Interview	$F(8, 190) = 1.75$, $p = .090$, $\omega^2 = 0.02$, 95% CI [0.00, 0.02]
Accuracy of new details	Group	$F(4, 93) = 3.07$, $p = .020$, $\omega^2 = 0.04$, 95% CI [0.00, 0.11]
	Interview	$F(1, 93) = 5.87$, $p = .017$, $\omega^2 = 0.02$ 95% CI [0.00, 0.12]
	Group*Interview	$F(4, 93) = 0.35$, $p = .842$, $\omega^2 = 0.01$, 95% CI [0.00, 1.00]
Accuracy of new high relevance details	Group	$F(4, 93) = 2.50$, $p = .048$, $\omega^2 = 0.03$, 95% CI [0.00, 0.08]
	Interview	$F(1, 93) = 3.42$, $p = .068$, $\omega^2 = 0.01$, 95% CI [0.00, 0.09]
	Group*Interview	$F(4, 93) = 1.02$, $p = .402$, $\omega^2 = 0.00$, 95% CI [0.00, 1.00]
Accuracy of repetitions	Group	$F(4, 94) = 0.68$, $p = .605$, $\omega^2 = 0.01$, 95% CI [0.00, 1.00]
	Interview	**$F(1, 94) = 16.47$, $p < .001$, $\omega^2 = 0.07$, 95% CI [0.00, 0.19]**
	Group*Interview	$F(4, 94) = 1.55$, $p = .193$, $\omega^2 = 0.01$, 95% CI [0.00, 0.03]
Rapport Questionnaire score	Group	$F(4, 86) = 0.65$, $p = .626$, $\omega^2 = 0.00$, 95% CI [0.00, 1.00]
	Interview	$F(2, 172) = 0.95$, $p = .388$, $\omega^2 = 0.00$, 95% CI [0.00, 1.00]
	Group*Interview	$F(8, 172) = 1.24$, $p = .281$, $\omega^2 = 0.01$, 95% CI [0.00, 1.00]
State anxiety Questionnaire score	Group	$F(4, 86) = 0.42$, $p = .797$, $\omega^2 = 0.01$, 95% CI [0.00, 1.00]
	Interview	**$F(2, 172) = 5.95$, $p = .003$, $\omega^2 = 0.03$, 95% CI [0.00, 0.09]**
	Group*Interview	$F(8, 172) = 1.31$, $p = .241$, $\omega^2 = 0.01$, 95% CI [0.00, 1.00]

Note: Correlations significant at a conservative $p < .005$ are highlighted in **bold**. Supplemental Bayesian analysis (see supplemental materials) also supports the results presented here.

[a]The 'Group' rows in the Effect column describe participant condition omnibus effects for the five levels of rapport style. The 'Interview' row describes the first, second or third time participants were interviewed.

and 4) however, in practical terms, the difference between 57.44% accuracy and 52.27% accuracy is minor. Similarly, there was some evidence of participants reporting higher anxiety (see Table 2) in their first interview than the second ($t = 2.99$, Tukey $p = .010$, $d = 0.30$ 95% CI [0.09, 0.50]) and third ($t = 2.49$, Tukey $p = .039$, $d = 0.27$ 95% CI [0.06, 0.48]) interviews. The second and third did not differ from each other ($t = 0.45$, Tukey $p = .900$, $d = -0.04$ 95% CI [−0.24, 0.16]).

4 | DISCUSSION

This study is the first to examine rapport-building across multiple interviews and one of very few to compare the neutral conversation rapport-building procedure with a no rapport *control* group (whose interviews were conducted in a friendly manner). It was found that none of the rapport-building conditions across first, second or third interviews made any significant difference to children's recall, perceived rapport, or their state anxiety in any of these interviews.

Consistent with K. Collins' (2012), our data showed that *combined* rapport-building as described in the ABE guidelines (Ministry of Justice, 2022) had no effect on children's recall in comparison to a *control* group that received no specific rapport-building. Thus, in this particular experimental setting, the results do not support the hypothesis that the inclusion in ABE of the rapport-building phase is based on (i.e., that such rapport-building will improve children's recall). Previous studies have found that well-conducted rapport-building (e.g., involving open-ended questions) can have beneficial effects on children's recall in comparison to poorly-conducted rapport-building (e.g., using closed questions; Brown et al., 2013; Roberts et al., 2004; Sternberg et al., 1997). However, the present study's rapport-building involved recommended practice, including open questions and asking for recall of a neutral event, and found no significant difference in children's recall in comparison to a *control* group in which no rapport-building phase was conducted but the interviewer acted in a friendly manner. Thus, it may be that poor rapport-building actively encourages short answers and less detail and so is detrimental to children's recall, whereas good rapport-building just maintains children's abilities to respond to open questions. Studies that have examined rapport-building in the field have often found it to be conducted poorly (Westcott & Kynan, 2006; Wood et al., 1996; Yi et al., 2015), and so the current study's results are not encouraging in terms of the likely impact this will have on the quality and quantity of the accounts obtained in such interviews.

The present study also found that conducting the rapport-building phase the day before does not affect children's recall differently from conducting it on the same day as the interview (or not at all). The length of the rapport-building in the present study was fairly short, and so we were not expecting to see evidence of children tiring due to the rapport-building phase in the *combined* conditions. However, the lack of differences between rapport built just before the interview and the day before are useful for practitioners who are concerned about the length of rapport-building (Burrows & Powell, 2014), and for supporting the ABE's (Ministry of Justice, 2022) suggestion of separating rapport-building from the interview if lengthy rapport-building is required. Conducting the rapport-building the day before reduces the risk of this phase tiring child interviewees and thus reducing their recall (Roberts et al., 2004). Additionally, it may make it easier for children's interviews to be shown in court without the rapport-building phase, as this phase could be considered irrelevant material (Krähenbühl, 2012; Waterhouse et al., 2020b). Visiting the interviewee the day before may also improve children's interviewing experience by giving them notice of the upcoming interview. Previously, children have described not being given notice and finding this a distressing aspect of their subsequent interviewing experience (Westcott & Davies, 1996).

Another key finding from the present study is that none of the rapport-building conditions resulted in significantly different impacts on children's well-being scores (state anxiety and perceived rapport). This replicates K. Collins' (2012) findings. However, in the present study, there was an issue with floor and ceiling effects: children's state anxiety scores were generally low (indicating low anxiety) and rapport perception scores were generally very high (indicating good rapport with the interviewer). Regardless of whether this is an outcome of social desirability or genuine feeling, this limits our conclusions. It appears from the lack of group differences that conducting a friendly interview results in rapport and low anxiety, regardless of whether the neutral topic discussion occurs at the beginning of the interview or not. This suggests that the current rapport-building technique recommended by the ABE (Ministry of Justice, 2022) may not be a good use of interviewers' time (especially given our argument above regarding the risks of poorly conducted rapport-building phases on recall). However, the novel questionnaire used to measure perceived rapport in the present study may not have been sensitive enough to variations in children's perceived rapport. Furthermore, the filmed event used was unlikely to replicate the levels of anxiety and reluctance most child victims of abuse feel during their interviews. Thus, in a situation where the base level of rapport may be very low and children are feeling reluctant to engage with the interviewer, this kind of rapport-building may be more beneficial and show a significant impact on children's feelings. This is, however, the first attempt to create a self-reported rapport measure for use with children and if a rapport-building process is discovered which is effective with children at low stress/trauma levels, this may be even more effective for child victims experiencing high levels of stress or trauma.

Alternative rapport-building techniques than those in the present study have been found to affect children's recall positively in comparison to no rapport-building, even in experimental conditions with positive or neutral events being recalled and thus low levels of stress/trauma. K. Collins (2012) found a play rapport-building phase improved children's recall in comparison to a control condition but did not strongly affect their state anxiety or any other indicators of rapport, suggesting it may not necessarily increase children's perceived rapport. The Revised NICHD also shows great promise in field studies with child victims (e.g., Blasbalg et al., 2021; Hershkowitz et al., 2021), indicating benefits for children's recall and reluctance (which may reflect reduced anxieties due to increased perceptions of rapport). However, to our knowledge, no experimental comparisons between the ABE guidance and Revised NICHD have yet been conducted. Thus, there may be techniques that improve recall and which are easier to conduct that could be used as a substitute to the neutral discussion recommended by the ABE (Ministry of Justice, 2022).

Rapport-building conditions in the second and third interviews also had negligible impact on children's anxiety or perceptions of rapport with the interviewer. Additionally, there were no significant differences in children's recall based on the second and third interview rapport-building conditions. This included no significant differences in the accuracy of reminiscence (i.e., new details provided in later interviews that were not provided in earlier ones). This again suggests that a specific rapport-building phase (of any length) may not be required in interviews that occur with just a week delay between them. Instead, conducting a friendly interview may be sufficient, particularly in situations where the child is not traumatised by the event and thus rapport is easier to build without a separate phase dedicated to this and possibly less necessary. However, it is important to consider that the shortened rapport-building examined here was only 5 min shorter than the *standard* rapport-building. Although this timing is realistic given the time constraints interviewers are often under, it may not have been sufficient for children to experience a difference.

Although the rapport-building conditions appeared to have no impact on children's well-being, children did appear to be significantly more anxious in their first interviews than their subsequent ones. One concern regarding multiple interviewing is that it may sometimes be traumatic for children (Plotnikoff & Woolfson, 2001). This is the first study to examine children's well-being across multiple interviews and the findings suggest that children may not find second interviews as unpleasant as first interviews. However, the difference in anxiety is very small across the interviews (possibly due to ceiling effects) and as discussed above, the effects of rapport and multiple interviewing on children's anxiety may be different when children experience an event which causes extreme distress, and so these results may be more appropriately generalised to child bystander witnesses than victims.

4.1 | Limitations

Some of the limitations of the present study have already been discussed. However, it is also subject to limitations that are found for many experimental eyewitness studies. In particular, watching a video of a non-traumatic crime as the to-be-remembered event does not reflect everyday witness and victim experience. Although this allowed

for the event to be crime-relevant (ethical constraints may have made a live crime event challenging), it meant that children were not personally involved in the event. The aspects that may affect children's memory and how accurately it will replicate a real crime memory include the lack of participation (Baker-Ward et al., 1990), and children's low levels of stress and/or anxiety about the event and recalling it. Also, the children were 'bystanders' rather than 'victims', the latter of whom may indeed have much greater need for rapport. Children's anxiety around recalling a non-traumatic event they watched is likely to have been negligible in comparison to children who are having to recount their own sexual, psychological or physical victimisation. However, if rapport-building is shown to be effective in less stressful situations, it is possible it would be particularly effective for children experiencing high levels of distress. Further research with to-be-remembered events that more closely align with the stress levels associated with victimisation and forensic interviews in an ethical manner (such as interviewing about emergency medical care) would be very beneficial.

Limitations also relate to the interviewer. As the interviewer knew the content of the video watched, the experimental group that the child was allocated to, and the hypotheses, the interviews may have been conducted in a biased manner. For example, questions may have been chosen or phrased to encourage accurate recall in some groups and not others. Furthermore, the same interviewer conducted all of the interviews over a period of 11 months and so the interviews and expertise of the interviewer may have changed over this time. However, interviews followed a protocol and were standardised in as far as possible, and so this should not have affected the lack of significant differences between conditions found in the present study.

The present study attempted to examine the impact of rapport-building and multiple interviews on investigation-relevant details. This is an important consideration for ensuring interventions do not solely result in the increased provision of peripheral details that would not have an impact on the investigation or investigative decision-making in any way. The method of asking experienced police officers to identify details of the event as of either high or low investigation-relevance is also a valid way of coding such details. However, in future research, it may be more reliable to require a higher level of agreement on whether a detail is of high or low investigation-relevance (i.e., more than three of five police officers agreeing).

Finally, it is possible that there were variations in the effect of rapport-building within the age range studied, but the sample size is not sufficient to allow deeper analysis of this with sufficient power. It is possible that the younger children within the sample benefitted more from the rapport-building than the older children due to younger children's reduced understanding of what they were being asked to do and more limited range of social interactions with adults. In future research a more in depth examination of the effects of rapport at different developmental levels could be considered.

5 | CONCLUSIONS

The present study is the first to have examined how the absence or presence and length of rapport-building affect children's well-being (state anxiety and perceived rapport) and recall across multiple interviews. The findings suggest that alternatives to the current rapport-building described in the ABE (Ministry of Justice, 2022) guidelines should be examined for bystander witnesses to non-abusive events. ABE (Ministry of Justice, 2022) rapport-building was found to have no effects on children's recall in first, second and third interviews in comparison to a no rapport-building *control* group. Additionally, the friendly interviewing approach taken resulted in high levels of perceived rapport and low anxiety, with no additive or detrimental impact of a preceding neutral topic discussion (as encouraged by the ABE, Ministry of Justice, 2022, for building rapport). Recall and well-being was also unaffected by rapport-building conditions in second and third interviews. In conclusion, the present study found that rapport-building as currently recommended in England and Wales does not improve children's recall or well-being as bystander witnesses of low stress crime. Although we are not suggesting that rapport-building is removed from child interviews, we recommend that alternative methodologies are investigated to identify ones that result in notable rapport and improved well-being for children in diverse settings (i.e., with differing levels of anxiety) and are easy to conduct with less risk of detrimentally affecting recall.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflict of interest.

DATA AVAILABILITY STATEMENT

Data, study materials, and analysis code will be made available upon reasonable request to the corresponding author.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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