JCR-14-0358 ENTITLED "ENEMY AT THE GATES: VARIATION IN ECONOMIC GROWTH FROM CIVIL CONFLICT"

Dear Professor Huth,

We first would like to thank you for the opportunity to revise and resubmit our manuscript. We believe the manuscript has greatly benefitted from the Reviewers' helpful and thoughtful comments. We have thoroughly revised the manuscript, taking seriously each individual point raised by the Reviewers. The revision memo is organized by first responding to your comments and then addressing the reviewers' points. Our comments and responses are shown in *BLUE* below each point.

We hope you agree that the manuscript has greatly improved through this helpful process and we are looking forward to your response.

Sincerely,

The Authors.

1. Editor

In revising your paper you should consider the full range of questions raised by each of the reviewers as they offer thoughtful comments and suggestions for improving the paper. I want to highlight some of those points to emphasize the need for your careful attention to them as you work on revisions.

Regarding R1, there are three issues to highlight.

(1) First, this reviewer would like to see your main model be run on a fixed effects setup with standard errors clustered on country.

There are a number of reasons why we chose a random effects framework, however, we agree with the reviewers that these reasons were not clearly explicated in the text. The following discussion provides three reasons why we employ a random effects framework. We have added the reasoning described below to the relevant section in the actual paper as well. For the "Capital City" and "Any Major City" models we ran a Hausman test to see if fixed or random effects were appropriate for our dataset, and in both cases we do not reject the null hypothesis of the Hausman test at a 90 or 95% confidence interval indicating by at least that standard that both fixed and random effect approaches are consistent, with the latter obviously being more efficient. However, as Clark & Linzer (2015) note the Hausman test should not be the sole determination for choosing between fixed or random effects. They perform a series of Monte Carlo simulations to determine the conditions under which a fixed or random effects model is appropriate and provide a rough typology. Specifically to take into consideration the size of the dataset (both number of units, in this case countries,

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and number of observations per unit, conflict instances) and the level of correlation between the regressor and unit effects. In our case, we have over 70 countries but for over half of those we only have five conflict instances or less. Given such a data structure Clark & Linzer (2015) recommend to examine the level of correlation between the regressor and unit effects to determine the appropriate modeling framework. For both our distance models, the level of correlation between the regressor and unit effects is less than 0.20, which accords with a random effects recommendation under the framework described by Clark & Linzer (2015). Another reason why we choose a random effects framework is because of our concerns with the unchanging and time invariant nature of where conflict is taking place relative to major urban centers. In Thailand, for example, the distance between conflict and urban centers in our dataset just ranges from approximately 790 to 810 kilometers, which basically indicates that conflicts are simply isolated to a specific part of the country. This same patterns holds for many other countries in our sample such as Mozambique (range: ≈ 705 - 860 km), Bangladesh (range: ≈ 198 - 237 km), Cambodia (range: ≈ 131 - 197 km), etc.. These ranges become even further compressed when we log them for use in our regression analysis. If we employed a fixed effects model to test our hypothesis we would in essence be removing many of these types of countries from our sample, or as Beck & Katz (2001) would put it "throwing out the baby with the bathwater".

(2) Second, this reviewer would like to see country year observations without conflict included in the analyses instead of being excluded. I should note that R2 raises these two concerns as well which suggests that it will be critical to address these issues in your revisions.

Including an interaction in this way is problematic for estimating the effect of conflict distance on conflict. The logged, minimum distance variable ranges from approximately 0.33 to 7.31, with closer values indicating that conflict is more proximate to an urban center for that country-conflict-year. This variable is NA for cases in which no conflict occurred for a given country year, thus before we interact it with the civil war variable in a full panel set up we would need to introduce some values for the NAs. One possibility is to simply invert the distance variable and then set the NAs to zero. However, the choice of introducing a zero for the NAs would be arbitrary and problematic, as our variable would no longer truly be continuous. Instead it would be discontinuous in that we would have a large lump of observations at zero and then no observations until 1/7.31. Beyond the problems associated with transforming the variable in this way we would also run into issues of perfect collinearity with the inverted conflict distance measure and the interaction variable. The collinearity would result because the binary conflict variable is zero for every case that the inverted conflict distance variable is zero and one otherwise, meaning that multiplying the two will simply result in the inverted conflict distance variable again. We have spent a fair bit of time trying to think of alternative approaches to modeling this in a full panel context using mixture or hierarchical approaches, but could find no mentions to develop a model for this type of data. Despite this we strongly feel that our results are illustrative of an important and meaningful finding for the conflict literature. There has been very little discussion of the role that the spatial distribution of conflict plays

in shaping macroeconomic outcomes, and the findings that we present here are the first to begin to disentangle this relationship.

(3) Third, this reviewer would like to see you estimate a baseline model for the impact of conflict on growth in which GDP growth is regressed on a dummy for armed conflict in a country. After that, you can then move onto your central argument and compare your findings to this baseline set of findings.

We have run a model on our full panel dataset to calculate a benchmark on the effect of any conflict on GDP growth. The results of this analysis have been added to the paper. To make the baseline model as comparable as possible to the model that we use to test our distance hypothesis, we adopt as similar a specification as possible. Specifically, the dependent variable is again GDP growth. Then our independent variables are, each lagged by one year:

- *Civil war, this is just a binary to indicate whether a civil war took place in that year*
- *Upperincome, this is a binary to indicate whether the country is classified as upper or lower income according to the Worldbank*
- *Logged, inflation*
- *Polity score*
- *Resources as a percent of GDP*
- *Average GDP growth across the world for that year*

We do not include the conflict-specific measures (Distance, Intensity, Duration, and Area) from our distance model since they would just have a value of NA during non-conflict years. The key finding from this analysis is that a civil war is related to lower levels of GDP growth, however, this effect is marginal and only significant at a 90% confidence interval. To determine the substantive significance of a civil war we employ a simulation based approach, similar to what we did to assess the effect of conflict distance from urban centers on growth, and we find that there is a marginal difference but the effect is highly uncertain. We use this finding as a lead in to our main model describing the effect of conflict distance from urban centers on growth.

Regarding R2, there is one additional issue to highlight (points one and two overlap with R1).

(1) The third point raised is to run a robustness check on the more disaggregated ACLED data set to see if your findings hold up.

We retested our hypothesis using the ACLED dataset and we find that our distance measure of conflict to major urban centers is significantly negative. We have included these results in the Appendix under the subsection ACLED Analysis, we have also added in a footnote in the paper indicating that our results remain robust when estimated on this alternative dataset.

2. Reviewer: 1

This paper advances the hypothesis that conflict should exert larger negative impacts on GDP if it located closer to urban centers. This idea is tested using subnational data on conflict location from the PRIO Conflict Site Dataset covering the 1989-2008 period. The

authors present evidence for their hypothesis using a random effects model. Robustness checks are conducted verifying the results are not driven by a particular sub-sample.

I very much like the conceptual hypothesis advanced by this paper, and the idea of using the conflict Site Dataset to test it. However, I am concerned about the empirical specification used for the analysis. Below I outline alternative approaches. If these can be implemented, I believe the paper can make a valuable contribution to the conflict literature.

2.1. **Major comments.**

(1) The random effects model is only appropriate compared to the fixed effects model if there is a compelling reason to believe that time invariant country characteristics are uncorrelated with spatial conflict location and income. The authors dont present a Hausman test to try and make the case for random effects. Moreover, this doesnt seem plausible because geographic factors such as roughness of terrain and climactic conditions in remote areas far from urban centers would determine the cost of launching an insurgency in urban vs. rural areas and also affect GDP. In addition, institutional features such as whether rule of law or state presence extends to rural areas would similarly act as potential omitted variables if country fixed effects are not included. A research question along these lines really should exploit variation within a country and show that as urban conflict location chances, GDP growth changes, instead of relying on comparisons across countries. As such, I would want to see the results hold with the fixed effects model as the primary model, including country and year fixed effects, and with standard errors clustered on country to control serial correlation over time.

- *There are a number of reasons why we chose a random effects framework, however, we agree with the reviewers that these reasons were not clearly explicated in the text. The following discussion provides three reasons why we employ a random effects framework. We have added the reasoning described below to the relevant section in the actual paper as well. For the "Capital City" and "Any Major City" models we ran a Hausman test to see if fixed or random effects were appropriate for our dataset, and in both cases we do not reject the null hypothesis of the Hausman test at a 90 or 95% confidence interval indicating by at least that standard that both fixed and random effect approaches are consistent, with the latter obviously being more efficient. However, as Clark & Linzer (2015) note the Hausman test should not be the sole determination for choosing between fixed or random effects. They perform a series of Monte Carlo simulations to determine the conditions under which a fixed or random effects model is appropriate and provide a rough typology. Specifically to take into consideration the size of the dataset (both number of units, in this case countries, and number of observations per unit, conflict instances) and the level of correlation between the regressor and unit effects. In our case, we have over 70 countries but for over half of those we only have five conflict instances or less. Given such a data structure Clark & Linzer (2015) recommend to examine the level of correlation between the regressor and unit effects to determine the appropriate modeling framework. For both our distance models, the level of correlation between the regressor and unit effects is less than 0.20, which accords with a random effects recommendation under the framework described by Clark & Linzer (2015). Another reason why we choose a random effects framework is because of our concerns with the*

unchanging and time invariant nature of where conflict is taking place relative to major urban centers. In Thailand, for example, the distance between conflict and urban centers in our dataset just ranges from approximately 790 to 810 kilometers, which basically indicates that conflicts are simply isolated to a specific part of the country. This same patterns holds for many other countries in our sample such as Mozambique (range: ≈ 705 - 860 km), Bangladesh (range: ≈ 198 - 237 km), Cambodia (range: ≈ 131 - 197 km), etc.. These ranges become even further compressed when we log them for use in our regression analysis. If we employed a fixed effects model to test our hypothesis we would in essence be removing many of these types of countries from our sample, or as Beck & Katz (2001) would put it "throwing out the baby with the bathwater".

(2) Currently country year observations without conflict are excluded from current specifications. They should not be because if low GDP correlates with periods of no conflict, as might arise if sufficient economic activity is required to finance internal conflicts, then this omission would lead to an upward bias on the estimates.

- *We have run a t-test to test whether GDP is lower during times of no conflict versus conflict and we find that GDP is higher during conflict years than non-conflict years at a 95% confidence interval. Thus the possibility of an upward bias on our main model estimates should not be so severe.*

(3) It will be fairly important to benchmark the results based on distance between conflict location and urban center against the overall effect of conflict. In other words, wed like to know if having any conflict lowers GDP, and if this effect is even stronger if its located closer to urban areas. To capture this, the authors should start with a simple specification that regresses GDP growth on a dummy for if any conflict occurred.

- *We have run a model on our full panel dataset to calculate a benchmark on the effect of any conflict on GDP growth. The results of this analysis have been added to the paper. To make the baseline model as comparable as possible to the model that we use to test our distance hypothesis, we adopt as similar a specification as possible. Specifically, the dependent variable is again GDP growth. Then our independent variables are, each lagged by one year:*
  - *Civil war, this is just a binary to indicate whether a civil war took place in that year*
  - *Upperincome, this is a binary to indicate whether the country is classified as upper or lower income according to the Worldbank*
  - *Logged, inflation*
  - *Polity score*
  - *Resources as a percent of GDP*
  - *Average GDP growth across the world for that year*

  We do not include the conflict-specific measures (Distance, Intensity, Duration, and Area) from our distance model since they would just have a value of NA during non-conflict years. The key finding from this analysis is that a civil war is related to lower levels of GDP growth, however, this effect is marginal and only significant at a 90% confidence interval. To determine the substantive significance of a civil war we employ a simulation based approach, similar to what we did to assess the effect of conflict distance from urban centers on growth, and we find that there is a marginal difference but the effect is highly uncertain. We

use this finding as a lead in to our main model describing the effect of conflict distance from urban centers on growth.

(4) Then they should introduce an interaction between this dummy variable and the conflict distance variable. The coefficient on the dummy in this second specification would then capture effects of 0 distance, or if conflicts occurred in cities.

- *Including an interaction in this way is problematic for estimating the effect of conflict distance on conflict. The logged, minimum distance variable ranges from approximately 0.33 to 7.31, with closer values indicating that conflict is more proximate to an urban center for that country-conflict-year. This variable is NA for cases in which no conflict occurred for a given country year, thus before we interact it with the civil war variable in a full panel set up we would need to introduce some values for the NAs. One possibility is to simply invert the distance variable and then set the NAs to zero. However, the choice of introducing a zero for the NAs would be arbitrary and problematic, as our variable would no longer truly be continuous. Instead it would be discontinuous in that we would have a large lump of observations at zero and then no observations until 1/7.31. Beyond the problems associated with transforming the variable in this way we would also run into issues of perfect collinearity with the inverted conflict distance measure and the interaction variable. The collinearity would result because the binary conflict variable is zero for every case that the inverted conflict distance variable is zero and one otherwise, meaning that multiplying the two will simply result in the inverted conflict distance variable again. We have spent a fair bit of time trying to think of alternative approaches to modeling this in a full panel context using mixture or hierarchical approaches, but could find no mentions to develop a model for this type of data. Despite this we strongly feel that our results are illustrative of an important and meaningful finding for the conflict literature. There has been very little discussion of the role that the spatial distribution of conflict plays in shaping macroeconomic outcomes, and the findings that we present here are the first to begin to disentangle this relationship.*

(5) It wasnt clear to me how 0 distance was dealt with the current specifications since logs were taken but this would eliminate the 0s. It would be very important to include these if in fact conflict in cities are represented by 0 distance in the data.

- *The minimum distance of the centroid of a conflict from the centroid of any major or capital city in our dataset is 0.38 kilometers, so we do not run into issues with zero distance values.*

(6) Instead of treating conflict intensity as a control, it would be useful to re-do the specifications in point 3 above but just restricting to the incidents coded as wars and then restricting to the lower intensity effects. Then the authors can test to see if coefficients on the former are in fact larger.

- *We reran the models per this specification and have included the results in the appendix. In both low intensity and high intensity cases we find that the distance variables remain significant and in the hypothesized direction, but as R1 expected the β estimate of our distance variables is noticeably higher when using high intensity versus low intensity civil conflict cases.*

(7) More information should be provided on how conflict events make it into the Conflict Site Dataset. For example, Figure 1 suggests too sparse picture of violent events in Colombia, given that nearly 1/3rd of the Colombian territory was war affected over this period.
- *The PRIO Conflict Site dataset provides information on the centroid of a conflict and that is what is plotted in each of the conflict map figures. The PRIO Conflict Site dataset then approximates the area that is covered by the conflict. However, the way in which they define this area is problematic as it is not a polygon representing the actual area of conflict, instead they just provide the radius of the conflict zone. If we were to try and construct the distance from a city to any part of a conflict area we would have to define the conflict area as just a perfect circle with a given radius, which would not actually reflect where conflict was taking place.*

(8) I like the use of the descriptive cases but more should be done with these if they will be included. In particular, the economic dynamic of these countries should be mentioned to clarify how the effect of conflict on GDP maps to the hypotheses advanced in the paper. I also wondered about the focus on conflict in the NE in India. Naxalite conflict has also spread rapidly and is also located in rural areas (which is consistent with the authors point), so this would be natural to draw upon as well.
- *We are not familiar enough with the Naxalite conflict to comment but we have added a lengthier discussion in the descriptive cases section to highlight the effect that we have found in our empirical analysis.*

3. Reviewer: 2

The authors claim that the distance between cities (or capital) and the location of violent events during a civil conflict is a first order argument to explain the effect of conflict on economic growth. Im fully convinced of the interest of the question to understand the legacies of civil conflict. Empirical evidence on the effect of civil conflict on growth is notably crucial for the post- recovery policies. While I find the the question very interesting, I think there is a number of issues that remain to be addressed.

3.1. **Major Comments.**

(1) Im convinced by the interpretation of the results but I can imagine also an alternative story. The state capacity is negatively correlated to the distance (to the capital or main cities - see Buhaug, 2010). That means that the fighting cost for a rebel group is decreasing with the distance to the capital. In other words, it is only the strongest (richest) rebel groups that are able to be close enough to the capital. The effect detected in the paper is perhaps only the effect of size groups. The biggest groups are the more violent, the more disruptive and consequently that have an higher effect on economic growth. Im not sure how it will be possible to arbitrate between this alternative story and the story of the authors. At the end, a discussion around this alternative explanation would be profitable to the paper.
- *We do not view this as an alternative to the story that we present, rather we see it as an interesting and valuable extension to pursue in future work. Our key argument is simply that conflicts which are proximate to points of economic*

interest, specifically, the capital and major cities, are likely to have a more deleterious impact on growth. If only strong rebel groups are able to initiate conflicts close to cities that does not directly challenge our argument, rather it adds another important dimension to it. At this point we are uncertain as to whether only strong rebel groups would be able to initiate conflict close to urban centers, but the argument suggested by the reviewer is certainly possible and we consider it a valuable future research question to pursue. We have added a discussion of it to the conclusion of our paper.

(2) The empirical strategy used is a major issue and the authors have to deeply improved this part. The authors mainly use a cross-country comparison using random effects and they justify the use of random effects because their purpose is to explain variation between unit. But the cross-country comparison doesnt involve the use of random effects. The authors missed also to control for time-specific shocks that are common to all countries by including year dummies. The year dummies will absorb yearly worldwide changes such as economic shocks, global climate shocks or natural resource price shocks. I dont believe also the explanation to avoid the use of country fixed effects. I think the authors should follow the following road map:

- Simple correlation between GDP growth and the distance of conflict.
- Inclusion of the control variables.
- Inclusion of year fixed effects.
- Inclusion of both year and country fixed effects.
- *There are a number of reasons why we chose a random effects framework, however, we agree with the reviewers that these reasons were not clearly explicated in the text. The following discussion provides three reasons why we employ a random effects framework. We have added the reasoning described below to the relevant section in the actual paper as well. For the "Capital City" and "Any Major City" models we ran a Hausman test to see if fixed or random effects were appropriate for our dataset, and in both cases we do not reject the null hypothesis of the Hausman test at a 90 or 95% confidence interval indicating by at least that standard that both fixed and random effect approaches are consistent, with the latter obviously being more efficient. However, as Clark & Linzer (2015) note the Hausman test should not be the sole determination for choosing between fixed or random effects. They perform a series of Monte Carlo simulations to determine the conditions under which a fixed or random effects model is appropriate and provide a rough typology. Specifically to take into consideration the size of the dataset (both number of units, in this case countries, and number of observations per unit, conflict instances) and the level of correlation between the regressor and unit effects. In our case, we have over 70 countries but for over half of those we only have five conflict instances or less. Given such a data structure Clark & Linzer (2015) recommend to examine the level of correlation between the regressor and unit effects to determine the appropriate modeling framework. For both our distance models, the level of correlation between the regressor and unit effects is less than 0.20, which accords with a random effects recommendation under the framework described by Clark & Linzer (2015). Another reason why we choose a random effects framework is because of our concerns with the unchanging and time invariant nature of where conflict is taking place relative*

to major urban centers. In Thailand, for example, the distance between conflict and urban centers in our dataset just ranges from approximately 790 to 810 kilometers, which basically indicates that conflicts are simply isolated to a specific part of the country. This same patterns holds for many other countries in our sample such as Mozambique (range: ≈ 705 - 860 km), Bangladesh (range: ≈ 198 - 237 km), Cambodia (range: ≈ 131 - 197 km), etc.. These ranges become even further compressed when we log them for use in our regression analysis. If we employed a fixed effects model to test our hypothesis we would in essence be removing many of these types of countries from our sample, or as Beck & Katz (2001) would put it "throwing out the baby with the bathwater". In terms of the roadmap mentioned by the reviewer, we have added an appendix item that runs the analysis in the way reviewer suggested and reports the results in a tabular format.

(3) Sample. Im not convinced by the sample choice. I would like to see results with a full sample of countries from 1989 to 2008, including peace countries. I expect the author to interact their measure of distance to conflict with a dummy coded 1 for country in civil conflict and 0 otherwise

- *Including an interaction in this way is problematic for estimating the effect of conflict distance on conflict. The logged, minimum distance variable ranges from approximately 0.33 to 7.31, with closer values indicating that conflict is more proximate to an urban center for that country-conflict-year. This variable is NA for cases in which no conflict occurred for a given country year, thus before we interact it with the civil war variable in a full panel set up we would need to introduce some values for the NAs. One possibility is to simply invert the distance variable and then set the NAs to zero. However, the choice of introducing a zero for the NAs would be arbitrary and problematic, as our variable would no longer truly be continuous. Instead it would be discontinuous in that we would have a large lump of observations at zero and then no observations until 1/7.31. Beyond the problems associated with transforming the variable in this way we would also run into issues of perfect collinearity with the inverted conflict distance measure and the interaction variable. The collinearity would result because the binary conflict variable is zero for every case that the inverted conflict distance variable is zero and one otherwise, meaning that multiplying the two will simply result in the inverted conflict distance variable again. We have spent a fair bit of time trying to think of alternative approaches to modeling this in a full panel context using mixture or hierarchical approaches, but could find no mentions to develop a model for this type of data. Despite this we strongly feel that our results are illustrative of an important and meaningful finding for the conflict literature. There has been very little discussion of the role that the spatial distribution of conflict plays in shaping macroeconomic outcomes, and the findings that we present here are the first to begin to disentangle this relationship.*

(4) Data on conflicts. One could imagine that the quality/quantity of reports on conflicts is negatively correlated to this distance to the capital. I would appreciate a discussion on the potential report bias and how it could influence the results. As a robustness,

I expect the authors to use ACLED data that are commonly used now as data for disaggregated analysis.

- *We retested our hypothesis using the ACLED dataset and we find that our distance measure of conflict to major urban centers is significantly negative. We have included these results in the Appendix under the subsection ACLED Analysis, we have also added in a footnote in the paper indicating that our results remain robust when estimated on this alternative dataset.*

(5) To avoid issues linked to reverse causation, I would consider the list of urban centers at the beginning of the period.

- *We reran our model on the effect of conflict proximity on GDP growth using the list of urban centers at the beginning of the period. The results are shown in table 1 below. Running the same analysis using capital cities is difficult as they change over time in our sample.*

TABLE 1. Rerunning model on the effect of conflict proximity using list of urban centers at the beginning of the period.

	Dependent variable:
	$\Delta\ GDP_t$
$Ln(Min.\ City\ Dist.)_{t-1}$	1.144^{***}
	(0.424)
$Intensity_{t-1}$	-1.197
	(0.973)
$Duration_{t-1}$	0.141^{***}
	(0.037)
$Area_{t-1}$	-4.965^{***}
	(1.283)
$Number\ of\ conflicts_{t-1}$	1.475^{**}
	(0.624)
Upper Income	1.038
	(2.742)
$Ln(Inflation)_{t-1}$	-2.586^{***}
	(0.471)
$Democracy_{t-1}$	-0.008
	(0.091)
$Resource\ Rents/GDP_{t-1}$	0.073^{**}
	(0.036)
$World\ GDP\ Growth_t$	0.549^{**}
	(0.271)
Intercept	1.688
	(3.455)
Countries	69
Observations	505
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

(6) Definition of variables. The authors take the minimum distance to the conflict as explanatory variable. I would like to see alternative measures to ensure that the results are not sensitive to the definition of the main variable. For instance, the weighted (by the distance) sum of the number of events is a credible candidate as an alternative measure.

- *We have looked at alternative parameterizations of the conflict distance measure including both the minimum and mean distances of conflicts to the respective nearest cities. Our results are robust across these parameterizations. We specifically chose not to create an aggregate conflict variable weighted by distance as we lacked theoretical justification for doing so given the argument advanced in the paper. If a country experiences both a rural and an urban conflict simultaneously, we do not expect the rural conflict to mitigate the economic consequences of the urban conflict. In the same way, we do not expect distant cities to compensate for the economic consequences felt by a city near conflict.*

3.2. **Minor Comments.**

(1) Introduction. The example on Mexico looks strange. By many aspects, the drug war in Mexico is very different to conflicts in Republic Democratic of Congo or in Uganda. In other words, it is difficult to compare conflicts with genocide, massive internal migration, ethnic cleavages with a drug war where almost all citizens support the government. The recent case of Nigeria and Cameroon with Boko Haram looks to be a better fit with the story of the authors.

- *We appreciate this suggestion and have included the cases of Nigeria and Cameroon rather than profiling Mexico at the lead of this paper.*

(2) The authors claim they focus on the proximities of conflict to cities and not on the area covered by the conflict. Im wondering whether the effect of the proximities of conflict to cities would be intensify by the area of the conflict. I expect the effect of the proximities of conflict to cities to be higher if the area of conflict is biggest. A very simple interaction term between distance and area would be appropriate to uncover this mechanism.

- *We agree that this is an interesting hypothesis and would constitute a valuable extension to this research. However, we believe that this hypothesis is not a clear corollary to the arguments presented in this paper and deserves more consideration than a simple interaction term. Conflict area is not necessarily a proxy for conflict intensity and it is unclear to us that, controlling for intensity as we do, conflicts with larger areas that are nearer to cities will be of necessarily greater economic impact. In fact, one might argue that there is a dilution effect in which larger conflicts, controlling for intensity, produce less violence per square kilometer. Also, an interaction term like this supposes that there is a multiplicative effect such that* conflict area × conflict distance *is correlated with economic growth in a way that* conflict area + conflict distance *is not. We reiterate that this is a very interesting avenue for further exploration but believe that we cannot address it adequately within the scope of this paper.*

(3) Results. Conflict duration and the number of conflicts a country is facing have an unexpected effect on growth. I would see a discussion to explain this results.

- *With respect to the duration finding our argument would be that more important to determining the effect of conflict on growth is its proximity to urban centers.*

Conflicts can be long lasting have an ambiguous effect on growth if they are simply occurring in the periphery of countries. We certainly would not have argued for a positive effect of conflict duration on growth prior to running the analysis, but the fact that it has occurred may open up future, interest research questions. Our response to the unexpected effect of the number of conflicts a country has faced on growth is similar. We would again argue that it is not just that a number of conflicts our occurring that would dampen growth but where they are occurring is what matters.

(4) Area covered by conflict. I dont understand why the authors use a binary measure instead of the continuous variable.
   - *When rerunning the model using the continuous variable each of the key results remain similar. We have used the continuous version of the variable in the revised draft.*

(5) Figures have to be self contained.
   - *We have added in additional information to aid in interpreting the visual results where possible. However, we do have preferences for presenting additional information in the captions instead of by adding in legends.*

(6) The visual presentation of the results are interesting but the classical way to present results (through tables) is a requirement.
   - *We have added in tabular representations of each of the key regression models.*