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**An Open Letter in Response to Zeev Maoz and Errol Henderson’s *Scriptures, Shrines, Scapegoats, and World Politics: Religious Sources of Conflict and Cooperation in the Modern Era*.**

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I write this letter in response to one aspect of Zeev Maoz and Errol Henderson’s critique of my work in their recent book. I specifically argue their use of my data to criticize my own work has a basic and glaring methodological flaw which involves using data which they claim is part of my Religion and State (RAS) dataset but, in fact, is not and was never part of the RAS dataset. I further argue that this flaw in their use of my data, as well as some other flaws I identify, is so problematic that it calls into question not only their critique of my work but nearly every analysis performed in their book. I also demonstrate that their core methodological error was easy to avoid and reveals, at the very least, a level of carelessness that is far beyond what should be acceptable in any publication much less one by two senior scholars. I provide my evidence for this below.

**Some General Context**

But first some general context. In their recent book with University of Michigan Press, *Scriptures, Shrines, Scapegoats, and World Politics: Religious Sources of Conflict and Cooperation in the Modern Era*, (a PDF of the book can be downloaded for free at <https://www.fulcrum.org/concern/monographs/dr26z034m>) Zeev Maoz and Errol Henderson criticize much of my previous work as well as the majority of scholarship over the past two decades by those studying religion and conflict as well as religion in international relations. For example on page 61 in the concluding paragraph of their literature review chapter they state referring to “the theoretical literature on religion and world politics”:

we find that much of it is not very useful in terms of usable knowledge. Despite the claim of some authors on the progress made in the understanding of the relationship between religion and politics, we argue that the theoretical foundations of these linkages are quite weak.

Similarly on page 35 they state regarding chapter 2, their literature review chapter:

Most of the studies reviewed in this chapter make a key substantive argument: religious factors have a profound impact on world politics. What that impact is and how it works are less clearly specified, however. Most importantly, as we argue below, very few of these studies offer a coherent theory of religion and international relations....Little systematic quantitative evidence exists, and that which does suffers from significant methodological problems. And when we find examples of quantitative “tests” of such hypotheses, these often are based on truncated and biased samples, and/or on tenuous empirical assumptions.



They, unsurprisingly, say their book does not have these flaws. For example, on page VI of their preface they state "the scope of our empirical analyses is considerably wider, and methodologically more innovative than most studies on the subject. This makes our results much more robust than previous investigations."

There are many similar quotes I could have taken from the book, but these are sufficient to represent Maoz and Henderson's claims that an entire literature has essentially nothing to contribute theoretically and is methodologically flawed in its entirety. To be clear they are not critiquing me alone. This is a general statement they make about the entire literature. They propose what they consider to be a novel theory on religion and IR which they apply through their empirical chapters.

While, I strongly disagree with their characterization of the literature and of my work in general, they are entitled to their opinion. I will leave it to you to read their review of the literature and their theory and decide for yourself on that issue. In this essay I simply critique the portion of their analysis which uses an inaccurate copy of the dataset I created, though I argue my critiques of this aspect of the book call into question fundamental elements of their study.

### **Maoz and Henderson's Flawed Analysis of the Religion and State Round 2 (RAS2) Dataset**

I wish to critique specifically an analysis they performed using my Religion and State dataset (RAS) which they claim contradicts my previous work. If you accept their analysis without question, on its face it does contradict some of my previous findings. However, I demonstrate here that Maoz and Henderson's analysis is critically flawed. As you will see below, my critique of this analysis not only calls into question their conclusions regarding my findings, but it reveals basic errors which call into question all findings presented in the book.

On pages 55 to 56 they analyze data they *claim* to be from my RAS dataset to critique one of my findings. The finding they critique is that religious legislation, religious discrimination against minorities, and government regulation of the majority religion have been consistently rising since 1990. They use what they say is round 2 of RAS (RAS2) to challenge this claim. In table 2.1 on page 56 they provide results which finds, using the year 1989 as a baseline, that this is not the case for two of three dependent variables both they and I analyze.

Below I outline my critique of this analysis. I use subheadings for my eight core critiques but this discussion includes several other critiques which while accurate, are in my estimation less central to my main argument: that Maoz and Henderson's analysis of my data is fatally flawed and demonstrates at the very least a lack of rigor that I would characterize as careless. Furthermore this flaw likely also taints the rest of their book.

### **Core Critique 1: The 1989 data they use was never part of the RAS dataset**



To be clear, both the text and the table in Maoz and Henderson’s book on pages 55 and 56 clearly state 1989 is the baseline year for their analysis. Yet I have never collected data from 1989 as part of the RAS dataset. All versions of the RAS dataset begin in 1990. Using data from 1989 from my RAS dataset should be impossible unless the data was somehow manipulated or extrapolated. There is no indication anywhere in the book that such a manipulation or extrapolation had been done and if it had been done I would expect this to be explicitly documented in the book which, again, it is not. Rather, in multiple parts of the book they claim that the 1989 data is part of the original RAS2 dataset. This claim is absolutely incorrect.

I, therefore, sent Maoz and Henderson an e-mail asking where the 1989 data had come from. Zeev Maoz in an e-mail sent to me on April 7 2020 answered “We used the attached file from your old website. It covers the years 1989-2008. File is attached in .xls format.”

I will begin by reiterating that the RAS dataset has never at any time included data from before 1990. However the file they sent me which Zeev Maoz specifically claims in his August 7 e-mail which I quote above was from my website has data from 1989. I can attest that this specific file was never posted on the RAS website and I am sure of this for at least five reasons. (1) It does not match the file currently archived for RAS2 on the website which has been the same file that was posted there for many years. (2) This file is not even in the same format as the RAS data files posted on my website. It is in country-year format with each country being a separate case for each year. Thus a country present in all years covered would be 19 cases (in rows), one row for each year between 1990 and 2008. My data has always been posted in country format with all years included in the same row. Thus each country is included with all of its data across the years in a single row. Each variable which was collected for multiple years (which is most of them) would have a year at the end of the variable name. So for example the MX variable would have 19 versions, MX1990, MX 1991, etc. (3) For posterity’s sake, I used <https://web.archive.org> which archives pages on the web at various points in time and spot-checked various past dates. In none of those pages is a file in country-year format available for download. (4) Many of the RAS2 variables are not in this file and several of those that are have variable labels which are different from those the RAS project uses. (5) Again, I never collected data for 1989 and would never have allowed such data to be posted under my name.

Given this, I can only conclude that while the data in the file was derived from RAS2, it was converted into a new format by someone else, presumably the books' authors, and during this conversion critical mistakes occurred. I outline below those mistakes I identify which I believe undermined their analysis.

The first critical mistake, as I note above, is that their analysis includes data for a year that was never collected by the RAS project. As 1989 is the baseline year for their analysis—the year for which they determine whether various variables are increasing or decreasing—that no such data had ever been part of the RAS dataset is a significant problem. Based on my reading of their table 2.1, if 1989 had not been included in their



analysis, which would make 1990 the baseline (as it was in my analysis), their results would likely have not contradicted my original findings.

However, this file revealed some even more serious issues. Until I examined this file I had assumed that the 1989 data must have been extrapolated backward from 1990. As I discuss below in more detail, some (but not all) of it was. Given the changes in the former Soviet bloc between 1989 and 1990, this extrapolation from 1990 to 1989 is quite problematic. However, the file they sent me revealed an even more fundamental set of errors beyond a problematic extrapolation.

**Core Critique 2: The 1989 data includes countries which did not exist as independent states in 1989**

When examining the file to see exactly of what the 1989 data consisted, I found an even more serious problem than data which was never collected by the RAS project. A number of countries which did not exist in 1990 were included with data in the dataset. That is they were listed as present in 1989 and data was coded for all relevant variables for that year. These include Armenia, Azerbaijan, Bosnia, Belarus, Croatia, Eritrea, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Macedonia, Moldova, Montenegro, Slovakia, Slovenia, Tajikistan, Timor, Turkmenistan, Ukraine, and Uzbekistan. All of these countries became independent after 1990, the majority of them in 1991 but several such as Timor and Montenegro became independent considerably later. This makes the data both doubly impossible and exponentially problematic. Improperly extrapolating backward is in and of itself problematic, but data for countries that do not yet exist is far worse.

So apparently, in their analysis of my data using 1989 as a baseline, countries which did not exist in that year as independent states were part of that baseline. In fact, by my count they are about 12% of the cases in that baseline. The presence of this data is even more glaringly obvious as in all of the cases I note above the data for 1990 through the year they became independent was blank in the file Zeev Maoz sent to me which he explicitly states in his e-mail is the source for the data used in the book. A simple eyeballing of the data would have revealed this.

This also accounts for Maoz and Henderson’s incorrect claim on page 55 that I did not account for the increasing number of states in the international system. That is as their baseline year of 1989 included 21 states which were later added to the international system, the flawed version of the data did not show a significant rise in the number of states which they claim is lacking in my data. Yet this lack of a rise is precisely because they included these not-yet-independent states in their baseline year. Put differently their flawed version of the RAS data did not show the Ukraine being added to the international system in 1991, for example, because they erroneously included in 1989.

**Core Critique 3: The 1989 data appears to have been improperly extrapolated from later years**



In addition, while I did not check all states, for all states that I did check which were included in RAS for 1990 (and all of those listed above were not) the data for 1989 and 1990 was identical. That is, it seems to have been extrapolated backward by simply copying the data from 1990 to 1989. I checked states such as Poland and Russia where there should have been significant changes due to changes in government and regime with the fall of the USSR and Soviet Bloc. Thus, the data for many states which did exist in 1989 is likely highly inaccurate for that year. I cannot say exactly how inaccurate because the RAS project has never researched states previous to 1990 but it is fair to assume that religion policies from 1989 and 1990 were very different in states such as Russia and Poland, for example. Thus, even if the data from those states which were not yet independent in 1989 was somehow excluded from the analysis (and I have no indication this was the case) there are still significant problems with this analysis.

**Core Critique 4: Maoz and Henderson did not accurately count how many states were included in RAS for most years**

On page 55 Maoz and Henderson state regarding the RAS2 data that “These data show a change in the number of states in his sample from 173 to 174 over the same period.” They explicitly use the number of states in any given year as part of their analysis to compute change in the RAS2 variables over time. Yet RAS2 includes 177 states, not 173 or 174. Furthermore, in many of the years covered the number of states was much lower. For example in 1990 RAS2 included data on 156 countries since, as noted above, many of the states included in RAS had not yet become independent. This likely considerably threw off their calculations which, in some manner not adequately explained in the book, take into account the number of states in the international system in any given year. This also indicates that they somehow dropped at least three states that were included in RAS.

Also, their claim that I didn’t take the number of states into account in my analysis is odd in that the test to which they refer took a mean for each year and measured change over time. As one may recall a mean is calculated by adding all cases then dividing by the number of cases. As my analysis did exactly this and included only those countries present in the world system in any given year, I do not understand their claim that I did not control for the number of states in the world system.

Also, I am not exactly sure how they took the number of states in the system into account in their analysis. This was done, according to their book, using a “relative rate” which they say is more methodologically appropriate than the “absolute rate” which they claim is how I calculated change over time. However, in the book there is no documentation whatsoever on how this “relative rate” was calculated so I am unable to fully evaluate why the results they present are so radically different from my original analysis of the dataset. This is one of many examples of a lack of proper documentation of procedures in the book.

**Core Critique 5: There are indications that post-1989 Maoz and Henderson version of the RAS data may also have been improperly extrapolated**



All of the above leads me to question whether data for states not yet independent was also included in their analysis also after 1989. That is, if for all years between 1989 and 2008 in their analysis of my data 173-174 states were included and, as I indicated, in some years there were as few as 156 states coded for the RAS data, where did the data for the states RAS never coded in that year come from? I can only assume it was extrapolation. Again, in nearly all cases, this extrapolation would not be to fill in data for a country where no data exists, it would be to add in data for a country before that country came into existence.

To be clear, the file that Zeev Maoz sent me was the file he said he used as the source for all RAS data in the book's analysis. I do not have the file which includes all of the data which was used in the book. Therefore in some cases I am making educated guesses as to what might have been done with my data after it was integrated into the master data file based on the information in the book and Zeev Maoz's e-mail communication. In the file sent to me by Maoz, other than for 1989, the years in which a country was not coded (usually due to that it did not yet exist) were blank.

Therefore, I cannot be sure whether this post-1989 extrapolation occurred. However, from the information provided in the book and the file sent to me by Zeev Maoz, I argue based on inference that it is likely that this error is present in the data they integrated into their combined dataset which they then used for analysis.

Nevertheless, if this error was not made and was limited only to a mistake in the number of states included in the dataset for certain years (and that may well be the case) there are still two issues. First, as the number of states in the system seems to have been included as a separate variable in their "relative" analysis, this number is certainly inaccurate for at least some of the years. Second, it does not undermine my other critiques and it would certainly mean that Maoz and Henderson's documentation of their analysis in this portion of the book is at the very least inadequate and almost certainly inaccurate.

**Core Critique 6: All of the above call into question the integrity of all aspects of the book's data analysis**

I would argue all of this undermines confidence in their analysis of my data to the extent that their analysis is at the very least unreliable. I can only conjecture that when converting my RAS2 data from its original format, errors were made. As the presence of major errors indicate the possibility of other errors, one might ask what other errors were made? A cursory look at the file sent to me by Maoz did not reveal any glaring errors other than those listed above, but (1) the inclusion of data on at least 21 countries in years that they did not exist as independent countries in 1989 as well as 1990 data being used for 1989 in the former Soviet bloc are sufficiently problematic to call this analysis into question and (2) that such a basic fundamental and easy to catch (see below) errors could have been made demonstrates an unacceptable lack of rigorousness in how their dataset was constructed that is so significant it would not be inappropriate to characterize it as carelessness.



This truly begs the question of what other errors might be present in the book and other elements of the dataset? That is, errors of this magnitude in one part of this study in my eyes undermine the presumption that other parts of the study were done correctly.

I have neither the time nor inclination to review all of their methodology throughout the book (and to do so I'd need a full copy of their data) so I cannot answer this question. However, given their multiple and repeating claims throughout the book that (a) most previous studies made major methodological errors and (b) that their work has methodological superiority, (see quotes earlier in this essay) I would expect at the very least the absence of such basic mistakes which profoundly undermine at least one of their claims and possibly all of their analyses in the book (as I discuss below). That is, while I'd argue this is a serious set of errors by any standard, given their claims of methodological superiority, I would posit that this certainly violates the methodological standards they explicitly and repeatedly apply to others in their book.

**Core Critique 7: All other analyses in the book are likely flawed due to their use of flawed RAS data**

However, the implications of these mistakes may be considerably broader. This is because they use data from RAS2 for nearly every analysis on the book. I am unclear on whether or not the 1989 data was used in the analyses other than their evaluation of my past work. This is because on page 169 when describing the data they use in the book's main analyses they state that the RAS2 dataset which they use in their analyses "covers most states during the period 1988–2008." (Maoz in his e-mail states this is a typo and it should be 1989-2008). However in the table on page 195 they state they use data from RAS2 from 1990-2010.

Even if the data used for the analyses in the book other than the one on pages 55 and 56 was only from 1990 onward, the possibility that parts of the post-1990 data includes improperly extrapolated data including data for states which did not yet exist still remains.

I would need to see the files used for the actual analysis to be sure whether any of these errors occurred. The poor and sometimes contradictory documentation in the book does not provide me enough information to be certain one way or another.

**Core Critique 8: Poor documentation**

I have little to add here beyond what I say above but as I highlight at several points, there are substantial gaps in the documentation of data and procedures provided by Maoz and Henderson in their book. A necessary aspect of quantitative academic work is transparency. I usually define transparency as sufficient information that another researcher could replicate the work. As I document repeatedly above, this book fails to meet this standard.

The book refers in multiple places to the book's website as an additional source of information but provides no URL. The book's page at the University of Michigan Press



website does have an online appendix for the book but it contains nothing but several detailed analysis tables and nothing that answers any of the questions I pose here. ([https://www.press.umich.edu/11353856/scriptures\\_shrines\\_scapegoats\\_and\\_world\\_politics](https://www.press.umich.edu/11353856/scriptures_shrines_scapegoats_and_world_politics), accessed April 23, 2020)

Because of this poor documentation, as I note repeatedly above, some of my critiques are based on educated guesses on what may have been done with the data. Thus, while the possibility that some of these critiques do not apply is real, this does not apply to all of them. Also, if any of the critiques are, in fact, off base, the authors could and should have avoided them by providing proper documentation of their procedures. I imagine that they might provide this type of documentation after the fact in response to this open letter. However, proper documentation on matters so basic should have been included in the book.

**A final quibble which while serious is less serious than those above**

As RAS2 has data only through 2008, I asked Maoz and Henderson where the data from 2009-2010 came from. Maoz, in his e-mail explains the data from 2009-2010 was extrapolated, from earlier years, though I could find no mention of this in the book. I cannot evaluate the accuracy of this extrapolated data since it was not included in the file Maoz sent to me. Also, I would expect when a dataset is modified for use in a study that this would be documented in the study. I could find no such documentation in the book and received no response from Maoz and Henderson when I asked where such documentation could be found in the book. As a side note I also outlined many of the core issues in this e-mail in the same e-mail I sent to both Maoz and Henderson which was in response to Zeev Maoz’s April 7 e-mail. I received no response.

Also, Maoz does not fully explain why the years in the text and in the table do not match up though, to be fair, this could be no more than an editing error combined with the typo Moaz admits to in his April 7 e-mail. But all of this is, in my view, an additional aspect of the larger pattern of carelessness I uncovered in my examination of this study.

Finally, this demonstrates that there were at least some manipulations of the RAS data after it had been integrated into their master dataset for the book which were not documented in the book or anywhere other than the April 7 e-mail from Maoz after I asked specifically where the additional data had come from. This lends credence to my belief which I discuss in more detail above that there were other manipulations of the data that were similarly not documented in the book.

**These Errors Were Easily Avoidable**

The error of including 1989 data which was never part of the RAS dataset in their analyses (including data for 21 not-yet existing countries) was entirely avoidable for at least three reasons. (1) All of the publications I have ever written using the RAS dataset explicitly include the years covered and in no RAS-based publication have I ever discussed data from 1989. In all of them I discuss the timespan covered and explicitly state the data begins in 1990. This includes all iterations of the project’s codebook which





is the basic source for describing the data as well as the project’s website. My publications are cited multiple times in the book, including in the citations in places where they claim my data begins in 1989 (or 1988 based on the “typo” on page 169). Thus, Maoz and Henderson are clearly aware of these publications and should have noticed the discrepancy between the years included in the data they used as compared to the years listed in every single one of these publications. (2) I am aware of no publication by anyone who has used RAS data from before 1990. Thus, none of the large and growing number of scholars who have use RAS have made this error. This book, in this respect, is truly a first. (3) As I note above even a cursory look at the data file would have revealed this error. There should be no data for countries which do not exist as independent states in a given year.

To sum up, in order to make this error, Maoz and Henderson would have needed to both entirely miss, forget, or ignore every piece of documentation that to my knowledge ever existed on the RAS data and fail to properly examine their data file for errors. This demonstrates what I would characterize as extreme carelessness. I find this particularly ironic in a book which repeatedly claims an entire genera of studies is plagued by methodological errors and that it is methodologically superior to all others.

**Conclusions: Fundamentally Flawed and Careless**

Given all of the above, as I state at the outset of this letter, it is my position that Maoz and Henderson’s analysis of my data is so fundamentally flawed that it should be disregarded. These errors are errors that should never be present in a professional and rigorous analysis. As I note above, I do not have sufficient information to determine whether these flaws also taint the other analyses in the book. However, I did find a larger pattern which includes (1) flawed data and (2) inadequate and contradictory documentation of their data sources and procedures in the book. The latter includes important manipulations of the data that were not documented at all in the book. Also, at least some of this often contradictory documentation suggests pre-1990 RAS data and improperly extrapolated data from 1990 onward was used in all of the book’s analyses. Given this, it is certainly possible and even likely, that they used their flawed version of the RAS data in nearly all statistical tests in the book. It is also quite possible that there are elements of this larger pattern of carelessness in other aspects of their data, procedures, and analyses that I have not uncovered. Given the errors and carelessness in multiple parts of the book that I do document above, I believe the onus is on Maoz and Henderson to prove that all analyses in their book are not tainted by these errors.

Also, it is certainly true that significant aspects of Maoz and Henderson’s research design are insufficiently documented. While I understand that documentation errors and oversights can happen, I would expect that authors who in multiple places in their book claim that nearly all previous works by scholars in the field of religion and world politics have significant methodological errors (and are theoretically flawed to boot) would be extra careful to provide a full, transparent, meticulous, and accurate description of their methodology. As I demonstrate above, this is clearly not the case. In fact, as I discuss above, it is my position that they fail spectacularly in accomplishing this task when using my RAS dataset. Their documentation does not meet the standards



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they explicitly set for others in their book and I argue that their documentation failings are so severe that they would be hard put to demonstrate that their documentation meets even the minimum professional standards in the field.

Based on this I would suggest that scholars more familiar than me with the other (non-RAS) data components used in their analysis examine those components closely. I would hope that their errors in using the RAS data are the only ones in the book's analyses. However as I note above, errors of this nature in one portion of a study, beg the question of whether there are errors in other portions. Maoz and Henderson have, in my assessment, proven themselves capable of publishing an analysis which includes basic errors which can profoundly skew an analysis to the extent that I would be reluctant to take any aspect of this book's data analysis as reliable until it had been rigorously checked by a reliable third party.

I would conclude by saying that I welcome any scholar using my data for any scholarly purpose. I actively facilitate this by making the data available on the RAS website ([www.religionandstate.org](http://www.religionandstate.org)). This use of the data specifically includes replicating, evaluating, and critiquing my own work. Over 100 publications which use RAS were authored by people other than myself and the data has been downloaded over 12,000 times.

This is the first time I have ever written and posted such a letter. If all Maoz and Henderson had done was critique my work but omitted the analysis on pages 55 and 56 of their book, I would have never written this essay. (Their critiques of my work can be found throughout the book, not just on those two pages.) If their analysis had been done properly, I also would not have written this essay. It was very specifically the obvious and glaring errors in their analysis and their misrepresentation of the content of the RAS2 dataset on pages 55 and 56 that spurred me to write this essay.

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