

Schoolchildren cooperate more successfully with non-kin than with siblings

Gladys Barragan-Jason, Maxime Cauchoux, Anne Regnier, Marie Bourjade, Astrid Hopfensitz and Alexis S. Chaine

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Review timeline

Original submission: 15 September 2020

1st revised submission: 26 November 2020

2nd revised submission: 12 January 2021

Final acceptance: 12 January 2021

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History

RSPB-2020-2286.R0 (Original submission)

Review form: Reviewer 1

Recommendation

Major revision is needed (please make suggestions in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?

Good

General interest: Is the paper of sufficient general interest?

Good

Quality of the paper: Is the overall quality of the paper suitable?

Good

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

Yes

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

Yes

Is it clear?

Yes

Is it adequate?

Yes

Do you have any ethical concerns with this paper?

No

Comments to the Author

In this paper, Barragan-Jason and colleagues describe the results of a coordination game played by children in schools in SW France. The game involves needing to pull on a rope at the same time in order to pull a reward (stickers) toward themselves. The children played the game with siblings, friends, and non-friends. Siblings pairs were less likely to succeed in the task than friends or other non-kin were. The experimental paradigm is neat, and the results are intriguing. My overall verdict is that I found the main result of this paper very interesting and that I am sure many anthropologists and psychologists will feel similarly. That said, there are omissions in statistical reporting that should be amended, I think that the explanations of why we see these differences could be better developed and I would suggest that generalisations be reined in and comparisons with results from adults be better qualified, as elaborated below.

Statistical analyses

The results of the experiment are analysed in five statistical models described in the main text and set out in the SM. For each predictor we are given the relevant test statistic and p value but not the coefficients! The coefficients/odds ratios are the model and without them the reader cannot reconstruct (for example) the probability of a pair being successful as a function of age, sex, number of siblings etc. This information (plus estimates of error around each parameter) really should be given.

Definitions

A source of confusion to me was what constitutes 'success' in the game. It is suggested that children can succeed on their own (L111+) as well as together. Models then predict what is described variously as "success", "overall success", "cooperation", "performance of a cooperative task" - exactly what each of these mean ought to be clarified and, if possible, standardized to "joint success" or similar (if it is joint success being predicted).

I also think that 'success' and 'cooperation' in this game should not be equated. Two children trying to work together but failing because of the physical difficulty of the task is not evidence of uncooperativeness. I appreciated that coordination/cooperativeness can aid success (as indicated by the eye tracking) but that does not mean that success is a measure of cooperativeness. It is also not a measure of cooperation in the sense of generosity, as is usually implied in discussions of cooperation with kin vs non-kin.

What is driving the effect?

What is going on with these unsuccessful trials in siblings? As an evolutionary

anthropologist/biologist, the explanations offered from L262+ about kin competition and the importance of building up a network of friendship seem reasonable a priori. However, kin competition doesn't tally with the fact the children report being (roughly) as happy to play with kin as non kin in Fig 3 and the value of building friendships is undermined slightly by the fact that children were just as successful working with non-friends than friends. I am also reluctant to speculate about these adaptive hypotheses without a better grasp of why trials failed. What were the reasons that trials failed? How much can failure be attributed to a lack of coordination/cooperation? The eye tracking results suggests it can be somewhat down to a lack of cooperation but more qualitative comment on why trials failed might provide a firmer ground for speculation about the kin/non-kin differences. Can the authors provide any comment on this, quantitative or otherwise?

Generalising from one population

In my own work I have written papers with titles that generalise well beyond my study population – so I feel that I am in a glass house throwing stones with this comment but I feel like the title and abstract (and paper in general) make a lot of generalisations: from a sample of schools in one region of France to all western populations, from this experimental paradigm to cooperation in general, and from studies of adults to adult cooperation in general for comparison with children. Perhaps the authors could at least include some mention of the context (schoolchildren between the ages of 3 and 10 in SW France) in the abstract and entertain adding more context to the title (by including some mention of 'French schoolchildren' and/or 'rope pull game' or similar)?

Comparing adults and children

Much of the interpretation of these results relies on comparisons of the results with evidence for bias toward cooperating with kin among adults. But these comparisons are not with results from the same experimental paradigm, and this should be made explicit. In fact, it would be good to have more information about the specifics of these studies of adults (e.g. what was the context, what was the magnitude of kin bias?).

Review form: Reviewer 2

Recommendation

Major revision is needed (please make suggestions in comments)

Scientific importance: Is the manuscript an original and important contribution to its field?

Good

General interest: Is the paper of sufficient general interest?

Good

Quality of the paper: Is the overall quality of the paper suitable?

Good

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

Yes

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

Yes

Is it clear?

No

Is it adequate?

No

Do you have any ethical concerns with this paper?

No

Comments to the Author

Review of Barragan-Jason et al., "Children cooperate more successfully with non-kin than with siblings."

The manuscript reports a study of cooperation in children using a dyadic "rope pulling" task. The authors find that dyads complete the task in fewer trials if they are non-relatives (whether friends or simply acquaintances) than if they are siblings. They also find that children with larger social networks are more likely than children with smaller social networks to succeed on the first trial, and that children in the non-friends condition increased their liking of one another after performing the task.

There is much to like about this study, but three aspects stand out to me: (1) it asks an interesting and relevant question regarding the development of mechanisms fostering nepotistic and mutually beneficial outcomes; (2) it has an impressive sample size for a study of children; and the manuscript is well written. The authors should be commended for all of this.

I do, however, have a few concerns regarding the methodological design and analysis. First, although the authors refer to their study as an experiment, it is clear from the methods that dyads were not assigned randomly to condition. Rather, the design prioritized assignment to the siblings condition, because within-school sibling relationships were relatively rare. This is a perfectly understandable approach, but calling the design "an experiment" overstates the ability to make causal inferences. There is nothing wrong with calling it a quasi-experiment.

Second, as the authors also point out, the siblings condition confounds kinship with age differences and the friends condition confounds friendship with shared sex/gender. The authors attempt to address this by creating age matched and sex matched dyads among the children assigned to the non-friends condition. However, it appears from the description on lines 142-146 and Figure S2 that these two different kinds of matched pairs were grouped together in the non-friends condition, rather than being treated as separate controls for the pairs assigned to the siblings and friends conditions. This makes it difficult to detect whether the effects attributed to kinship are not actually about age differences and whether the effects of friendship are not actually about shared sex/gender. Ideally, the analyses would compare siblings to sex- and age-matched non-friends and, separately, compare friends to sex- and age-matched non-friends rather than attempt to control for these effects statistically.

Third, in a few places, the analysis switches between tests of task success across all three trials and tests of the first trial alone. This may be for perfectly logical reasons, but it equally suggests that "researcher degrees of freedom" may have been at play. Unfortunately, the analysis plans were not preregistered. However, a simple solution is to increase transparency: whenever an

analysis is performed on the first trial alone, it should be followed by an analysis of all trials.

Fourth, the unique effect of being paired in the task on relationship ratings of non-friends (relative to siblings and friends) may simply be the result of a ceiling effect. Figure 3A shows nearly perfect liking of siblings and friends in the “before” measurement period, and Figure 3B shows a parallel result with respect to happy and neutral emoticons. It is also unclear why these analyses included all pairs rather than just those who managed to successfully complete the task. I would imagine that successful cooperation would lead to greater liking whereas failure would lead to greater disliking, but I did not see any discussion of this.

Finally, I also have a few smaller comments on the manuscript:

- The statement on lines 20-21 that the study’s results “indicate that non-kin cooperation in humans has deep developmental foundations” is a bit of an overstatement. On the one hand, if the results had been reversed, as they are in adults, they would not indicate that non-kin cooperation in humans does *not* have deep developmental foundations. This is because the study is comparing different relationship types, not showing that children *always* or *never* cooperate with non-kin. On the other hand, no study needs to be done to show that children cooperate with non-kin, as the mere existence of friendships in childhood makes this patently obvious.
- The manuscript appears to imply at times that, in mechanistic terms, nepotism should depend on age *per se*. That is, children everywhere should be biased towards mutually beneficial relationships with non-kin whereas adults everywhere should be biased towards cooperative relationships (whether mutually beneficial or altruistic) with kin. However, as the authors rightly point out on lines 269-271, one possible explanation for age-related variation in non-kin cooperation is age-related variation in kin competition. If so, it is possible that any putative effects of age on sibling rivalry are due to contextual effects of kin competition rather than age itself. For example, conflict among adult siblings can arise suddenly when a large inheritance is on the horizon (e.g. Daly & Wilson, 1988). Thus, we might expect considerable cross-cultural variation in the extent to which cooperation and conflict among siblings are expressed over the lifespan.
- It is unclear whether the rope pulling task is a measure of reciprocity, exactly. The behaviour is certainly cooperative, as the dyad must work together, but there is neither dilemma nor risk at work here: individuals simply cannot acquire the reward (their sticker) without the help of their partner. Indeed, the rope pulling apparatus limits the opportunities for both competition and cheating, making it more of a measure of a “fixed enforcement” kind of policing than reciprocity (West et al. 2007).
- The analysis of eye gaze is interesting, but it was not motivated in the Introduction. I did not understand why it was introduced until I got to the Results and Discussion.
- Lines 209-210 are a bit confusing, as the sentence starts with “Overall, 61% of pairs succeeded in the rope pulling task” when 63% succeeded in the first trial alone. In fact, overall, 90% of the pairs succeeded in the rope pulling task, and the 61% figure is calculated only over the total number of trials. This is fine, but the opening of the sentence should be rephrased to refer to trial success rather than pair success.

To sum up, I think this manuscript describes an interesting and worthwhile study, but I would like a bit more clarity on some details.

References

Daly, M., & Wilson, M. (1988). *Homicide*. Aldine de Gruyter.

West, S. A., Griffin, A. S., & Gardner, A. (2007). Evolutionary explanations for cooperation. *Current Biology*, 17, R661-R672. <https://doi.org/10.1016/j.cub.2007.06.004>

Decision letter (RSPB-2020-2286.R0)

21-Oct-2020

Dear Dr Barragan-Jason:

I am writing to inform you that your manuscript RSPB-2020-2286 entitled "Children cooperate more successfully with non-kin than with siblings" has, in its current form, been rejected for publication in Proceedings B.

This action has been taken on the advice of referees, who have recommended that substantial revisions are necessary. With this in mind we would be happy to consider a resubmission, provided the comments of the referees are fully addressed. However please note that this is not a provisional acceptance.

The resubmission will be treated as a new manuscript. However, we will approach the same reviewers if they are available and it is deemed appropriate to do so by the Editor. Please note that resubmissions must be submitted within six months of the date of this email. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office. Manuscripts submitted after this date will be automatically rejected.

Please find below the comments made by the referees, not including confidential reports to the Editor, which I hope you will find useful. If you do choose to resubmit your manuscript, please upload the following:

- 1) A 'response to referees' document including details of how you have responded to the comments, and the adjustments you have made.
- 2) A clean copy of the manuscript and one with 'tracked changes' indicating your 'response to referees' comments document.
- 3) Line numbers in your main document.
- 4) Data - please see our policies on data sharing to ensure that you are complying (<https://royalsociety.org/journals/authors/author-guidelines/#data>).

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Sincerely,
Dr Robert Barton
<mailto:proceedingsb@royalsociety.org>

Associate Editor
Board Member: 1
Comments to Author:

Many thanks for submitting your manuscript on cooperation among kin-versus non-kin, as well as the role of social networks in mediating these effects, to Proceedings B.

As you will see below, your manuscript has now been read in detail by two expert reviewers, who both agreed that you address an important topic of general interest. I agree with the reviewers, and I very much enjoyed reading this well-written manuscript. Nevertheless, the reviewers have provided a set of detailed and constructive comments and suggestions.

Reviewer 1 would like to see more discussion of what constitutes 'success', what is causing some trials to be unsuccessful, and the degree to which the results from this study can be generalised.

Finally, I agree with their comment that strictly speaking this study cannot show that the effect of kinship is different between adults and children, which would require repeating the experiment in adults and testing for an age x kinship interaction. Although clearly beyond the scope of this study, this does warrant some discussion. Finally, I fully agree with the reviewer that the reporting of the statistical results needs improvement.

Reviewer 2 raises several issues also raised by reviewer 1. In addition to a series of excellent smaller comments, they highlight the need for discussion/clarification of the study design and its limitations, and what this study can and cannot show.

All things considered, I am afraid that in its current form this manuscript is not acceptable for publication in Proceedings B. Having said this, I think most of the issues raised can be addressed, and I would therefore welcome a significantly revised resubmission.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)

In this paper, Barragan-Jason and colleagues describe the results of a coordination game played by children in schools in SW France. The game involves needing to pull on a rope at the same time in order to pull a reward (stickers) toward themselves. The children played the game with siblings, friends, and non-friends. Siblings pairs were less likely to succeed in the task than friends or other non-kin were. The experimental paradigm is neat, and the results are intriguing. My overall verdict is that I found the main result of this paper very interesting and that I am sure many anthropologists and psychologists will feel similarly. That said, there are omissions in statistical reporting that should be amended, I think that the explanations of why we see these differences could be better developed and I would suggest that generalisations be reined in and comparisons with results from adults be better qualified, as elaborated below.

Statistical analyses

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Definitions

A source of confusion to me was what constitutes 'success' in the game. It is suggested that children can succeed on their own (L111+) as well as together. Models then predict what is described variously as "success", "overall success", "cooperation", "performance of a cooperative task" - exactly what each of these mean ought to be clarified and, if possible, standardized to "joint success" or similar (if it is joint success being predicted).

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What is driving the effect?

What is going on with these unsuccessful trials in siblings? As an evolutionary anthropologist/biologist, the explanations offered from L262+ about kin competition and the importance of building up a network of friendship seem reasonable a priori. However, kin competition doesn't tally with the fact the children report being (roughly) as happy to play with kin as non kin in Fig 3 and the value of building friendships is undermined slightly by the fact that children were just as successful working with non-friends than friends. I am also reluctant to speculate about these adaptive hypotheses without a better grasp of why trials failed. What were the reasons that trials failed? How much can failure be attributed to a lack of

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Comparing adults and children

Much of the interpretation of these results relies on comparisons of the results with evidence for bias toward cooperating with kin among adults. But these comparisons are not with results from the same experimental paradigm, and this should be made explicit. In fact, it would be good to have more information about the specifics of these studies of adults (e.g. what was the context, what was the magnitude of kin bias?).

Referee: 2

Comments to the Author(s)

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There is much to like about this study, but three aspects stand out to me: (1) it asks an interesting and relevant question regarding the development of mechanisms fostering nepotistic and mutually beneficial outcomes; (2) it has an impressive sample size for a study of children; and the manuscript is well written. The authors should be commended for all of this.

I do, however, have a few concerns regarding the methodological design and analysis. First, although the authors refer to their study as an experiment, it is clear from the methods that dyads were not assigned randomly to condition. Rather, the design prioritized assignment to the siblings condition, because within-school sibling relationships were relatively rare. This is a perfectly understandable approach, but calling the design “an experiment” overstates the ability to make causal inferences. There is nothing wrong with calling it a quasi-experiment.

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actually about shared sex/gender. Ideally, the analyses would compare siblings to sex- and age-matched non-friends and, separately, compare friends to sex- and age-matched non-friends rather than attempt to control for these effects statistically.

Third, in a few places, the analysis switches between tests of task success across all three trials and tests of the first trial alone. This may be for perfectly logical reasons, but it equally suggests that “researcher degrees of freedom” may have been at play. Unfortunately, the analysis plans were not preregistered. However, a simple solution is to increase transparency: whenever an analysis is performed on the first trial alone, it should be followed by an analysis of all trials.

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Finally, I also have a few smaller comments on the manuscript:

- The statement on lines 20-21 that the study’s results “indicate that non-kin cooperation in humans has deep developmental foundations” is a bit of an overstatement. On the one hand, if the results had been reversed, as they are in adults, they would not indicate that non-kin cooperation in humans does *not* have deep developmental foundations. This is because the study is comparing different relationship types, not showing that children *always* or *never* cooperate with non-kin. On the other hand, no study needs to be done to show that children cooperate with non-kin, as the mere existence of friendships in childhood makes this patently obvious.
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- It is unclear whether the rope pulling task is a measure of reciprocity, exactly. The behaviour is certainly cooperative, as the dyad must work together, but there is neither dilemma nor risk at work here: individuals simply cannot acquire the reward (their sticker) without the help of their partner. Indeed, the rope pulling apparatus limits the opportunities for both competition and cheating, making it more of a measure of a “fixed enforcement” kind of policing than reciprocity (West et al. 2007).
- The analysis of eye gaze is interesting, but it was not motivated in the Introduction. I did not understand why it was introduced until I got to the Results and Discussion.
- Lines 209-210 are a bit confusing, as the sentence starts with “Overall, 61% of pairs succeeded in the rope pulling task” when 63% succeeded in the first trial alone. In fact, overall, 90% of the pairs succeeded in the rope pulling task, and the 61% figure is calculated only over the total number of trials. This is fine, but the opening of the sentence should be rephrased to refer to trial success rather than pair success.

To sum up, I think this manuscript describes an interesting and worthwhile study, but I would like a bit more clarity on some details.

References

Daly, M., & Wilson, M. (1988). Homicide. Aldine de Gruyter.

West, S. A., Griffin, A. S., & Gardner, A. (2007). Evolutionary explanations for cooperation. *Current Biology*, 17, R661–R672. <https://doi.org/10.1016/j.cub.2007.06.004>

Author's Response to Decision Letter for (RSPB-2020-2286.R0)

See Appendix A.

RSPB-2020-2951.R0

Review form: Reviewer 1

Recommendation

Accept as is

Scientific importance: Is the manuscript an original and important contribution to its field?

Good

General interest: Is the paper of sufficient general interest?

Good

Quality of the paper: Is the overall quality of the paper suitable?

Good

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

Yes

Is it clear?

Yes

Is it adequate?

Yes

Do you have any ethical concerns with this paper?

No

Comments to the Author

The authors have satisfactorily addressed my concerns and the revised paper looks good.

My only minor caveat is that while the addition about the lower degree of attentiveness between kin is welcome, I still feel that I have only a vague sense of why kin dyads were more likely to fail. But I acknowledge that one study cannot do it all and that quantifying and unpicking proximate explanations is difficult. This will be a question for future work, I suppose!

My initial opinion that this is an interesting and intriguing finding that will be of interest to a wide audience still stands; I hope that this paper will be published in PRSB.

Review form: Reviewer 2

Recommendation

Accept as is

Scientific importance: Is the manuscript an original and important contribution to its field?

Good

General interest: Is the paper of sufficient general interest?

Excellent

Quality of the paper: Is the overall quality of the paper suitable?

Excellent

Is the length of the paper justified?

Yes

Should the paper be seen by a specialist statistical reviewer?

No

Do you have any concerns about statistical analyses in this paper? If so, please specify them explicitly in your report.

No

It is a condition of publication that authors make their supporting data, code and materials available - either as supplementary material or hosted in an external repository. Please rate, if applicable, the supporting data on the following criteria.

Is it accessible?

Yes

Is it clear?

Yes

Is it adequate?

Yes

Do you have any ethical concerns with this paper?

No

Comments to the Author

I apologize for the delayed review, and thank the editor and authors (especially) for their patience. I think the authors have done a great job with the revised manuscript of addressing my previous concerns, and I have no significant issues remaining. Consequently, I recommend the manuscript for publication in Proceedings B.

Decision letter (RSPB-2020-2951.R0)

08-Jan-2021

Dear Dr Barragan-Jason

I am pleased to inform you that your Review manuscript RSPB-2020-2951 entitled "Schoolchildren cooperate more successfully with non-kin than with siblings" has been accepted for publication in Proceedings B.

The referee(s) do not recommend any further changes. Therefore, please proof-read your manuscript carefully and upload your final files for publication. Because the schedule for publication is very tight, it is a condition of publication that you submit the revised version of your manuscript within 7 days. If you do not think you will be able to meet this date please let me know immediately.

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- 3) Electronic supplementary material: this should be contained in a separate file from the main text and the file name should contain the author's name and journal name, e.g `authorname_procb_ESM_figures.pdf`

All supplementary materials accompanying an accepted article will be treated as in their final form. They will be published alongside the paper on the journal website and posted on the online figshare repository. Files on figshare will be made available approximately one week before the accompanying article so that the supplementary material can be attributed a unique DOI. Please see: <https://royalsociety.org/journals/authors/author-guidelines/>

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<http://datadryad.org/submit?journalID=RSPB&manu=RSPB-2020-2951> which will take you to your unique entry in the Dryad repository.

If you have already submitted your data to dryad you can make any necessary revisions to your dataset by following the above link.

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Once again, thank you for submitting your manuscript to Proceedings B and I look forward to receiving your final version. If you have any questions at all, please do not hesitate to get in touch.

Sincerely,

Dr Robert Barton

<mailto:proceedingsb@royalsociety.org>

Associate Editor

Comments to Author:

Many thanks for submitting your revised manuscript, as well as your patience: I realise it has been a long time since your resubmission, but I hope you feel it has been worth the wait. Your manuscript has been reviewed by both of the original reviewers, and as you will see below, both of them appreciate your efforts to address all their comments. Reviewer 2 has no further comments, and although Reviewer 1 remains somewhat puzzled by the lower degree of cooperation among kin, they also acknowledge that there are limits to what one can do in a single paper. I have wholeheartedly agree with these assessments and have nothing to add to this.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s).

The authors have satisfactorily addressed my concerns and the revised paper looks good.

My only minor caveat is that while the addition about the lower degree of attentiveness between kin is welcome, I still feel that I have only a vague sense of why kin dyads were more likely to fail. But I acknowledge that one study cannot do it all and that quantifying and unpicking proximate explanations is difficult. This will be a question for future work, I suppose!

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Comments to the Author(s).

I apologize for the delayed review, and thank the editor and authors (especially) for their patience. I think the authors have done a great job with the revised manuscript of addressing my

previous concerns, and I have no significant issues remaining. Consequently, I recommend the manuscript for publication in Proceedings B.

Sincerely,
 Proceedings B
 mailto: proceedingsb@royalsociety.org

Associate Editor,
 Board Member
 Comments to Author:

Many thanks for submitting your revised manuscript, as well as your patience: I realise it has been a long time since your resubmission, but I hope you feel it has been worth the wait. Your manuscript has been reviewed by both of the original reviewers, and as you will see below, both of them appreciate your efforts to address all their comments. Reviewer 2 has no further comments, and although Reviewer 1 remains somewhat puzzled by the lower degree of cooperation among kin, they also acknowledge that there are limits to what one can do in a single paper. I have wholeheartedly agree with these assessments and have nothing to add to this.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s).

The authors have satisfactorily addressed my concerns and the revised paper looks good.

My only minor caveat is that while the addition about the lower degree of attentiveness between kin is welcome, I still feel that I have only a vague sense of why kin dyads were more likely to fail. But I acknowledge that one study cannot do it all and that quantifying and unpicking proximate explanations is difficult. This will be a question for future work, I suppose!

My initial opinion that this is an interesting and intriguing finding that will be of interest to a wide audience still stands; I hope that this paper will be published in PRSB.

Referee: 2

Comments to the Author(s).

I apologize for the delayed review, and thank the editor and authors (especially) for their patience. I think the authors have done a great job with the revised manuscript of addressing my previous concerns, and I have no significant issues remaining. Consequently, I recommend the manuscript for publication in Proceedings B.

Decision letter (RSPB-2020-2951.R1)

12-Jan-2021

Dear Dr Barragan-Jason

I am pleased to inform you that your manuscript entitled "Schoolchildren cooperate more successfully with non-kin than with siblings" has been accepted for publication in Proceedings B.

You can expect to receive a proof of your article from our Production office in due course, please check your spam filter if you do not receive it. PLEASE NOTE: you will be given the exact page length of your paper which may be different from the estimation from Editorial and you may be asked to reduce your paper if it goes over the 10 page limit.

If you are likely to be away from e-mail contact please let us know. Due to rapid publication and an extremely tight schedule, if comments are not received, we may publish the paper as it stands.

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Thank you for your fine contribution. On behalf of the Editors of the Proceedings B, we look forward to your continued contributions to the Journal.

Sincerely,

Proceedings B

<mailto:proceedingsb@royalsociety.org>

Appendix A

Responses to reviewers

Associate Editor

Board Member: 1

Comments to Author:

Many thanks for submitting your manuscript on cooperation among kin-versus non-kin, as well as the role of social networks in mediating these effects, to Proceedings B.

As you will see below, your manuscript has now been read in detail by two expert reviewers, who both agreed that you address an important topic of general interest. I agree with the reviewers, and I very much enjoyed reading this well-written manuscript. Nevertheless, the reviewers have provided a set of detailed and constructive comments and suggestions.

Reviewer 1 would like to see more discussion of what constitutes 'success', what is causing some trials to be unsuccessful, and the degree to which the results from this study can be generalised. Finally, I agree with their comment that strictly speaking this study cannot show that the effect of kinship is different between adults and children, which would require repeating the experiment in adults and testing for an age x kinship interaction. Although clearly beyond the scope of this study, this does warrant some discussion. Finally, I fully agree with the reviewer that the reporting of the statistical results needs improvement.

Reviewer 2 raises several issues also raised by reviewer 1. In addition to a series of excellent smaller comments, they highlight the need for discussion/clarification of the study design and its limitations, and what this study can and cannot show.

All things considered, I am afraid that in its current form this manuscript is not acceptable for publication in Proceedings B. Having said this, I think most of the issues raised can be addressed, and I would therefore welcome a significantly revised resubmission.

[Thank you very much for the positive feedback and we are glad it was easy to read! We've addressed all the issues raised by Reviewer 1 and 2 and appreciate the very constructive feedback. Please see our detailed responses below.](#)

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)

In this paper, Barragan-Jason and colleagues describe the results of a coordination game played by children in schools in SW France. The game involves needing to pull on a rope at the same time in order to pull a reward (stickers) toward themselves. The children played the game with siblings, friends, and non-friends. Siblings pairs were less likely to succeed in the task than friends or other non-kin were. The experimental paradigm is neat, and the results are intriguing. My overall verdict is that I found the main result of this paper very interesting and that I am sure many anthropologists and psychologists will feel similarly. That said, there are omissions in statistical reporting that should be amended, I think that the explanations of why we see these differences could be better developed and I would suggest that generalisations be reined in and comparisons with results from adults be better qualified, as elaborated below.

1- Statistical analyses

The results of the experiment are analysed in five statistical models described in the main text and set out in the SM. For each predictor we are given the relevant test statistic and p value but not the coefficients! The coefficients/odds ratios are the model and without them the reader cannot reconstruct (for example) the probability of a pair being successful as a function of age, sex, number of siblings etc. This information (plus estimates of error around each parameter) really should be given.

We now provide the requested information. We only reported the main effects in the text for brevity, but provide all the information for comparison in supplemental tables (odds ratios are particularly useful to compare all the different factors in the model). Please see modified Tables S1 to S5 in the supplemental information.

2- Definitions

A source of confusion to me was what constitutes 'success' in the game. It is suggested that children can succeed on their own (L111+) as well as together. Models then predict what is described variously as "success", "overall success", "cooperation", "performance of a cooperative task" – exactly what each of these mean ought to be clarified and, if possible, standardized to "joint success" or similar (if it is joint success being predicted). I also think that 'success' and 'cooperation' in this game should not be equated. Two children trying to work together but failing because of the physical difficulty of the task is not evidence of uncooperativeness. I appreciated that coordination/cooperativeness can aid success (as indicated by the eye tracking) but that does not mean that success is a measure of cooperativeness. It is also not a measure of cooperation in the sense of generosity, as is usually implied in discussions of cooperation with kin vs non-kin.

We have now clarified what is considered success in the game: when both children receive a reward. We refer to a positive outcome of our tests as "success" or "cooperation" since children had to cooperate and coordinate to succeed in the task as there was no way to succeed without cooperation. If a child pulled at the same speed to get its own reward but let go quickly so that their partner would not get a reward, the trial was considered as a failure in our task and the child did not keep their reward.

Together, this means that success requires cooperation. The rope-pulling task has been usually used to measure cooperation, coordination and collaborative effort in both humans and non-humans (Plotnik et al., 2011; Hamman et al., 2011) but we agree with R1 that we're not measuring cooperation in the sense of altruistic sharing or reciprocal donation. Likewise, it was possible for two children to want to cooperate and yet not succeed because the task was too difficult, although we suspect such cases are unlikely as difficulty did not prevent them from accomplishing the task as most dyads did succeed by the third attempt (90%). However, given the reviewer's concern, we shift our language to refer to "joint success" in a cooperative game in the manuscript and provide more details in the methods. Please see P5 to 10 and P12 to 17.

Plotnik, J. M., Lair, R., Suphachoksakun, W., & De Waal, F. B. (2011). Elephants know when they need a helping trunk in a cooperative task. Proceedings of the National Academy of Sciences, 108(12), 5116-5121.

Hamann, K., Warneken, F., Greenberg, J. R., & Tomasello, M. (2011). Collaboration encourages equal sharing in children but not in chimpanzees. Nature, 476(7360), 328-331.

3- What is driving the effect?

What is going on with these unsuccessful trials in siblings? As an evolutionary anthropologist/biologist, the explanations offered from L262+ about kin competition and the importance of building up a network of friendship seem reasonable a priori. However, kin competition doesn't tally with the fact the children report being (roughly) as happy to play with kin as non kin in Fig 3 and the value of building friendships is undermined slightly by the fact that children were just as successful working with non-friends than friends. I am also reluctant to speculate about these adaptive hypotheses without a better grasp of why trials failed. What were the reasons that trials failed? How much can failure be attributed to a lack of coordination/cooperation? The eye tracking results suggests it can be somewhat down to a lack of cooperation but more qualitative comment on why trials failed might provide a firmer ground for speculation about the kin/non-kin differences. Can the authors provide any comment on this, quantitative or otherwise?

The hypothesis we proposed to explain our results certainly requires more testing as the reviewer points out and as we suggested in the manuscript, but it will require other tests to do so. We tried to lay out the logic of our new hypothesis in general and show how our data are largely consistent with the broad basics of this notion, but we are under no illusion that we provide definitive support for this new hypothesis nor can we dig into the subtle and tangled effects that shifts in life history pay-offs with age or with context might entail. Indeed, why exactly friends and non-friends perform similarly can not be answered by the data we have – but the pattern is consistent with maintaining and creating a larger network to cooperate with. Confirmation would require experiments designed precisely for that question. Furthermore, as the reviewer points out, effects such as kin-competition are complicated and no doubt context (i.e. is the reward sharable or not) and age dependent. Exploring such effects would be very interesting, but likely would be the object of a full theoretical and/or empirical effort in its own right. As such, we have sought to emphasize that we are proposing a new hypothesis and framework that will require more exploration both theoretically and empirically.

While we do have some information on why dyads failed, we did not include more details initially as that information does not fully resolve the hypothesis we put forward. Most failures occurred when the two partners were not able to coordinate: either one child pulled the rope before the other could grab it, or one child let go of the rope and the reward went away before his/her partner could grab it. In other words, failure largely occurred when one partner was not paying attention to the other (as suggest by the low numbers of gazes) or because they were not interested in cooperating to get the reward. Of course, these were more common among kin than non-kin (expected from our main result), but does not really help understand why children might want to cooperate with non-kin more than kin nor why cooperating with friends or non-friends might be of value. Nonetheless, we now clarify descriptions of successes and failures as well as information on gazes in the results and discussion section, please see P17 L356-362.

4- Generalising from one population

In my own work I have written papers with titles than generalise well beyond my study population – so I feel that I am in a glass house throwing stones with this comment but I feel like the title and abstract (and paper in general) make a lot of generalisations: from a sample of schools in one region of France to all western populations, from this experimental paradigm to cooperation in general, and from studies of adults to adult cooperation in general for comparison with children. Perhaps the authors could at least include some mention of the context (schoolchildren between the ages of 3 and 10 in SW France) in the abstract and entertain adding more context to the title (by including some mention of ‘French schoolchildren’ and/or ‘rope pull game’ or similar)?

Fair enough! We’ve now added schoolchildren in the title and in the abstract as well as mentioning in the discussion that further studies across cultures would be of value. As the reviewer says, all findings apply to the population studied and would need more research to determine how widely applicable they are to other populations. Please see P1, P2 and P17 L364-369.

5- Comparing adults and children

Much of the interpretation of these results relies on comparisons of the results with evidence for bias toward cooperating with kin among adults. But these comparisons are not with results from the same experimental paradigm, and this should be made explicit. In fact, it would be good to have more information about the specifics of these studies of adults (e.g. what was the context, what was the magnitude of kin bias?).

The reviewer brings up a valid point – indeed our contention that children and adults differ in their responses are based on comparisons of our results with previous studies in adults. In general, studies of adults show a very consistent pattern across a broad variety of experimental paradigms and populations which is why we felt on reasonable ground suggesting that our results in children contrast those other studies. However, we could not run the same exact study in adults because the task would be far too easy for adults. Creating a more complicated game for adults with a similar conceptual basis or a single game that could be applicable to both children and adults would be very interesting, although we have not been able to come up with such a design to date. Studies in adults usually involved prisoner dilemma, dictator games or ultimatum games which involved donations, cheating and punishment rather than active

collaboration and coordination like our study. In such studies adults favor kin rather than non-kin. Specifically, while empirical studies show that reciprocal acts in adults improve reputation (Fehr et al., 2002 ; Milinski et al., 2002 ; Forsythe et al., 1994, Wedekind & Milinski, 2000, Reis and Gruzen, 1976 ; Haley and Fessler, 2005), adults nonetheless tend to favor cooperation with kin (Vollan, 2011 ; Krupp et al., 2008) and friends (Majolo et al., 2006) rather than strangers when given a choice. For instance, adults tend to prefer to trust kin (Vollan, 2011), are more likely to cooperate with individuals who have similar facial features to their own (Krupp et al., 2008), tend to minimize punishment after rule transgression for kin compared to strangers (Lieberman and Linke, 2007 ; O’Gorman et al., 2005), and tend to « consistently cooperate with their children and partners, and forgive their children’s betrayals » while they tend to compete with non-kin children in a prisoners’ dilemma (Ramenzoni and Gonzalez-Gadea, 2020). Similarly, anthropologists report that adults from traditional non-Western societies (e.g Yanomamö village (Chagnon and Bugos’s (1979)) tend to support relatives during fights and organize farm labor (K’ekchi’ Maya BertJ (1988)) and hunting sessions (Inuit Smith (1991) according their degree of genetic relatedness. We now discuss more explicitly this point and suggest possible follow-ups in future studies. Please see P11-12; L241-249.

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Wedekind C, Milinski M (2000) Cooperation through image scoring in humans. Science 288(5467):850–852.

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Haley KJ, Fessler DMT (2005) Nobody’s watching? Evolution and Human Behavior 26(3):245–256.

Lieberman D, Linke L (2007) The effect of social category on third party punishment. Evol Psychol 5(2):147470490700500.

O’Gorman R, Wilson DS, Miller RR (2005) Altruistic punishing and helping differ in sensitivity to relatedness, friendship, and future interactions. Evolution and Human Behavior 26(5):375–387.

Ramenzoni, V. C., & Gonzalez-Gadea, M. L. (2020). Parental Behavior Models Prosocial Behavior in Face-to-Face Interactions with their School-age Children.

Chagnon, N. A. (1979). Evolutionary biology and human social behavior: An anthropological perspective. Duxbury Press.

Referee: 2

Comments to the Author(s)

Review of Barragan-Jason et al., “Children cooperate more successfully with non-kin than with siblings.”

The manuscript reports a study of cooperation in children using a dyadic “rope pulling” task. The authors find that dyads complete the task in fewer trials if they are non-relatives (whether friends or simply acquaintances) than if they are siblings. They also find that children with larger social networks are more likely than children with smaller social networks to succeed on the first trial, and that children in the non-friends condition increased their liking of one another after performing the task.

There is much to like about this study, but three aspects stand out to me: (1) it asks an interesting and relevant question regarding the development of mechanisms fostering nepotistic and mutually beneficial outcomes; (2) it has an impressive sample size for a study of children; and the manuscript is well written. The authors should be commended for all of this.

Thank you!

1- I do, however, have a few concerns regarding the methodological design and analysis. First, although the authors refer to their study as an experiment, it is clear from the methods that dyads were not assigned randomly to condition. Rather, the design prioritized assignment to the siblings condition, because within-school sibling relationships were relatively rare. This is a perfectly understandable approach, but calling the design “an experiment” overstates the ability to make causal inferences. There is nothing wrong with calling it a quasi-experiment.

As evolutionary biologists, we actually did not know the term “quasi-experimental” which is not used in our literature, although it is the right term in social sciences. We have now included the term, please see L80.

2- Second, as the authors also point out, the siblings condition confounds kinship with age differences and the friends condition confounds friendship with shared sex/gender. The authors attempt to address this by creating age matched and sex matched dyads among the children assigned to the non-friends condition. However, it appears from the description on lines 142-146 and Figure S2 that these two different kinds of matched pairs were grouped together in the non-friends condition, rather than being treated as separate controls for the pairs assigned to the siblings and friends conditions. This makes it difficult to detect whether the effects attributed to kinship are not actually about age differences and whether the effects of friendship are not actually about shared sex/gender. Ideally, the analyses would compare siblings to sex- and age-matched non-friends and, separately, compare friends to sex- and age-matched non-friends rather than attempt to control for these effects statistically.

Controlling for effects of each factor (age, gender, etc.) statistically in a multiple regression is a very common and robust approach to evaluate and control for the effects of potentially confounding factors on estimates of the main effects of interest. Indeed,

this approach is the standard in many domains and the statistical approaches have been created specifically for this purpose to maximize the use of data, identify potentially confounding effects, and generate much more robust findings. As such, this is the approach we took to examine the influence of dyad category while controlling for effects of age and gender which could not be perfectly balanced in our experiment (sibs are not the same age nor necessarily the same gender whereas friends often are). In our analyses, we found no effect of the gender of dyads and the effect of age (estimated either by mean age of the dyad or the difference in age between members of the dyad) had a very straight forward effect where older children had a higher probability of joint success in the task. Importantly, though, the interaction effect between age and dyad status (kin, friend, non-friend) was not significant which means that age did not influence the differences in success rates between the dyad status categories.

However, we do understand the reviewers concern as the common practice in some fields is to contrast only the most comparable categories (assuming all else is equal). Given the constraints inherent in our experiment, comparing only samples with the same age difference or gender difference leads to fairly small samples and therefore less confidence in the result. However, as a means to alleviate the reviewer's concern and to provide full transparency of our results, we have added to the supplement a full description of sample sizes by age and dyad status as well as comparisons of success by each dyad status for different age categories (Figures S3 and S4 respectively). In addition to this information, we have also added the direct comparisons the reviewer asked for (Figure S5). As you will see, the primary results are supported by these direct comparisons as kin dyads show lower success than non-friend dyads with a similar age difference and gender profile and friend dyads show similar joint success as non-friends who are the same age and same gender (Figure S5) despite the relatively low sample size. Please see additional Figures S3-S5 and L223-228 in the main text.

3- Third, in a few places, the analysis switches between tests of task success across all three trials and tests of the first trial alone. This may be for perfectly logical reasons, but it equally suggests that "researcher degrees of freedom" may have been at play. Unfortunately, the analysis plans were not preregistered. However, a simple solution is to increase transparency: whenever an analysis is performed on the first trial alone, it should be followed by an analysis of all trials.

Our initial experimental design was to conduct a single trial since subsequent trials could be impacted by learning or other events that might occur during the first trial. However, teachers asked us to conduct multiple trials to avoid too many inequalities in their classrooms (some with stickers and others without) – so that children would not end on a failure (this is why children stopped after a successful trial, but continued if they failed). As nearly all dyads completed the task in three trials during our first few sessions, we decided to set three trials as the upper limit. For the sake of transparency, we felt it was important to point out that most dyads completed the task in subsequent trials and that results examining all trials were consistent with those from the first trial only (e.g, results for number of trails to completion between categories mirrors those of success on the first trial), but we understand the worry and confusion from this approach. As such, we have made our experimental design more clear in the text and

kept the first trial only in the main text while placing results from subsequent trials in the supplemental information for complete transparency (Table S1 and Figure S2.) Please see P6-7; L125-134.

4- Fourth, the unique effect of being paired in the task on relationship ratings of non-friends (relative to siblings and friends) may simply be the result of a ceiling effect. Figure 3A shows nearly perfect liking of siblings and friends in the “before” measurement period, and Figure 3B shows a parallel result with respect to happy and neutral emoticons. It is also unclear why these analyses included all pairs rather than just those who managed to successfully complete the task. I would imagine that successful cooperation would lead to greater liking whereas failure would lead to greater disliking, but I did not see any discussion of this.

The reviewer’s point is a valid one which we fully agree with. Indeed, it was not our intention to say that kin or friends did not increase their willingness to play again in the future as they already were near 100% in agreement to play. Our intention, rather, was to show that non-friends were less keen to play with the partner compared to kin or friends initially – as you would expect – and whatsmore, that non-friends were much more willing to play with their partner after the cooperative game compared to the initial response. We have now clarified it in the result section, please see P15 , L325-330.

While including only successful trials would indeed be informative, it was not possible in our experiment since nearly all dyads (90%) succeeded within the three allotted trials (see also explanation in point 3 about why we had additional trials). If it had been possible to stop trials after the first one, we agree that we would expect only successful dyads to improve their perception of friendship. As such, we make this point clear in the description of our analysis in the methods P9- L186-190.

Finally, I also have a few smaller comments on the manuscript:

- The statement on lines 20-21 that the study’s results “indicate that non-kin cooperation in humans has deep developmental foundations” is a bit of an overstatement. On the one hand, if the results had been reversed, as they are in adults, they would not indicate that non-kin cooperation in humans does *not* have deep developmental foundations. This is because the study is comparing different relationship types, not showing that children *always* or *never* cooperate with non-kin. On the other hand, no study needs to be done to show that children cooperate with non-kin, as the mere existence of friendships in childhood makes this patently obvious. A valid point and we have now modified this sentence in the abstract and the main text.

- The manuscript appears to imply at times that, in mechanistic terms, nepotism should depend on age *per se*. That is, children everywhere should be biased towards mutually beneficial relationships with non-kin whereas adults everywhere should be biased towards cooperative relationships (whether mutually beneficial or altruistic) with kin. However, as the authors rightly point out on lines 269-271, one possible explanation for age-related variation in non-kin cooperation is age-related variation in kin competition. If so, it is possible that any putative effects of age on sibling rivalry are due to contextual effects of kin competition rather than age itself. For example, conflict among adult siblings can arise suddenly when a large inheritance is on the horizon (e.g. Daly &

Wilson, 1988). Thus, we might expect considerable cross-cultural variation in the extent to which cooperation and conflict among siblings are expressed over the lifespan. Indeed, the effects of factors such as kin cooperation and kin competition are very likely context dependent. Nonetheless, experiments to date generally show that adults prefer to cooperate with kin over non-kin – at least in the contexts presented in the variety of experiments in the literature. However, the point the reviewer makes is an important one in the broad scheme of things: the costs and benefits of cooperation are likely context dependent as well as age dependent. Exploring the variety of ways cooperation and conflict can shift with context and age would make for a very interesting study, but is beyond the scope of this manuscript. Nonetheless, the general point is important and we have now added text making it clear that cooperation could indeed vary with context and that additional explorations are necessary to evaluate our hypothesis. Please see P17.

- It is unclear whether the rope pulling task is a measure of reciprocity, exactly. The behaviour is certainly cooperative, as the dyad must work together, but there is neither dilemma nor risk at work here: individuals simply cannot acquire the reward (their sticker) without the help of their partner. Indeed, the rope pulling apparatus limits the opportunities for both competition and cheating, making it more of a measure of a “fixed enforcement” kind of policing than reciprocity (West et al. 2007).

We agree that the task we provided was largely focused on cooperation rather than reciprocity (or at least not delayed reciprocity). While cheating was possible (one could quickly grab their reward and let go before the other could get theirs), there was no real benefit in doing so since the sticker was put back again into the device for a new trial (i.e. the child did not get to keep the sticker). As such, “cheating” did not increase the reward for the cheater and there was little if any cost of holding the string a few seconds longer to let the partner get their reward. However, in the introduction we felt that it was important to mention reciprocity since it forms a key reason why non-kin might cooperate across a broad array of interactions beyond our task. We do shift our language towards cooperation when referring specifically to our experiment. Please see P4 and P5 and P6; L112-113.

- The analysis of eye gaze is interesting, but it was not motivated in the Introduction. I did not understand why it was introduced until I got to the Results and Discussion.

The analysis of eye-gaze was a post-hoc analysis to try to better understand why some dyads were successful and others were not. Indeed, this analysis suggested one possible mechanism for why kin were less successful was because they may not pay attention to each other. But given that it was a post-hoc analysis and not one central to the question we wished to pose, we feel that referring to it in the introduction would distract from the main aims. We have clarified the point that this was an analysis to help clarify the primary result and not a main goal of the study. Please see P17; L356-362.

- Lines 209-210 are a bit confusing, as the sentence starts with “Overall, 61% of pairs succeeded in the rope pulling task” when 63% succeeded in the first trial alone. In fact, overall, 90% of the pairs succeeded in the rope pulling task, and the 61% figure is calculated only over the total number of trials. This is fine, but the opening of the sentence should be rephrased to refer to trial success rather than pair success.

We modified this sentence as suggested – indeed, we have now shifted focus in the main text to the first trial only. Please see P6 and P7; L128-134.

To sum up, I think this manuscript describes an interesting and worthwhile study, but I would like a bit more clarity on some details.

Thank you for the constructive feedback and we hope that our modifications have made the results clear.

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West, S. A., Griffin, A. S., & Gardner, A. (2007). Evolutionary explanations for cooperation. *Current Biology*, 17, R661–R672. <https://doi.org/10.1016/j.cub.2007.06.004>