

Theory as guide to the analysis of polygyny and conflict: A response to Ash (2022)

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Abstract

Ash (2022, "Ash" hereafter), in this journal, re-assesses our 2020 article "Polygynous Neighbors, Excess Men, and Intergroup Conflict in Africa." The article analyzed the relationship between polygyny (one man marrying several wives) and intergroup conflict events in Africa. Ash includes assessments that support our original results, though others lead Ash to call our findings into question. While some of Ash's data transformations are reasonable robustness tests, others seem to be insufficiently rooted in theory and ignore statistical power. Specifically, using fatality counts as an outcome requires theorization, in particular against the backdrop of different underlying theoretical mechanisms, issues of measurement accuracy, and other statistical considerations. Overall, we argue that there are good theoretical and statistical reasons why some of the operationalizations in Ash are not appropriate substitutes for our outcome variable. We appreciate the re-assessment but dismiss the claim in Ash that its findings give sufficient evidence to change the conclusion in our article.

Keywords

Robustness, conflict severity, polygyny, intergroup conflict

In a recent contribution to this journal, Ash (2022)¹ assesses the robustness of the results of our study "Polygynous Neighbors, Excess Men, and Intergroup Conflict in Africa" published in the *Journal of Conflict Resolution* (2020).

In this article, we examined the effect of the share of neighboring ethnic groups that historically have had the marriage norm of polygyny on intergroup conflict. We argued that polygyny alters the supply–demand conditions in marriage markets—compared to monogamy—such that fewer men marry multiple women, leaving many men with decreased chances of finding a partner and starting a family. Given the strong normative desire to achieve just that, we argued that young men feel more frustrated and direct their aggression against other ethnic groups. Based on ACLED and UCDP conflict event data, we found evidence for this claim. To elicit the mechanism, we used Afrobarometer survey data and found that childless young men² in polygynous groups are more likely to report feeling subject to unequal treatment and have a higher likelihood of perceiving the use of violence as sometimes justified.

Ash (2022) ("Ash" hereafter) uses several re-adjustments, some of which support our findings and some of which cause him to call our findings into question. The first set of re-adjustments consists of replacing our main outcome variable with alternative operationalizations (aggregating same-day events, dichotomizing event counts, and replacing event counts with fatalities). Secondly, concerning the analysis of the mechanism, Ash sub-sets the data by (a) moving age windows of likely affected men and (b) using our rural/urban control variable to only look at respondents in rural areas. Finally, a case study of two outlier cases is presented.

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We appreciate Ash's re-assessment and think it opens potentially new and interesting pathways for future studies. We do not believe that the findings in Ash change the main interpretations of our study substantively. While there is certainly a limit to what our theory can predict—for example, how fatal intergroup violent events are—we do not think that dichotomizing events or counting fatalities are appropriate substitutes to our approach. Instead, we argue that operationalization should be grounded in theory. Further, we point to ensuing statistical problems and potential measurement errors in Ash. Concerning sub-setting the data to rural respondents (instead of controlling for rural residences), we fear that this leads to problems with statistical power. We discuss these issues below. Finally, the case study evidence is certainly interesting and points to avenues for further research, but does not call into question our evidence on average effects.

In conclusion, we disagree with the statement in Ash that based on the new analysis one could call “the claim that polygyny is associated with rural violence into question” (abstract). Indeed, Ash contains many sensible robustness tests where our original results hold.

Outcome variable replacement in the main analysis

In our main analysis, we regress the number of intergroup conflict events observed for each ethnic group per year (using separate analyses for events measured by ACLED and UCDP) on the percentage of border length shared with neighboring polygynous groups. A large part of Ash focuses on the operationalization of our main outcome variable.

As a first step, Ash collapses ACLED events occurring across multiple days, using location, source, and description as identifiers (Appendix 2). This reduces the number of events considerably (19% are identified as multi-day events), but the effects are robust.

As a second step, Ash introduces an argument by Biggs's (2018) to make the case that a count of conflict events is an inappropriate operationalization. Biggs's main argument is that—for urban protests—the number of participants is more informative than the number of events. In consequence, Ash suggests for our study that fatality counts (as the equivalent of protest size) and binary event incidence measures would be more appropriate outcome variables.

Using battle-related deaths instead of events as the outcome leads to no consistently statistically significant effect of polygynous neighbors, with some statistically significant results in the appendix. While certainly an interesting auxiliary analysis, we have reasons to doubt that fatalities are a theoretically and methodologically more meaningful outcome than our original operationalization.

We note first that there may be issues of directly transferring arguments about protests to the rural conflict events we are interested in.³ The fatality of an event is driven, for instance, by the type of arms and weapons used. Unlike protest size, more deaths can be caused without necessarily needing more people participating in perpetrating violence. The use of such an outcome thus requires theorization: Why would we expect polygynous neighbors to increase the intensity of violence leading to more deaths?

Importantly, it has been shown that we cannot use the same theories for conflict incidents and conflict severity (Clauzet et al., 2007; Lacina, 2006). As shown in Lacina (2006), the predictors of conflict can be quite different from the ones explaining conflict severity and cannot be easily carried over. One important reason for this is that the underlying processes driving incidents and severity (and their resulting data distributions) are inherently different (Clauzet et al., 2007). In general, we still lack a good understanding of what drives the magnitude of conflict.

We write in the original publication that violence as a consequence of polygyny “is largely spontaneous, individualistic, and local” (409). As we indicate in our paper, we see no strong theoretical reason to exclude non-lethal conflict events (414; FN11 on 426), which also was the main reason for including ACLED's data as one of our outcome variables. Retrospectively, we think that this point could have been emphasized more strongly in the article.

Due to the individualistic nature of the grievance that comes from pressure on the marriage market, we argue that the adverse effects of polygyny should result in small-scale events that do not necessarily (but have the potential to) spiral into (fatal) violence. Examples are cattle raids or similar events that occur between ethnic groups and may be caused by individual frustrations and resulting aggression.

On a methodological level, using fatalities as an outcome brings about other issues. It leads to measurement difficulties: Moving from 0 to 1 on the fatality count proxies the move from no (or non-lethal) conflict to lethal violent conflict, whereas anything on the scale beyond 1 measures the intensity of violence. For this reason, many contributions that use conflict fatality as an outcome restrict their sample to ongoing conflicts (e.g., Beardesley et al., 2019; Lacina, 2006).

Furthermore—and as we wrote in the original article—conflict datasets are prone to selection and reporting biases that favor urban areas over rural ones (Weidmann, 2016). This is not only the case for event detection but especially so for measurement accuracy which relates to conflict event descriptions, including fatality data. A recent and growing literature describes the difficulty of accurately measuring the death toll of violent events (Dawkins, 2021; Duursma, 2017; Price and Ball, 2015; Seybolt et al., 2013; Shaver et al., 2022). As a result, fatality data are less reliable, which

should be particularly pronounced when it comes to rural, low-threshold fighting where direct observations by journalists are rarely possible.

In sum, we see good theoretical and methodological reasons to lend less credence to the results in Ash that are based on fatality counts.

Finally, Ash also contains analyses where the original event count variables are dichotomized. But dichotomizing causes more problems than it solves. In general, it has been advised that this is “a practice to avoid” (Dawson and Weiss, 2012). In particular when it comes to outcome variables, it rids the analysis of a lot of meaningful information, only focusing on the cut-off point from no event to at least one event. This not only reduces statistical power but also runs into risks of classifying the data inappropriately. For instance, single events on the territory of ethnic groups are equated with many other scenarios from medium to high event density.

Given these theoretical and methodological considerations, we caution against the view in Ash that the lack of statistical significance using fatalities instead of events and replacing event counts with dummies should call “the claim that polygyny is associated with rural violence into question” (abstract).

Mechanism

Ash also scrutinizes our test of the individual-level mechanism that men in polygynous groups should be more prone to experiencing grievances, which we proxy by using two outcome variables: first, whether respondents feel treated unequally in general and second, whether respondents sometimes see violence as a justified means of action. Our original analysis focuses on men below the age of 40 without children, as these are generally under higher pressure to find a partner.

First, Ash shows that moving this age cutoff does not change our results, which we think is encouraging (Appendix 8). Second, Ash criticizes that we use a rural/urban indicator as a control instead of sub-setting our analysis only to respondents living in rural areas. It is not obvious to us why sub-setting is superior to controlling for rural respondents. Indeed, sub-setting and the resulting reduction in observations bring about one crucial problem: a statistical power reduction. As is clear from Tables 5 and 7 in Ash, sub-setting reduces observations by up to 45%. In consequence, it is not surprising that significance would vanish.

There are also theoretical reasons to caution against this approach: Urban respondents may be rural migrants (a research avenue that Ash suggests). Therefore, they should not be void of ethnic identity and share norms and practices with their kin. Hence, the opinions of urban respondents should be considered and not dismissed. This is of course

different when it comes to conflict event data. In cities, the heterogeneity of events is larger and includes riots, electoral violence, burglaries, and violent protest events that rarely exist in rural areas.

Sub-setting the models to rural-only respondents leads to a significant correlation of polygyny with feelings of inequality but a loss of significance for the violence acceptance outcome (which is significant for urban polygynous respondents). Hence, a core part of our mechanism holds: rural men in polygynous groups perceive, on average, to be treated less equally.⁴

It is important to make clear that using Afrobarometer in this context has clear limits, which we also allude to in the original article. As such, representative samples within ethnic groups would have been required, also for validation of the polygyny indicator (as in Dalton and Leung 2014). Importantly, a growing body of literature examines the consequences of marriage market competition and finds evidence that it affects both violence and individual-level attitudes conducive to violence (e.g., Baranov et al., 2022; Dancygier et al., 2022).

Conclusion

We stand by the findings in our original article. The above considerations show that there are both good statistical and theoretical reasons to reject the claim in Ash to call the results into question.

Family norms and practices are a fairly new topic in peace and conflict research. We believe that there are many avenues for important new research—some of which we reference above—that elicit how such norms affect different forms of conflict as well as individual-level preferences. Our article was a first step in this regard. Of course, there is plenty of room to work with more accurate underlying data to tease out more precise mechanisms and strengthen causal identification. Given this new research agenda, it is important to re-assess and add new results, based on sound theoretical and empirical advancement.

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Notes

1. Ash (2022) is entitled “Does polygyny cause intergroup conflict?”. While we discussed issues of causality and were clear about the direction of our theory, we were also aware of the limits of causally identifying the effect, which is reflected in the language used in our original article.
2. Our georeferenced versions of Afrobarometer data did not include data on marriage.
3. Note that literature on conflict measurement has so far not made this specific argument. Rather, other biases are discussed, some of which we highlight here.
4. The positive effect on perceptions of inequality among women in polygynous societies has been found in the original article and the replication. We argued that this relationship may be confounded by gender inequality (422).

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