

Interactive comment on “Toward a classification of the Central Pacific El Niño” by M. Pascolini-Campbell et al.

Anonymous Referee #2

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In the last few years, a phenomenon different from the canonical ENSO has been identified in the tropical Pacific (Ashok et al., 2007, Kao and Yu, 2009; Kug et al. 2009). Notwithstanding the possibility that it is only a different flavor of ENSO (Trenberth et al., 2001, Larkin and Harrison 2005), or that the events are comparable to moderate ENSOs (Takahashi et al., 2011), substantial literature evolved that shows the distinct impacts and distinct evolution of the events, particularly since late 1970s. The increasing frequency of the Modoki events in the recent decades, also referred to as the CP events in some publications, has been attributed to the anthropogenic activities (Yeh et al., 2009), while some studies indicate that they may be a natural manifestation (Wittenberg et al. GRL 2009, and other works that the authors cite in this manuscript). In the current work, the authors aim at clarifying the classification of the “CP events”. In the process, they introduce a new definition. The idea of consolidating results from

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earlier defined indices, and using it to identify past events since 1900s is and potentially interesting. What was most interesting to me, however, is the claim that the CP events have also occurred in the past. However, the manuscript suffers from serious limitations such as incorrect interpretation of earlier work. Serious lapses, such as not considering the time evolution constraint of the ENSO Modoki definition while using the corresponding index, result in incorrect interpretations. The poor quality of earlier SST data is another issue. Finally, in addition to proposing a “new” index, it would need more analysis using other data sets such as atmospheric reanalysis (and available station data sets or derived indices such as the SOI) to demonstrate the presence of past events. Analysis of outputs from long simulations would also help. Given all these limitations, unfortunately, the current work cannot be recommend for publication. Major Comments: 1. Fig. 3c: The criterion for identifying the CP events is not clear. From this figure, it is clear from the consolidated analysis that, until late 1970s, there have been any CP events with probabilities above 0.4. There are hardly 5-6 events that exceed the value (even if one assumes that this low value of 0.4 as the threshold), all after 1970s. This contrasts with the conclusion that CP events existed prior to 1970s. 2. Definition of the EMI (table 1): To identify an event as a Modoki, Ashok et al. 2007 have proposed that the maximum warming should persist in the central tropical Pacific for at least through 3 seasons (i.e. boreal summer through following boreal winter). Accordingly, they identified seven events since 1979 (1986-87, 1990-91, 1991-92, 1994-95, 2002-03, 2004-05) only after verifying that the EMI exceeds the threshold of 0.7σ in boreal summer and following boreal winter, and not just for winter, as presented in the table 1 of this manuscript. Their condition makes sure that any transnino signal associated with ENSO will not be falsely reflected as a Modoki. By not applying this persistence constraint, and by simply using EMI DJF values, the authors artificially show more Modoki events during pre-1950s. The same lack of application of persistence in central tropical pacific while using the Hendon definition artificially increases the Modoki/CP events numbers during that period, while in reality the signal that is shown in most of the cases is a transnino signal associated with canonical EN-

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SOs. 3. Page 982, lines 14-16 (“..In this method... December”): The expression for the definition is not clear. For example, how long should the signal be in the NINO4 region before reaching NINO1+2 region? Does the April-December indicate the total time of residency in both the regions, or only in the NINO1+2 region? Further, the definition can also apply to canonical ENSO evolution. This is reflected in cases such as 1914/15 shown in Fig. 4. The authors have to consider ruling out any such events that are also associated with a concurrently strong NINO3 value, particularly during the pre-1970s cases, as they may be capturing only the transnino events. 4. Page 982, lines 10-12 (“..the present study... evolution in different regions of the pacific”): The EMI has incorporated the philosophy of capturing the time evolution in different regions already, as described in the earlier comment. Thus, the constraint/concept is not novel. The definition of the CP events, coined by the authors [“In this method a CP event is said to occur if SSTAs exceeding the 0.5 C criterion, occur in the Niño4 region before reached in the Niño-1+2 region for the period of development (April–December)” can also apply to canonical ENSO evolution. How do the authors distinguish the EP events from the CP events? 5. Definition of Ren and Jin index (Table 1): The authors have cited a ‘Ren and Jin 2011’ index, but the corresponding reference was not provided. If the authors were referring to Ren and Jin GRL 2011, then the index they have defined is different from the current one used by the authors. 6. The idea of occurrence of the CP events and/or their impacts prior to 1970s is not entirely new. This been explored in several recent papers (e.g. Kumar et al. Science 2006, Cai & Cowan GRL 2009; Chen and Tam; GRL 2010; Pradhan et al JGR2011 ; Ren & Jin GRL 2011). Notwithstanding these studies, a serious limitation of identification of the events prior to the pre-satellite era is the sparse observations that have gone into the SST reanalysis datasets. This raises a flag when the datasets are used to distinguish between the flavors of ENSO. Another issue is the strong association between the two leading modes of the tropical Pacific SSTA prior to 1970s (Trenberth and Stepaniak, 2001; Ashok et al 2007; Ren and Jin 2011). 7. Fig. 4: The spatial distribution of the SSTA during some of the “CP events”, as identified by the proposed index, does not appear like a CP event evolu-

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tion. For example, 1941/15, and 1963/64 events display predominantly a canonical El Niño signature. The SSTA during the JJA & SON panels of the 2002/03 look like a La Niña. Even the 1968/69 case looks like a La Niña. Ruling out these cases reduces the claimed number of the CP events as well as that which may have occurred during the of pre-1970s period. 8. Lines 19-20, page 984: The statement (“It is interesting to note the lack of skill in the EMI index...”) is very misleading. Ashok et al. (2007) have, based on the EMI index, already identified 1986/87, 1991/92, 2002/03, and 2004/05, which overlap with those by the authors for the post-1990 period, in contradiction to their claim. 9. Line 18-19: The use of the “new phenomenon” terminology by Ashok et al. (2007) is based on the 1958-2005 data, actually refers to recording of a hitherto undocumented type of phenomenon during 1950s to 1970s and occurring with increasing frequency since late 1970s. I guess that the work study has not ruled out the possibility of occurrence of the CP/Modoki events in the earlier period. Also, a paper by the same authors (Ashok and Yamagata 2009) states that Modoki events can also occur due to natural causes. 10. Lines 4-5 Page 987: On a related issue to the above, 1994/95 event has been well documented as an El Niño Modoki/CP event (e.g. Ashok et al., 2007; Kug et al. 2009; Yu and Kao 2009; Ren and Jin 2011) using various indices. The event is not a canonical La Niña (see the differences in the central tropical Pacific in all the three panels for 1994/95 case and, say, that for the 2001/2002 case from Fig. 4). Even during the boreal winter (1994December through February 1995), the maximum warming was stationed only in the central tropical Pacific. 11. Lines 25-26, Page 986: I do not remember earlier studies classifying 2001/2002 as a CP/Modoki event, definitely not the EMI. The statement that EMI identifies the event falsely is incorrect. The confusion arises here because the authors miss the time-evolving aspect of the Modoki definition i.e. need for the EMI to be above the threshold for through a consecutive boreal summer & successive winter, i.e. the event should persist through at least 3 seasons. Using a single season’s value alone to identify the events based on EMI will result in capturing just a transient signal associated with canonical ENSO, or that associated with an intraseasonal signal. 12. Fig. 1: The Modoki/CP events

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have a dominant interannual periodicity. While filtering out the signals above 10-year frequency leaves us with decadal variations, the residue is not a suitable measure to capture the interannual variability. 13. Interestingly, the manuscript touches upon the potential association of the volcanic eruptions with the occurrence of CP events, a potentially interesting and useful issue, but does not explore these in detail. If the authors can establish this issue, if necessary by conducting some GCM sensitivity experiments, this would provide a basis to an argument that the tropical pacific variability can be modulated by atmospheric aerosols. Technical/minor comments: 1. Page 980, Line 22: The “new” events have been first identified by Ashok et al. (2007). 2. Page 981, line 7: Is it a 3-month or a 5-month running average?

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