

Response to Jonathan Fox's Accusations

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Dear All,

In one form or another you have received Jonathan Fox's letter accusing us of misusing (abusing) his religion and state data, because "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."

Fox's entire argument is based on the use of 1989 data which his dataset doesn't cover (he correctly states his data starts at 1990 and (in the version we had used for the book) ended in 2008). He argues that (a) we have incorrectly used his data, setting 1989 as the base year, (b) we converted (or manufactured a new dataset) from his data which employs countries that he did not have data for, (c) used a wrong baseline for his data in our time-series, and (d) incorrectly extrapolated data through 2010. Accordingly, he claims that most of our empirical analyses, and hence our substantive inferences are "Fundamentally Flawed and Careless." Of course, Fox does not provide any data to substantiate his arguments, nor does he provide evidence of anything else beyond these claims.

Let me respond to these accusations, but rather than to engage in an endless debate, I will try to support my argument with some data. But first, an explanation of how we used 1989 as a base year and how we interpolated the data through 2010.

Fox's RAS old dataset (covering the period of 1990-2008) is structured in a country-record format. Each record has a country's name and a long list of variables coding various types of indicators concerning aspects of religious discrimination, religious regulation, and religious legislation. Each record also contains three variables that combine the individual indices into composite indices of religious discrimination, regulation, and legislation. These are the variables we used. For the year 1990 he has two distinct composites of each variable (MX1990 and MX1990X, NX1990 and NX1990X, and LX 1990 and LX1990X).

The variables with 1990 are labeled "Composite measure of religious discrimination against minority religions, 1990 (higher scores indicate higher levels of discrimination)",

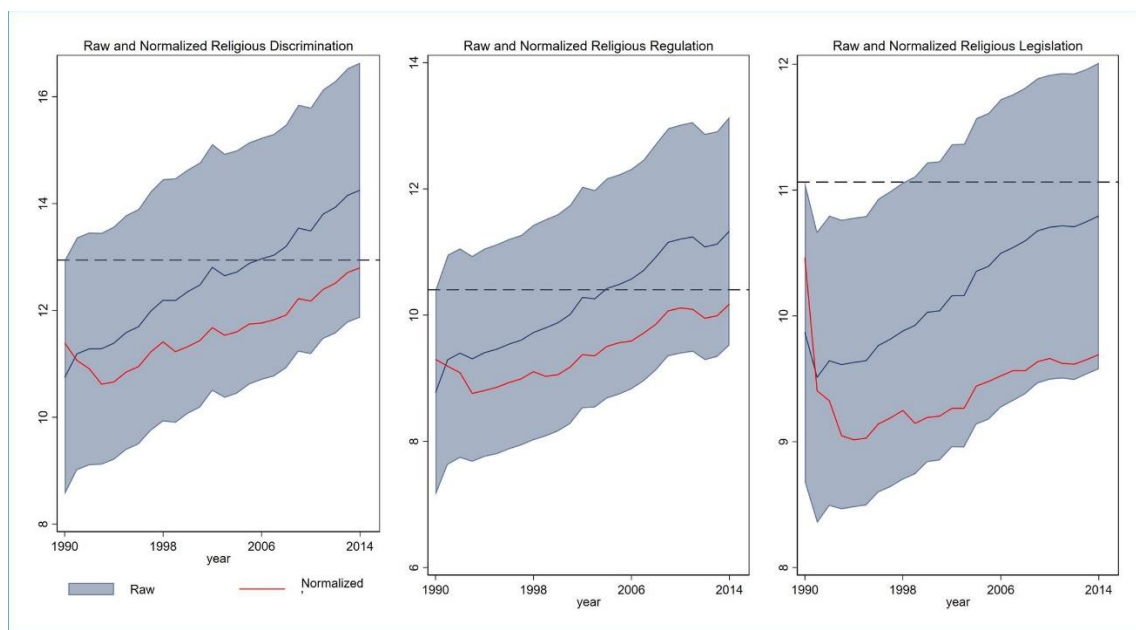
The variables ending with 1990X are labeled "Composite measure of religious discrimination against minority religions, 1990 **or earliest** (higher scores indicate higher levels of discrimination)" [emphasis mine ZM]

(source <http://www.religionandstate.org/> go to the "downloads" tab and click the "codebook" link.)

We did make a mistake, and the mistake is mine. I treated the 1990X variables as equivalent to 1989 based on the “or earliest” statement. Indeed a grievous error. We also used a method called multiple imputations to interpolate the data through 2010, which our other data covered. Did we impute incorrectly? To answer these questions I will show two things. First, Fox claims “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.” Ironically, Table 2.1 in our book (*Scriptures*, in short), pp. 51-52 does not involve any data that Fox has created or provided us. Rather, this table analyzes data by Pearce (Pearce, S. 2006. “Religious Rage: A Quantitative Analysis of the Intensity of Religious Conflict.” In *Religion in World Conflict*, edited by J. Fox and S. Sandler, 39–58. New York: Routledge.). Good reading, Fox.

What Fox might be referring to are the three parts of Figure 2.1 p. 56 and the discussion on pp. 55-6, which show Fox’s three composite indicators over the (interpolated) period of 1989-2010. On the basis of this analysis we argued that the hype about the growth of religious-state cohabitation over the period was vastly exaggerated. If Fox was right about the “erroneous” and “avoidable”, then the “real” data would show a different picture, and of course, our inference would be “fundamentally flawed.”

To examine this argument, I now downloaded RAS version 3, which covers the period of 1990 to 2014. I did not interpolate anything either forward or backward, just used the data as they are provided by the RAS website. Here are the results. I’ll explain what I did below.



The shaded area are 95% confidence intervals (where 95% of the observations fall) of the composite variables. The blue line is the mean of the raw distribution. This is the mean of the composite religious discrimination, regulation, and discrimination scores. This mean is obtained by summing, for each variable, all composite scores and dividing them by the number of countries that have valid score in Fox’s data (N = 176. **Note:** I dropped non-state entities such as, Iraqi Kurdistan, Palestinian Authority West Bank, or Gaza). The red line is the same score normalized by the number of countries in the Correlates of War (COW) country list (which varies between 178 and 196). The dashed line enables us to

answer the question of whether levels of religious-state relations have changed significantly over the period (**Also note:** The time period is also longer than in our book—currently going to 2014.) How do we decide? Now, we used 1990 as the base year. The upper confidence interval for this year is where the dashed line starts. If, by 2014, there was a significant increase in religious discrimination, regulation, or legislation, the bottom range of the confidence interval should be higher than the baseline year. Not only is it not the case, but the rate of increase is considerably less steep than our old figures.

What do we conclude? Fox is correct, we did make a mistake. However, the mistake is that we overestimated the degree of change. In fact, we should have been more resolute in our argument, and Fox's own argument is fundamentally flawed. His inferences are even more out of the park than we thought they were.

Fox also argues that we included countries that were not independent in 1989. I am not sure what is Fox's definition of a state. We are using the COW state definition and state list, which is the standard in IR and comparative politics. The COW definition of states in the relevant period is: an entity is considered a state member of the international system if it meets one of the following conditions: (1) It must be a member of the United Nations, or (2) receive diplomatic recognition of two major powers, and (3) control a population of 500,000 or more. (COW makes one exception here and that is Taiwan, which is also in Fox's data.)

This state list does not include the Palestinian Authority, Gaza (I am pretty sure Fox is happy about that, but he did include them in his data), Kurdistan (Iraq) or Greek/Turkish Cyprus. So it is Fox who invents states without any explanation. Nice to include nonstate actors, but if you do that, you want to be (a) explicit about which actors should be included, and (b) exhaustive in terms of your definition. Fox is correct about interpolation of data for countries that had only partial temporal data. But we did this with a great deal of caution which is afforded by the multiple imputation algorithm. (See Blackwell, M., Honaker, J. and King, G., 2017. A unified approach to measurement error and missing data: overview and applications. *Sociological Methods & Research*, 46(3), pp.303-341.)

Fox claims, without any evidence, that these errors cast doubt on our other analyses relying on the RAS data. "All of the above call into question the integrity of all aspects of the book's data analysis." He claims that "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." If he had read the book rather than cherry pick his cites and statements in the book, he would have found that all of our data, methods, measured, and estimation procedures are fully documented, both in the text of the various chapters, and even more so in detailed appendices at the end of each empirical chapter. In fact, 86 pages out of a total of 397 text pages of our book are devoted to design, data, and method specification aspects. Very few books—certainly none of Fox's boos—devote as much space to detailing what they do in terms of empirical analyses, and why they make the data and methods choices they do.

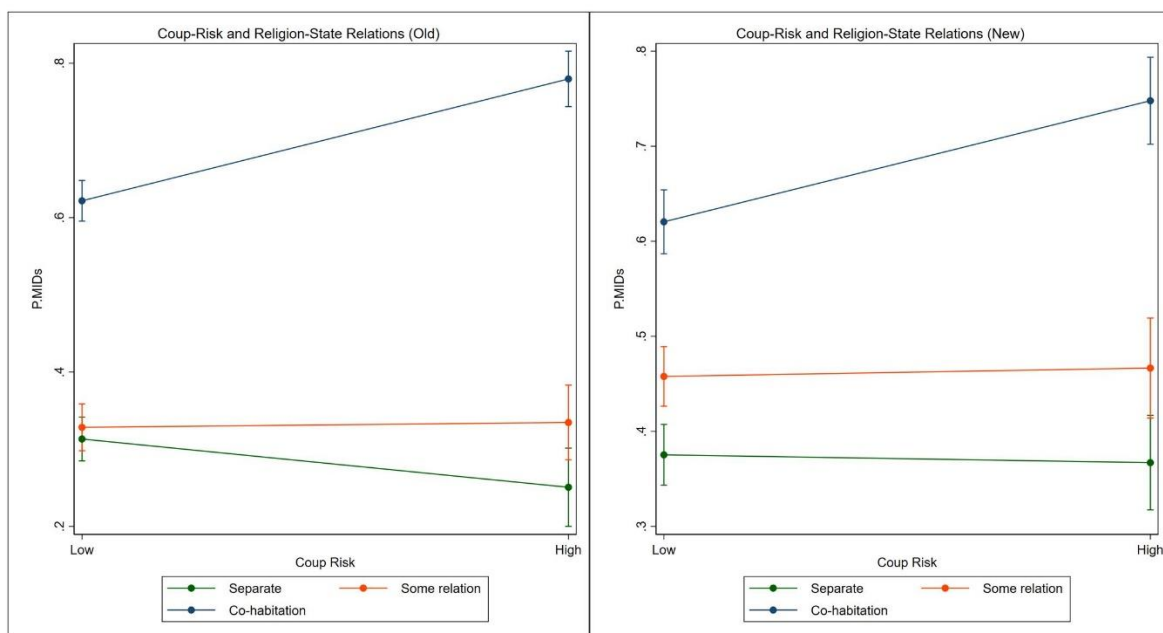
It is true that the book's website is still incomplete. This is due to the fact that the book came out in the midst of the COVID-19 outbreak (in fact, the hard copies of the book sent to me by the UM press are in my department's office, which has been closed for nearly two months), and much of the replication data require cleaning and detailed documentation, all of which are in the works. So Fox (and anyone else for

that matter), will have full opportunity to analyze our data. On the other hand, while the RAS website includes an impressive list of publications associated with the project, there is not a single replication link associated with any of them. Apparently, Fox is fond of the “do what I say, not what I do” practice.

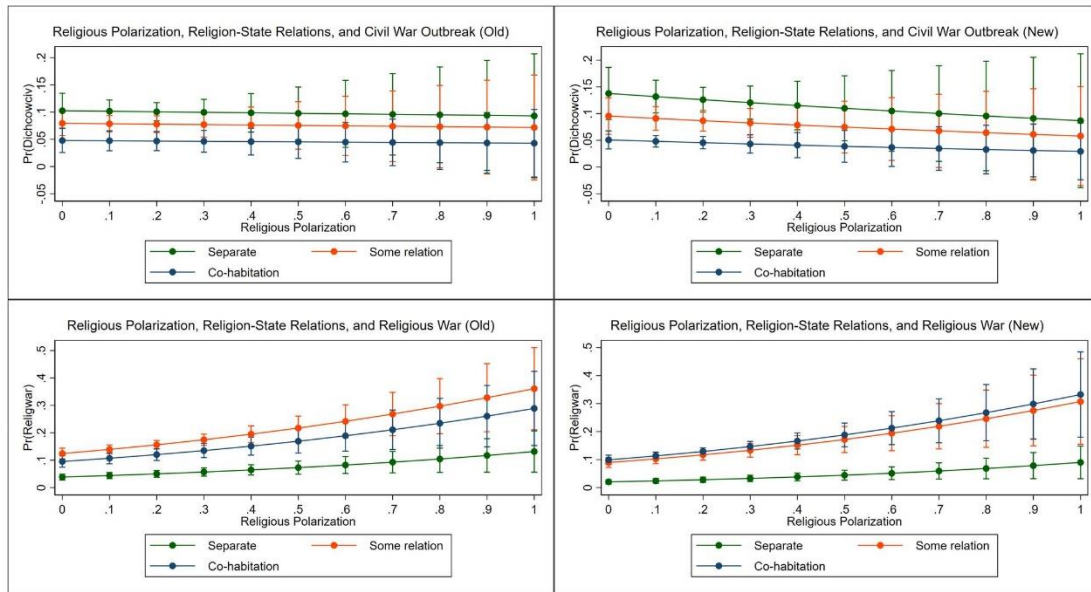
However, in order to test Fox’s arguments about the adverse consequences of “misusing” the RAS data, I re-ran some of the analyses comparing the results based on the data we had used for the book with the new RAS3 data which we did not interpolate and we did not “move” back to 1989. There are dozens of tables and graphs listing results of analyses involving Fox’s data, so I will give only a few for illustration purposes. Obviously, it will take more time to re-run all the analyses, but these results suggest that not much will change.

The first set of graphs shows the interaction between coup risk and religion-state relations (grouped into three levels as discussed in the book p. 170) and coup-risk. Note that the variable we use in the book combines RAS data with the Comparative Constitutions Project. Here we focus strictly on a breakdown (into three levels of the composite religion-state relation variable which sums over all three composite Fox variables). The dependent variable in the graph is the probability of Militarized Interstate Dispute (MID, Maoz et al., *JCR* 2019), and the coup-risk variable is taken from Sudduth (Sudduth, J. K. 2017. “Coup-risk, Coup-Proofing, and Leader Survival.” *Journal of Peace Research* 54, no. 1: 3–15.)

The left panel shows the results based on the old data, the right panel shows the results based on RAS3. Not much of a difference.

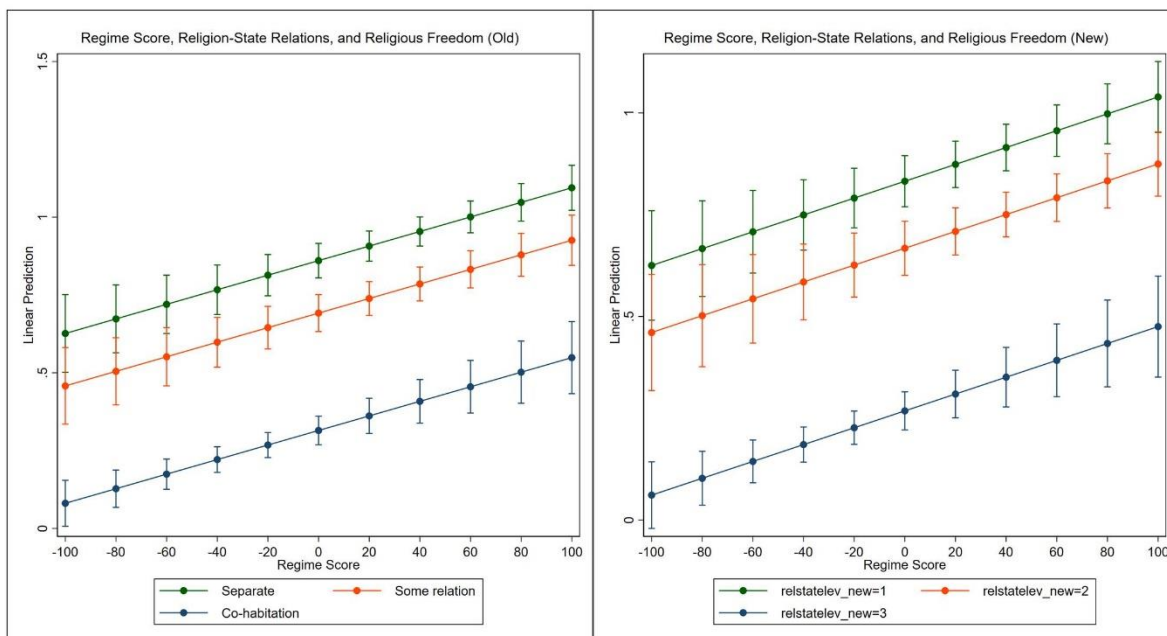


The next set of graphs examines the interaction effect of religious polarization and religion-state relations on the outbreak and type (religious or non-religious) of civil war.



As you can see, the top panel shows that religious-state relations do not exert a significant impact on the outbreak of civil war. By contrast, the bottom panel shows that the probability of a religious civil war outbreak is significantly higher in states which bear some relationship between religious and political institutions, and states in which religious and political institutions tend to co-habitate compared to states that practice separation between religion and politics. The new Fox data show a more significant relationship, especially at high levels of religious polarization, but the story is fundamentally the same.

Finally, we examine the effects of the interaction of regime score and religion-state relations on religious freedom using the US State Department religious freedom index (from the ARDA website, <http://www.thearda.com/Archive/CrossNational.asp>), which we discussed and analyzed in more detail in Chapter 8. Here too, we compare the “old” Fox data which included interpolated data to 2010, and the current Fox dataset (RAS 3).



Here too, despite minor differences—which can be expected given the changes and updates in version 3 of the RAS—we find the results remarkably similar.

The final and most definitive check focuses on the overall match between the data we used in our book and RAS3. This is a more demanding test than comparing our data to RAS2, because we can safely assume that the recent version includes some data cleaning, filling some missing cases, and so forth.

Here are the correlations between the RAS2 data we used in our book and the current RAS (3) version on the three composite variables. The unit of analysis is the nation-year:

		RAS3
Rel. Discrimination (RD)	r	0.979
	N	4,931
Rel. Regulation (RR)	r	0.946
	N	4,932
Rel. Legislation (RL)	r	0.964
	N	4,932
Relig-State Relations (RS) (Combined)	r	0.950
	N	4,994

Note that the correlations are extremely high despite the fact that the ranges of the variables in RAS2 and RAS3 are different ($0 \leq RD2 \leq 71$, $0 \leq RD3 \leq 80$; $0 \leq RR2 \leq 56$, $0 \leq RR3 \leq 57$; $0 \leq RL2 \leq 42$, $0 \leq RL3 \leq 46$; $0 \leq RS2 \leq 144$, $0 \leq RS3 \leq 167$).

Since we collapsed the religion-state relations into three levels, splitting this distribution into three equal parts at the 33 and 67 percentiles (p. 169 of *Scriptures*), we can do the same for the RAS3 data and compare the result to the breakdown of this variable in our book. Here are the results.

RAS3		Separate	Some rela	Co-habita	Missing	Total
<i>Scriptures</i>						
Separate	Actual Freq.	1,174	151	0	79	1,404
	Expected	153.2	160.6	156.2	934.0	1,404.0
Some relation	Actual Freq.	352	1,386	44	105	1,887
	Expected	206.0	215.9	209.9	1,255.3	1,887.0
Co-habitation	Actual Freq.	116	129	1,642	60	1,947
	Expected	212.5	222.7	216.6	1,295.2	1,947.0
Missing	Actual Freq.	185	249	176	10,891	11,501
	Expected	1,255.3	1,315.8	1,279.3	7,650.6	11,501.0
Total	Actual Freq.	1,827	1,915	1,862	11,135	16,739
	Expected	1,827.0	1,915.0	1,862.0	11,135.0	16,739.0

The stats for this table are: Chi-square = 5947.7, Gamma = 0.923; Tau-b = 0.806 (all $p < 0.001$; stats computed for non-missing data only).

This match is all the more remarkable given the changes in the distribution of the composite variable. That is, the baseline for this joint distribution has changed considerably from RAS2 that formed the basis of our data to RAS3. Also, note that we include missing data in the table to account for discrepancies in missing data because Fox argued that we had included states that did not exist in 1989. However, since the joint missing data figure (the missing-missing cell) inflates the relationship between RAS2 and RAS3 (joint missing data cover the period of 1945-1989 that are not included in RAS), the association statistics are calculated only on non-missing data.

These correlations account for the remarkable similarity between our original analyses and replicated analyses based on RAS3, and they clearly suggest that we have actually used the RAS2 data very carefully, and that the outcome of using the YXXX1990X variables as YXXX1989 and the imputations of missing data had virtually no effect on our analyses and inferences.

All in all, Fox's arguments that we mismanaged his data, do not hold water. Fox rushed to criticize without making even minimal effort to support his suspicions by careful analysis.

Over the course of the work on this book, we have spent a great deal of time looking at the literature. What we discovered, from frequent interactions with scholars who do religion and politics, was that---while there are many honest and open-minded scholars in this field--there is a clique of people who talk to each other in a closed circle, magnify the role of religion in politics in general, and in IR in particular, engage in rather vague and unsupported polemics and sloppy empirical analysis. One of the main criticisms we received in the course of working on this project is "you didn't cite me (or you didn't cite my clique member)," which often translates to "you didn't compliment us." It seems that many of the people we criticize--like some of Fox's work--have dogs in the fight, which is understandable given that people had spent their entire career on this topic. But this doesn't mean that our criticism is ill-founded. On the contrary. Rushing to criticize other people's work, as Fox has done in his open letter, without

carefully understanding their arguments or the way they document them theoretically and empirically is counterproductive.

Ironically, had Fox spent some time actually trying to understand our arguments, he would have found that the relationship between religion and state plays an important role in our theory. We argue that the interaction of social structure—defined as the religious homogeneity/diversity of a society—with religion-state relations affects the willingness and ability of political leaders to manipulate religion for the sake of insuring their political survival.

Set out to nitpick and criticize, Fox ignored the fact that his data were very useful in supporting some of these claims. The evidence for our argument is not always supportive, but there is enough evidence to suggest that leaders use religion when they can and when they feel the need to do so. One of the factors that defines both willingness and opportunity is the relationship they build with religious groups and religious institutions. In that sense, we believe that many of the arguments made by Fox and others have merit. The figures above are examples of this fact.

I am not going to go into the manner in which Fox chose to criticize our work. Some of you have to live and work with him; we don't. However, I believe that a scholarly interaction—even a critical one—requires some integrity. Fox accuses us of lack of transparency; unfortunately, he provides little or no evidence to his arguments, and in some cases (e.g., referring to Table 2.1 which has nothing to do with his data, or in (deliberately?) ignoring the careful attention we placed into explaining our methods in the manuscript). I haven't seen any evidence for replication data for any of his books, but he has a problem with a book that has just been published when it comes to replication data.

We are open to criticism, and I have communicated with Fox, but have not had a chance to respond to his recent email. Nor have I had a chance to discuss Fox's queries with my coauthor Errol. Fox chose the public route. That's fine, bring it on. We are happy to continue this dialog, and we are more than anxious to see replications and criticisms of our work which are founded on facts and evidence rather than the kind of rhetoric we have encountered in much of the literature and of which Fox's open letter is a good example.

The venue of the open letter is also puzzling. Typically, if you wish to criticize someone's scholarly work, you do it by trying to publish an article in a peer-review journal rather than to communicate to your departmental colleagues. I am not sure what Fox tried to accomplish here, but whatever it is, this sounds more like Fox News, rather than responsible scholarship. No surprise there either.

Unfortunately, given Fox's choice of venues, we are forced to respond in kind. It would be interesting to see how seriously these accusations are considered if submitted to a respectable peer-reviewed journal. We hope that this dialog moves to more credible venues, and we welcome any additional criticism of our book, which it may deserve. However, we hope that future discussion of our work will be based on careful reading and on substance rather than unsupported claims and baseless accusations, such as the ones we have seen in Fox's open letter.