Dear Editor,

We appreciate the two Reviewers' perceptive and helpful comments and suggestions on our manuscript entitled "Observed Trends of Clouds and Precipitation (1983–2009): Implications for Their Cause(s)" (MS No.: acp-2020-577). We have carefully considered all comments and suggestions and carried out major revisions as suggested. We believe that the revisions have resulted in a significantly improvement of the paper. Listed below are point-by-point responses to all comments and suggestions of the two reviewers (ordered by the time of review came to us). Reviewer's points are in black, our responses in blue.

Anonymous Referee #2

I have read through the revised paper, and find that many of the major issues originally raised by the reviewers still largely remain in the manuscript.

1. Reviewer 3 stated that "the authors need to show that the relationships between global temperature and regional variations in cloud cover and precipitation are consistent when linear trends are removed." The authors have done this analysis in Table S1, and found that this is actually not the case. In fact, they conclude that the "high correlation coefficients are nearly entirely contributed by the long-term linear trends." As I stated in my original review, just because global temperatures are warming, it doesn't mean that concurrent trends in clouds and precipitation are necessarily caused by global warming. The similarity in Figs. 1 and 4 is by construction, as the global temperature time series is dominated by an increasing trend (so any trend in clouds and precipitation will by definition be highly correlated with global temperature). While the authors have acknowledged this problem at various points in the manuscript (for example, see paragraph starting on line 325), the large amounts of variance attributed to global warming, AMO, and PDO are still discussed throughout as a key conclusion of the manuscript (line 11). A much more careful analysis (such as that discussed in Chen et al. 2019) needs to be performed to more precisely partition the recent trends into the

global warming, PDO, and AMO components.

Response:

We worked very hard on the first revision, here we hope the second revision would get this reviewer's approval. We fully agree that a good correlation of concurrent trends in clouds and precipitation with the trends of global warming, PDO, or AMO does not imply any cause-effect relationship, thus cannot be used to partition the recent trends in clouds and precipitation into the global warming, PDO, and AMO components. As this reviewer also noted that this point was stated repeatedly in our original as well as the first revised manuscripts. It was a major reason we decided to focus our analysis on two critical regional characteristics of the trends in clouds and precipitation: namely the widening of the global Hadley and Walker circulations (see below changed to: the broadening of the major ascending zone of Hadley circulation) and the long-running trends in the high quality station data of clouds and precipitation in China, to help partitioning the recent trends in clouds and precipitation into the global warming, PDO, and AMO components. Surely as this reviewer stated that regional results might not be representative of the global phenomena, nevertheless the partitioning established globally should be evaluated more rigorously regionally. In addition, the area of the rectangles in Fig. 2 covers about one third of the entire domain in this study and it includes most of the prominent features in the trends in clouds and precipitation. In regard to the analysis of clouds and precipitation in China, we have the advantage of the large number of long-running, high-quality surface weather stations over the period of 1957–2005 (1957–2017 for precipitation). The long-running data enable the analysis to be carried out over a period in which the linear trends of AMO and PDO have both diminished to insignificant values. More importantly, the high-quality data allow us to make some critical analyses without using the correlation method, which has an intrinsic weakness in implying a cause-effect relationship as discussed above.

We have used a straightforward arithmetic analysis of the relationship between interannual variabilities in cloud cover and light precipitation in China, which provides evidence of a quantitatively matching closure between the variabilities of light precipitation and those of cloud cover. Furthermore, the cause-effect relationship between the changes in precipitation intensity and global warming has been investigated in a large number of studies which include theoretical, modeling as well as correlational approaches (e.g. Trenberth, 1998; Allen and Ingram, 2002; Trenberth et al., 2003; Sun et al., 2007; Liu et al., 2015, 2016). These studies concluded that the extensive worldwide reports of enhancements in heavy precipitation and reductions in the light and moderate precipitation are most likely a result of global warming and the primary driving mechanism is the moisture-convection-latent heat feedback cycle associated with global warming.

We choose not to follow the modeling analysis used by Chen et al (2019) because current climate models tend to have large uncertainties, particularly in the simulation of regional distributions of clouds and precipitation, as evident by the low model performance rating during the IPCC model evaluation (Flato et al., 2013).

2. All three reviewers found the suggested linkage of the results with Hadley cell expansion to be inadequately supported and recommended further analyses. To address these comments, the authors have added two figures (Figs. 2-3) to describe a broadening of high cloud cover and precipitation over the deep convective region over the western Pacific Ocean centered on Indonesia. The authors conclusively show that this region has expanded in recent decades, which is an interesting new result. However, the authors attribute this very local feature to a widening of the global Hadley and Walker circulations (line 146), but do not show any evidence to support this claim. For example:

- Figures 2 and 3 combine changes in the zonal and meridional directions, making it impossible to tell which direction is most contributing to the changes.

- No analysis is provided for the eastern tropical Pacific Ocean. To show that the Walker circulation is widening, the authors need to show that the descending branch is not changing. Figure 3 only shows changes in the ascending branch.

- The Hadley cell is a zonal-mean quantity. Changes in the ascending region over the western Pacific Ocean do not necessarily imply changes in the zonal mean.

- To show that the Hadley cell is widening, the authors need to show that the descending branch is moving. Even if the deep tropical precipitation is broadening, it doesn't necessarily imply that the descending branch is moving.

Because of these two major issues remaining in the manuscript, I cannot recommend publication at this time.

Response:

Both reviewers raised this concern on the widening of Hadley circulation and recommended changing some of the basic terminology used. We acknowledge that what we showed in Fig. 3 of the first revision should be more precisely described as the broadening/expansion of the major ascending/wet zone of Hadley circulation, rather than widening of the Hadley circulation. Therefore, we decide to accept both reviewers' suggestion and change "the widening of Hadley circulation" to "the broadening of the major ascending zone of Hadley circulation" throughout the paper. Having said that, we would like to explain that, in our view, Hadley and Walker cells are two components of one single atmospheric circulation; and the expanding/broadening convective region over the western Pacific Ocean within the rectangles in Figure 2 is by far the predominant ascending branch of Hadley and Walker cells, which encompasses as much as one third of the entire domain in this study. As stated in our first revision (Lines 155–157), Zhou et al. (2011) had already shown that the broadening of this ascending branch of Hadley and Walker cells is a primary contributor to the widening of Hadley cell. That is why we misnamed "the broadening of the major ascending zone of Hadley circulation" as "the widening of Hadley circulation".

Line 11: "and negligible" --- awkward phrasing … I would remove referring to ENSO in the abstract if it's negligible.

Response:

Done as suggested.

Line 166: rectangle

Response:

Corrected.

Lines 167, 210: I don't see the close correspondence between Figs. 3e and 3f.

Response:

We have clarified this point by revising the text near Lines 208–215 to "The quantity of global total annual precipitation, which is equal to global evaporation and determined by the global surface energy budget, increases with global temperature at a rather small rate of about 2%–3% K^{-1} (Cubasch et al., 2001), which is manifested in Figs. 3a and 3b by the small/negligible change of the net area between blue and black lines, while Figs. 3c and 3d have significant enhancements. Therefore, based on the results of Figs. 3a-3d, we propose that the trend in global temperature, rather than that of AMO and PDO, is the primary contributor to the observed linear trend of precipitation in 1983–2009. Likewise, Figs. 3e and 3f both have small/negligible changes of the net areas between blue and black lines, while Figs 3g and 3h have significant enhancements of cloud cover. Accordingly, we propose that the trend in global temperature, rather than that of AMO and PDO, is the primary contributor to the observed linear trend of cloud cover in 1983–2009."

Lines 213-214: Incorrect figure (Fig. 2e) is cited here.

Response:

Sorry, changed to Fig. 3e.

Line 242: AMO loses? … not sure what is meant by this, please rephrase

Response:

Thanks. The associated sentence near Line 249 has been rephrased to "The long-running data enable the analysis to be carried out over a period in which the linear trends of AMO and PDO have both diminished to insignificant values."

Line 247: Figure 6

Response:

Done.

Line 259: Fig. 7a

Response:

Done.

Line 266: Fig. 7b

Response:

Done.

Line 270: Fig. 8

Response:

Done.

Figure 2 caption: Need to clarify that these maps are showing cloud cover trends from Fig. 1.

Response:

Done as suggested.

Statistical significance in Tables 1 and 2: As per my previous comment, the authors shouldn't be using a sample size of n to calculate the p-value of the correlation coefficient, but rather an effective sample size (n*) that accounts for the autocorrelation in the time series. See equation 31 of Bretherton et al. (1999): https://doi.org/10.1175/1520-0442(1999)012<1990:TENOSD>2.0.CO;2

Response:

Sorry for this mistake. Per your suggestion, we followed Bretherton et al. (1999) to recalculate the effective sample size (N^{*}_{ef}) and perform the statistical significance test. Now the Table 1 is revised as follows:

R	Trend of TCC	Trend of TP
δ(GT)	0.82 ***	0.93 ***
δ(-PDO)	0.62 ***	0.73 ***
δ(AMO)	0.70 ***	0.77 ***
δ(Niño3.4)	-0.20 ***	0.02
δ(GT)+δ(-PDO)	0.74 ***	0.85 ***
δ(GT)+δ(AMO)	0.86 ***	0.89 ***
δ(GT)+δ(Niño3.4)	0.89 ***	0.93 ***
δ(-PDO)+δ(AMO)	0.67 ***	0.79 ***
δ(-PDO)+δ(Niño3.4)	0.61 ***	0.72 ***
δ(AMO)+δ(Niño3.4)	0.65 ***	0.73 ***
δ(GT)+δ(-PDO)+δ(AMO)	0.76 ***	0.87 ***
δ(GT)+δ(-PDO)+δ(Niño3.4)	0.72 ***	0.84 ***
δ(GT)+δ(AMO)+δ(Niño3.4)	0.86 ***	0.88 ***
δ(-PDO)+δ(AMO)+δ(Niño3.4)	0.65 ***	0.78 ***
δ(GT)+δ(-PDO)+δ(AMO)+δ(Niño3.4)	0.75 ***	0.86 ***

Note: GT denotes global temperature anomalies. δ(GT) denotes ΔGT×dTCC/d(GT/GTσ) or ΔGT×dTP/d(GT/GTσ), where ΔGT is the change of GT for the studied period and GTσ is the standard deviation of GT, and other factors likewise. *** indicates statistically significant at the 99% confidence level based on student's t test.

For the comment "A trend of 0% (see T60%) should not be statistically significant at all", sorry for our carelessness, we checked our calculation again and found that we mislabeled 0 with ***. The corresponding p-value was 0.75. Table 2 is revised as follows:

Table 2. Climatology and days changed for precipitation days and cloudy days

	Climatology (day)	Change rate (day per decade)	Relative change rate (% per decade)	Change over 49 years (day)	Relative change over 49 years (%)
NPD	202.5	4.5±0.2 ***	2.2±0.1 ***	22.1±1.0 ***	10.9±0.5 ***
B10%	116.9	-4.2±0.2 ***	-3.6±0.2 ***	-20.6±1.0 ***	-17.6±1.0 ***
B20%	132.0	-4.3±0.2 ***	-3.3±0.2 ***	-21.1±1.0 ***	-16.0±1.0 ***
B30%	141.2	-4.4±0.2 ***	-3.1±0.1 ***	-21.6±1.0 ***	-15.3±0.5 ***
B40%	147.5	-4.5±0.2 ***	-3.1±0.1 ***	-22.1±1.0 ***	-15.0±0.5 ***
T60%	15.0	0±0	0±0	0±0	0±0
CFD	34.9	2.3±0.1 ***	6.6±0.3 ***	11.3±0.5 ***	32.3±1.5 ***
≤50%	152.3	4.3±0.2 ***	2.8±0.2 ***	21.1±1.0 ***	13.7±1.0 ***
>50%	212.7	-4.3±0.2 ***	-2.0±0.2 ***	-21.1±1.0 ***	-9.9±1.0 ***

Note: *** indicates statistically significant at the 99% confidence level based on student's *t* test. NPD denotes non-precipitation days, B10% denotes bottom 10% precipitation days, T60% denotes top 60% precipitation days, ≤50% denotes ≤50% cloud cover days and CFD denotes cloud-free days.

We have added corresponding description on Lines 116–117.

Table S2 is not discussed at all in the main text.

Response:

Table S2 is deleted.

Anonymous Referee #1

I commend the authors for the significant work in revising their manuscript. Overall, it has been substantially improved. However, there still seems to be a bit of disconnect in some of their statements, particularly those related to what is meant by expansion of the Hadley cell.

To summarize, I feel as if the authors are not using correct terminology, as has been previously established in (many) prior publications. Which adds confusion and makes interpretation of the results more difficult than is necessary. I would recommend changing some of the basic terminology used throughout the paper, such that it is consistent with prior publications.

Comments

As mentioned in the first round of reviews, Hadley cell expansion refers to a poleward displacement of the outer edge of the circulation. There are numerous publications that have used this definition. In the context of precipitation, it has been defined as a poleward shift of the subtropical latitude where P-E = 0; the subtropical latitude where P is a minimum could also work. It has also been pointed out that looking at this separately in each hemisphere is important, because the NH and SH show different tropical expansion signals.

The authors choose to construct their own methodology to define “tropical expansion”, based on the rectangular boxes. That’s fine. But they do not really give a simple explanation for what this new methodology is calculating. It appears to me, based on panel a in Figure 3 from the response, that precipitation is not changing near the center of the region (inner rectangles), but it is increasing in the outer regions (outer rectangles). I assume precipitation is estimated in each rectangle independently, and not summed over the inner rectangles? In any case, this would seem to suggest an increase in tropical precipitation starting near layer 6 and extending outwards to layer 15. In other words, tropical precipitation in this region is moving outwards from the center rectangle? Or maybe a better description is that there is just an increase in tropical precipitation moving outwards from the local maximum (assuming the inner rectangle is a local maximum)? This is not “tropical expansion” as used in numerous other publications. To add confusion, the authors also describe this as not only Hadley cell expansion, but also expansion of the Walker circulation.

The center of the rectangles is “in the middle of Kalimantan, Indonesia which is located near the major ascending/wet zone of Hadley cell”. What is meant by “near” the major wet zone? Is the center rectangle chosen so that it represents a local maximum in precipitation?

The authors then go on to say (again, in the response): “In summary, the spatial distributions of the linear trends of total cloud cover and precipitation are characterized primarily by a widening of the center of precipitation (ascending/wet zone of Hadley cells) over the Maritime Continent in all directions”.

Yes, I agree with this description. But this is not what is meant by “tropical expansion”. This is more related to the thickness of the band of intense tropical rain, right? And the authors are showing an increase in this “thickness”? Perhaps this is not related to “tropical expansion”. But it does seem to be interesting, as others have shown that under continued GHG increases, the ITCZ is projected to narrow (or decrease its “thickness”). But the authors are showing the opposite. It would appear that the authors should remove “tropical expansion” type statements and verbiage, and instead replace this with something more similar to what they are quantifying—“thickness” or area, etc. of the intense precipitation over the Maritime continent.

I think the last reviewer summarized this concern well:

Line 69, 131-132, 138-140: See major comment #4. The expansion of the Hadley cells has nothing to do with enhancement of tropical precipitation. It is related to subtropical static stability (Chemke and Polvani 2019: https://doi.org/10.1175/JCLI-D-18-0330.1). If anything, an expansion of tropical precipitation would contradict the literature, which suggests a narrowing of the Intertropical Convergence Zone in a warming climate (Byrne and Schneider 2016: https://doi.org/10.1002/2016GL070396; Su et al. 2017: https://www.nature.com/articles/ncomms15771).

Sorry for the confusion, we should have said that our result on the broadening of the major ascending zone of Hadley circulation applies only to the rectangles circling the major ascending zone of Hadley circulation. We also noticed significant contraction of the ITCZ between 80°W–180°W, as shown in Fig. 1b.

But as can be seen, the authors have not really addressed this confusion. They continue to refer to their signal as "tropical expansion" (as well as expansion of the Walker circulation). Furthermore, their response again seems to be disconnected from the comment. The comment is pointing out that the authors are not using the term "tropical expansion" properly. And instead of changing it, they simply say that this is controversial? I don't quite follow.

Response:

Both reviewers raised this concern on the widening of Hadley circulation and recommended changing some of the basic terminology used. We acknowledge that what we showed in Fig. 3 of the first revision should be more precisely described as the broadening/expansion of the major ascending/wet zone of Hadley circulation, rather than widening of the Hadley circulation. Therefore, we decide to accept both reviewers' suggestion and change "the widening of Hadley circulation" to "the broadening of the major ascending zone of Hadley circulation" throughout the paper. Having said that, we would like to explain that, in our view, Hadley and Walker cells are two components of one single atmospheric circulation; and the expanding/broadening convective region over the western Pacific Ocean within the rectangles in Figure 2 is by far the predominant ascending branch of Hadley and Walker cells, which encompasses as much as one third of the entire domain in this study. As stated in our first revision (Lines 155–157), Zhou et al. (2011) had already shown that the broadening of this ascending branch of Hadley and Walker cells is a primary contributor to the widening of Hadley cell. That is why we misnamed “the broadening of the major ascending zone of Hadley circulation” as “the widening of Hadley circulation”.

About the calculation for Fig. 3, the reviewer’s understanding is correct. To make this easily understandable, we have added “The summing up was done for each rectangle independently, the inner rectangles were not included.” on Lines 141–142. The center rectangle (5 degree wide in latitude and 55 degree wide in longitude) was chosen because it locates near the major ascending zone of Hadley cell which also coincides with the local wet zone.

Regarding this response:

Direct effects of anthropogenic aerosols on clouds and precipitation tend to be regional and/or sub- yearly time scale, which are beyond the scope of discussion in this study

Obviously, this is a small point and not particularly important to the main analysis. But again, there seems to be a disconnect in the response. Why are possible aerosol effects on cloud/precipitation important on only sub-yearly time scales? I do not think this is true. There have been multi-decadal changes in anthropogenic aerosol emissions, which leads to multi-decadal changes in aerosol forcing. So it would stand to reason that such a multi-decadal forcing may in fact lead to long term changes in temperature, clouds, precipitation, etc.

It’s a bit odd (and frustrating) that I’ve pointed out past papers that have addressed causes of tropical expansion (e.g., aerosols, as well as the PDO), and the authors have chosen to disregard any acknowledgement of these prior papers. Why not add a simple sentence in the introduction that points out prior papers that have addressed the causes of tropical expansion? But then again, I do not think what the authors show is really “tropical expansion”, and therefore, the causes of their signal may very well have nothing to do with previously identified causes of tropical expansion. So in a round-about way, lack of addressing this point fine, I suppose.

Response:

In the original manuscript as well as the first revision, we did address the issue of "There have been multi-decadal changes in anthropogenic aerosol emissions, which leads to multi-decadal changes in aerosol forcing". This can be seen in Lines 235–236 "The long-term radiative effect of aerosols on the global temperature and other climate parameters are expected to be imbedded in the observed changes of these climate parameters, and thus included in this study."

The effects of anthropogenic aerosols on clouds and precipitation by acting as cloud condensation nuclei (CCN) is a highly controversial issue. We apologize for our timidity in trying to deal with this issue in earlier manuscripts. In this revision, this issue is addressed by adding Lines 234–239: "The effects of anthropogenic aerosols on clouds and precipitation by acting as cloud condensation nuclei (CCN) is a highly controversial issue which has been discussed extensively in a number of studies as well as one of our earlier papers (Liu et al., 2015). We defer the discussion on this issue to future studies, and acknowledge here that the CCN effects could introduce an unknown amount of uncertainty in this study." In addition, we elaborate below the controversy of this issue by quoting a key paragraph from Liu et al. (2015):

"It has been long recognized that aerosols may have a significant influence on clouds and precipitation by acting as cloud condensation nuclei (Warner and Twomey, 1967; Albrecht, 1989; Ramanathan et al., 2001; Andreae et al., 2004; Dai et al., 2008; Koren et al., 2008). The aerosol effect on precipitation processes, considered part of the "Albrecht" effect—the "second indirect" effect on cloud extent and life time (Ackerman et al., 1978; Albrecht, 1989; Hansen et al., 1997)—is complex and uncertain, especially for mixed-phase convective clouds (Tao et al., 2012). There have been numerous studies conducted on the effects of aerosol on total precipitation over different periods (e.g. annual, seasonal), producing mixed results. An excellent example is Warner (1971), who concluded there was no change in 60 years of precipitation due to aerosols emitted from sugarcane burning in northern Australia. In addition, a report by the U.S. National Research Council (2003) concluded “there still is no convincing scientific proof of the efficacy of international weather modification efforts,” of which many are modification efforts using aerosols.”

The authors again use this terminology:

Further analysis of the widening of the Hadley and Walker circulations (Figures 3a-3h)

What is meant by widening of the Walker circulation?

The authors also use this description of their signal, which is more reasonable in my opinion: “widening of the center of precipitation over the Maritime Continent in all directions”. And maybe “broadening” in more appropriate than “widening”, as widening implies an increase in one spatial direction, whereas broadening is more general.

Response:

This point has been addressed in our response to this reviewer’s first comment.

Another example of a disconnect between reviewer comment and author response pertains to significance:

Table 1: How are you evaluating significance? I have a difficult time believing that a correlation of 0.02 is still significant at the 95% confidence level. Are you taking into account autocorrelations among neighboring grid points, which would greatly reduce the number of degrees of freedom in your t-test? Table 2: Similarly, how is significance being evaluated here? A trend of 0% (see T60%) should not be statistically significant at all, especially at the 99% level.

We used the function imbedded in R named corr to do this significance test. The function corr we chose applies Pearson correlation formula…The p-value of the correlation is determined by calculating the t value as follow…then using t distribution table for the degrees of freedom: df = n-2 to get the p-value. We believe even when the correlation coefficient r is very small, due to the big value of n (the number of samples we used in calculation), the t value should remain a very big value, therefore brings a reliable significance.

Yes, the small correlations (e.g., 0.02) are deemed significant in this analysis likely due to the large n. But this comment is suggesting n is not as large as what the authors are using, due to spatial autocorrelation. But the authors do not address this point.

Response:

Sorry for this mistake. As we have addressed in the response to reviewer #2's comment, we followed Bretherton et al. (1999) to recalculate the effective sample size (N^{*}_{ef}) and perform the statistical significance test. Now the Table 1 is revised as follows:

R	Trend of TCC	Trend of TP
δ(GT)	0.82 ***	0.93 ***
δ(-PDO)	0.62 ***	0.73 ***
δ(AMO)	0.70 ***	0.77 ***
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δ(-PDO)+δ(AMO)+δ(Niño3.4)	0.65 ***	0.78 ***
δ(GT)+δ(-PDO)+δ(AMO)+δ(Niño3.4)	0.75 ***	0.86 ***

Note: GT denotes global temperature anomalies. δ(GT) denotes $\Delta GT \times dTCC/d(GT/GT\sigma)$ or $\Delta GT \times dTP/d(GT/GT\sigma)$, where ΔGT is the change of GT for the studied period and GTσ is the standard deviation of GT, and other factors likewise. *** indicates statistically significant at the 99% confidence level based on student's t test.

For the comment "A trend of 0% (see T60%) should not be statistically significant at all”, sorry for the carelessness, we checked our calculation again and found that we mislabeled 0 with ***. The corresponding p-value was 0.75. Table 2 is revised as follows:

Table 2. Climatology and days changed for precipitation days and cloudy days

	Climatology (day)	Change rate (day per decade)	Relative change rate (% per decade)	Change over 49 years (day)	Relative change over 49 years (%)
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B10%	116.9	-4.2±0.2 ***	-3.6±0.2 ***	-20.6±1.0 ***	-17.6±1.0 ***
B20%	132.0	-4.3±0.2 ***	-3.3±0.2 ***	-21.1±1.0 ***	-16.0±1.0 ***
B30%	141.2	-4.4±0.2 ***	-3.1±0.1 ***	-21.6±1.0 ***	-15.3±0.5 ***
B40%	147.5	-4.5±0.2 ***	-3.1±0.1 ***	-22.1±1.0 ***	-15.0±0.5 ***
T60%	15.0	0±0	0±0	0±0	0±0
CFD	34.9	2.3±0.1 ***	6.6±0.3 ***	11.3±0.5 ***	32.3±1.5 ***
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Note: *** indicates statistically significant at the 99% confidence level based on student’s *t* test. NPD denotes non-precipitation days, B10% denotes bottom 10% precipitation days, T60% denotes top 60% precipitation days, ≤50% denotes ≤50% cloud cover days and CFD denotes cloud-free days.

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