Response to Charges Concerning the Herdon/Ash/Pollin Replication of Reinhart and Rogoff's "Growth in a Time of Debt"

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The April 2013 preliminary working paper by Thomas Herndon, Michael Ash and myself "Does High Public Debt Consistently Stifle Economic Growth," (HAP 2013A) which was a critical replication of the 2010 paper, "Growth in a Time of Debt" by Professors Carmen Reinhart and Kenneth Rogoff, generated an intense global debate within hours of it being posted online.[1] This reaction took us totally by surprise. But once this global debate began, we were not surprised at all that the reactions to our working paper varied widely among academics, policy analysts, journalists, bloggers, and the public at large. Of course, we took special notice of the responses by Professors Reinhart and Rogoff themselves. Beyond Reinhart and Rogoff's comments, it has been impossible for us to keep track, much less address, the myriad of issues and perspectives advanced in this global debate.

At this point, eight months after we posted our working paper, my co-authors and I have now had the opportunity to move the debate into a more formal scholarly setting. This is because we have now published a revised version of our paper in the *Cambridge Journal of Economics* (HAP 2013B).[2] This revised paper addresses the comments on our working paper by Reinhart and Rogoff (RR).[3] These responses by RR have enabled us to focus our analysis in the *CJE* paper more sharply than was possible in our preliminary working paper. In the interests of achieving further clarity on the important issues at hand, Professors Reinhart and Rogoff (and anyone else for that matter) are now free to send comments on our paper to the editors of the *CJE*. We would especially welcome such a response by Professors Reinhart and Rogoff.

In addition to the purely scholarly issues that we have addressed in our initial working paper and our revised and published *CJE* paper, Reinhart and Rogoff, Rogoff writing by himself, and some others, including Professor Angus Deaton, have raised related matters of concern over

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[1] Our critical replications of "Growth in a Time of Debt" considered both RR's working paper version of the paper (RR 2010A) and their published version (RR 2010B).
[2] To date, our *CJE* paper (HAP 2013B) is available in an Advance Access form from Oxford University Press and at the PERI website <http://www.peri.umass.edu/>. A printed version is forthcoming in the printed journal as well. The Oxford University Press and PERI websites also both include a blog by myself summarizing some of the main findings of the *CJE* paper.
[3] Reinhart and Rogoff have made numerous published response to HAP 2013A and related commentary, including Pollin and Ash (2013A). In this discussion we focus on these responses: Reinhart and Rogoff (2013A, 2013B, 2013C), Rogoff (2013) and Bernau (2013).

the past months that require a response. These matters include questions of professional conduct, political biases, methodology, interpretations of our critical replication exercise, and the implications of our results. It is appropriate to address some of these issues in this separate document, as opposed to our published *CJE* paper. For the most part, they are clearly matters that do not belong in an academic paper. We consider five topics: 1) Professional standards and political biases; 2) Issues with our replication exercise; 3) Research errors and methodological differences; 4) The statistical patterns that emerge from our replication relative to those by RR; and 5) Overall significance of our findings for policy debates.

To provide some context for the present discussion, it will be useful to begin with a summary of the scholarly debate itself. Here is the abstract in full of the published *CJE* paper by HAP:

> We replicate Reinhart and Rogoff (2010a and 2010b) and find that selective exclusion of available data, coding errors, and inappropriate weighting of summary statistics lead to serious miscalculations that inaccurately represent the relationship between public debt and GDP growth among 20 advanced economies. Over 1946 – 2009, countries with public debt/GDP ratios above 90 percent averaged 2.2 percent real GDP growth, not −0.1 percent as published. The published results for 1) median GDP growth rates for the 1946 – 2009 period, and 2) mean and median GDP growth figures over 1790 – 2009 are all distorted by similar methodological errors although the magnitude of the distortions are somewhat smaller than with the mean figures for 1946 - 2009. Contrary to Reinhart and Rogoff's broader contentions, both mean and median GDP growth when public debt levels exceed 90 percent of GDP are not dramatically different than when the public debt/GDP ratios are lower. The relationship between public debt and GDP growth varies significantly by period and country. Our overall evidence refutes RR's claim that public debt/GDP ratios above 90 percent consistently reduce a country's GDP growth (HAP 2013B, p. 1).

1. PROFESSIONAL STANDARDS AND POLITICAL BIASES

1A) Did we engage in unprofessional conduct by sending our working paper to RR at the same time we posted it online and sent it to journalists?

RR begin their 4/25/13 *NY Times* technical article "Responding to Our Critics," (RR 2013B) by writing that "Last week, we were sent a sharply-worded paper by three researchers from the University of Massachusetts Amherst at the same time it was sent to journalists." Rogoff repeats this point in his 10/1/13 "FAQ", writing that "We received their paper on the same day," that we posted the paper online.

In making this charge, RR neglect to mention that on 4/4/13, when Professor Reinhart sent Herndon the working Excel spreadsheet for "Growth in a Time of Debt," she explicitly told

Herndon that he should feel free to publish whatever results he wished and that she would not have time to engage with him on the topic. Reinhart wrote "By all means publish whatever results you have. I cannot speak for my coauthor, but I have my hands full in working with my students and cannot unfortunately engage with all the students who are working in this area and crises in general. I look forward to your contributions in this field."

As to RR's claim that we "sent to journalists" the paper at the same time we sent it to themselves, the fact is that, among all three co-authors, Professor Ash had exactly one conversation with one blogger, Mike Konzcal of the Roosevelt Institute on 4/15/13, the same day that we posted our working paper. That was the full extent of our engagement with anybody in any way connected to the news media prior to our paper becoming a major global news story. It is unclear as to how this one discussion with Mr. Konzcal could be construed as improper or unfair to RR, especially given what Professor Reinhart wrote to Herndon on 4/4/13, as quoted above.

1B) Are our results tainted by political biases?

In his 10/1/13 response "FAQ on Herndon, Ash and Pollin's Critique of 'Growth in a Time of Debt," Rogoff claims that we infer that his research is politically motivated (Rogoff 2013). However, he does not cite any passage in our working paper, or any subsequent commentary by any of us, to support this claim. Further, the German newspaper *Frankfurter Allgemeine* published an interview on 10/22/13 with Rogoff (Bernau 2013). This story quotes Rogoff as claiming, among other charges, that he has been the victim of an "unyielding personal attack, from people with a strong political agenda." He also charged that his opponents had "distorted and consciously falsely interpreted and polemicized his work."[4] However, again, to the extent that Rogoff is referring to Herndon, Ash and myself as his critics, he offers no evidence in this interview, as published, as to where we may have distorted or consciously provided false interpretations of his results.

In his FAQ memo, Rogoff does however provide some brief comments that appear to be his effort to support his claim of political bias on our part. Rogoff writes as follows: "Is our research politically motivated, as HAP infer? No, we are centrists, our academic research has always been completely apolitical. See, however, Ash's similar claim of a coding error in Deaton and Lubotsky's study of inequality and mortality, and Deaton's and Lubotsky's response. Deaton now weighs in on uncanny similarities in two critiques. See also the Sander-Pollin debate."

What Rogoff is referring to here are two completely separate research projects, one conducted by Michael Ash and Dean Robinson in 2009 and the other by Robert Pollin and Mark Brenner in 2000.

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[4] The original interview in German can be found here: http://blogs.faz.net/fazit/2013/10/22/kenneth-rogoff-ueber-excel-fehler-hexenjagd-2818/. The quotations are English translations from the German original.

The Ash/Robinson 2009 study. The Ash/Robinson paper referred to by Rogoff was a critical replication of a 2003 paper by Deaton and Lubotsky (full citations in reference section). Rogoff does not explain why he regards this 2009 critical replication, which was refereed and published in a professional journal, along with a response by Deaton/Lubotsky (2009), as having any bearing on the substance of our current critique of RR or on his charge that we claimed his work was politically motivated. However, Angus Deaton did also weigh in on this issue, as noted by Rogoff. Deaton wrote a blog post on 10/9/13 titled "On Weights and Coding Errors: Odd Coincidence or Dress Rehearsal?" (Deaton 2013). In Deaton's blog post, he suggests that the Ash/Robinson critique of his work is without merit and, further, that Ash and Robinson had engaged in unprofessional conduct. Deaton writes, "If you want to debunk a paper, working through it equation by equation, trying out a range of weights, you will eventually find something that changes. You can then cry 'coding error' and hope that the rhetoric shifts the burden of proof back to the original authors."

However, Deaton does not address the substance of the results Ash/Robinson presented in their published 2009 paper. Nor did he respond to Ash and Robinson's 10/14/13 response to his blog post (Ash and Robinson 2013). In their 10/14 response, Ash and Robinson demonstrate again that there is a meaningful coding error in Deaton and Lubotsky's 2003 paper.

But Deaton does use his debate with Ash/Robinson as a basis for drawing conclusions with respect to our professional conduct as regards our 2013 replication of Reinhart/Rogoff. Deaton writes, for example, that "Ash et al. did not submit their paper to a journal where there might have been a chance of an appropriate professional response, but sent it directly to the world press, copying it to Reinhart and Rogoff on the same day." It is not clear how Deaton formed this false impression as to how we proceeded in posting our working paper. We assume that, in leveling his charges, Deaton was unaware that, as noted above, Prof. Reinhart had written to Herndon on 4/4/13 that he should feel free to publish whatever results he wished and that she would not have time to engage with him on the topic. It is also clear that, in leveling his charges, Deaton had no knowledge at all as to the extent of our engagement with the news media at the time we posted our working paper, which we describe in full above.

Deaton goes so far as to charge that we engaged in "smear methods" in posting our working paper. These are grave charges indeed. Deaton needs to explain what exactly we did that constituted such "smear methods." That we posted online our critical replication as a preliminary working paper, *after* Prof. Reinhart wrote that Herndon should feel free to do so and that she would have no time to engage on the topic? That one of us had one discussion with one writer at the Roosevelt Institute after having posted our working paper? That we have now published a revised version of our paper in a professional journal, in which Reinhart/Rogoff and any anyone else is free to submit a comment? Deaton should be willing to explain why he believes this behavior constitutes "smear methods." Otherwise, the appropriate professional course of action would be for Deaton to provide a formal, written retraction of these false charges.

The 2000 Pollin/Brenner study. Prof. Rogoff's reference in his FAQ is to a 9/14/00 online news story that described a meeting of the City Council of Santa Monica, California. The Santa Monica City Council had commissioned me to provide an analysis of a proposed living wage ordinance for the city. The purpose of the meeting was for me to present the findings of this study that I co-authored with Mark Brenner in an open public forum. Rogoff makes no reference to the Pollin/Brenner 2000 study itself or to this context. Nor does Rogoff refer at all to the formal endorsement of our methods and findings by Professor Richard Freeman of Harvard, which is mentioned in the 9/14/00 news story cited by Rogoff. The City of Santa Monica had commissioned Freeman, along with myself, precisely for the purpose of offering an objective professional assessment of our study.[5] Instead of citing Freeman's evaluation of our study, Rogoff chose rather to refer only to the assessment by Richard Sander. At that time, Mr. Sander was serving as a consultant hired by the Santa Monica hotel owners who opposed the City's living wage proposal.

It is not clear why any of this has any bearing on our critique of RR's paper. The minimum professional responsibility by Prof. Rogoff in this case would be for him to explain explicitly why he sees this 13-year old news article as relevant to our critique of his 2010 paper with Reinhart; and if so, why the views of the Santa Monica hotel owners' consultant are somehow relevant to the issues at hand while those of Prof. Freeman are not.

1C) Did we unfairly claim that the Reinhart/Rogoff study supports austerity policies?

In his 10/1/13 FAQ, Rogoff writes: "Is our work about austerity, as HAP imply? No. The word "austerity" does not appear in our 2010 paper," (Rogoff 2013).

In fact, we never stated, or implied, in our 4/13 working paper, our *CJE* published paper, or any other published comments that the RR paper was "about austerity." We were quite careful in all of our writings to avoid making this claim. What we did say was different. That is, quoting from our 4/13 working paper, we said "RR have clearly exerted a major influence in recent years on public policy debates over the management of government debt and fiscal policy more broadly. Their findings have provided significant support for the austerity agenda that has been ascendant in Europe and the United States since 2010 (p. 4)." We then concluded the working paper with the observation that "The fact that RR's findings are wrong should therefore lead us to reassess the austerity agenda itself in both Europe and the United Sates (p. 15)."

These observations are quite distinct from claiming that RR's paper is "about austerity," or that they themselves necessarily support austerity policies. We clarify this point further in our

[5] Indeed, when my co-authors and I published a book *A Measure of Fairness* (Pollin et al. 2008) that drew from this Santa Monica study and related research on living wage policies, Prof. Freeman provided this assessment: "The study of living wages in the US has moved from an odd peripheral topic to a major issue in economic policy analysis largely because of the research reported in *A Measure of Fairness*. This volume defines the issues and provides a glow of empirical sunlight on an economic topic traditionally shrouded with ideology instead of evidence."

published *CJE* paper, in which, among other related observations, we state that "It is an indisputable fact that "Growth in a Time of Debt" has provided a critical intellectual underpinning on behalf of austerity policies. This is the case regardless of whether it was RR's intention to exert this type of influence. We do not attempt to discern RR's intentions on this matter," (HAP 2013B, p. 3, footnote #2).

2. ISSUES WITH DATA AVAILABILITY AND REPLICATION

2A) Did RR make their full dataset publicly available?

In their 4/25/13 technical *NY Times* article, RR write that "We took great pains to provide the data in as accessible form as possible, including especially meticulous source documentation in the spreadsheets," (RR 2013B) Rogoff repeats this point in his 10/1/13 FAQ.

What RR do not acknowledge is that it was not possible to replicate their findings on the basis of the data and methodological explanations they had provided. This includes all of the material they provided online as well as all of the data and descriptions presented in either the working paper or published version of "Growth in a Time of Debt." Thomas Herndon made three separate requests to RR for the full documentation of their data and methodology precisely because he was unable to replicate their findings while working extensively with only the materials RR had provided online along with the brief methodological explanations they provide in either the working paper or published version "Growth in a Time of Debt." This is why it was only when Prof. Reinhart sent Herndon their working Excel spreadsheet on 4/4/13 that we were then able to produce our critical replication, posted on 4/15/13.

After we posted our working paper, we received numerous requests from other researchers for us to provide them with RR's working Excel spreadsheet or to post the RR spreadsheet online. We did not do so, even while we did make fully available online all of the data and calculations that we used ourselves to produce our 4/15 working paper. We did not post RR's Excel spreadsheet online because, on 4/16/13, we asked Reinhart and Rogoff whether they would grant us permission to do so. Prof. Reinhart responded on 4/18 that they would prefer that we not post publicly their spreadsheet or share it with others.

We have fully honored RR's request in this matter. Yet the fact that RR chose not to have their working spreadsheet made publicly available makes clear that they are aware that the data that they provided online as of 4/18/13 was not sufficient for other researchers to conduct a replication of "Growth in a Time of Debt."

2B) Did we fail to appropriately cite other literature on public debt and GDP growth?

In his 10/1/13 FAQ, Rogoff makes two charges on this point: 1) We did "not appropriately cite" RR's own two subsequent papers on the same topic; and 2) We did not "appropriately cite the other literature on growth and debt," (Rogoff 2013).

It is of course true that we did not make these additional citations. The reason for this is straightforward. As is evident in our 4/13 working paper and the 12/13 *CJE* published paper, what we have written is a critical replication, *not* a literature survey. At no point in anything we have written do we claim to have attempted to provide a literature survey of any kind. Here is how my co-authors and I state the matter in the *CJE* version of the paper: "Our paper is a narrowly gauged critical replication. As is standard for such critical replication exercises, we maintain our focus on the two versions of their "Growth in a Time of Debt" paper. Apart from a few brief comments, we do not attempt to integrate our discussion on "Growth in a Time of Debt" into a broader survey of the literature on public indebtedness and GDP growth or other related matters," (HAP 2013B, p. 2).

I would assume that Prof. Rogoff, like us, sees merit in such critical replication exercises in their own right, as distinct from broader literature surveys.[6] But there is a further crucial point here, to which I would assume Prof. Rogoff also would agree: that any and all research papers can be fairly subjected to critical replications, unless the authors have subsequently withdrawn the paper in question in full or in some way explicitly retracted at least some aspects of the paper's findings—either some parts or all of its methodology, data, or results. We have seen no evidence that, prior to the posting of our 4/13 working paper, Reinhart and Rogoff had ever withdrawn "Growth in a Time of Debt" or retracted any aspect of its methodology, data, or results.

3. RESEARCH ERRORS AND METHOLOGICAL DIFFERENCES

My co-authors and I have identified three major problems with "Growth in a Time of Debt" through our critical replication: 1) selective exclusion of available data; 2) Excel spreadsheet coding errors; and 3) inappropriate methods for the weighting of summary statistics.

Reinhart and Rogoff have acknowledged their Excel coding errors, but, as they wrote in their 4/25/13 *New York Times* technical article, "adamantly deny the other accusations," (RR 2013B). Moreover, they state that the coding error is of minor significance. As Rogoff wrote in his 10/1/13 FAQ, "The coding error in the original paper is of minor quantitative significance and hardly central to the debate (Rogoff 2013, p. 1).

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[6] Indeed, our critical replication of "Growth in a Time of Debt" emerged out of an Applied Econometrics course that Michael Ash and I have co-taught to UMass Amherst Ph.D. students for 14 years. The main assignment for every student enrolled in this course throughout the full 14 years we have taught the course has been to conduct a critical replication of one paper in the economics literature. Ash and I have found this to be an exceptionally useful way to bring graduate students to the research frontier on any given topic and to acquire important research skills in the process. As has been widely publicized by now, our joint critical replication of "Growth in a Time of Debt," began as Herndon's term paper for this course. While Prof. Rogoff apparently has made some effort to scan our previous publications, it is surprising that he neglected to mention Zhu, Ash, and Pollin (2004), a critical replication of Levine and Zervos (1998). This previous critical replication with another one of our Ph.D. students grew out of an exactly parallel experience as the 2013 HAP paper.

In fact, as we document carefully, both in our 4/13 working paper and the 12/13 *CJE* published paper, none of the three major problems in "Growth in a Time of Debt" has large quantitative significance on its own. It is rather through the interaction of the effects of these three problems that we derive results as to the impact of high public debt levels of GDP growth that are significantly at variance with those presented by RR. As such, Rogoff is correct that if the only problem with their paper had been its Excel coding error, then there would not be any significant substantive issues worth discussing out of our critical replication exercise.

Yet, the other two major problems that we have identified with "Growth in a Time of Debt" remain, despite the fact that RR "adamantly deny" there are any other problems with their paper. We examine these issues in depth in our 4/13 working paper, and still further in our 12/13 *CJE* paper. There is no need to review all the details we present in the *CJE* paper (to which we again invite Reinhart and Rogoff to respond formally). But in the context of the present discussion, it will be useful to highlight two of the main issues at hand.

3A) Did Reinhart and Rogoff selectively exclude available data?

Despite the fact that, as a general assertion, RR "adamantly deny" that that made any selective data exclusions, at no point in either their 4/25/13 *NY Times* technical article (RR 2013B), their 5/5/13 Errata memo (RR 2013C), Rogoff's 10/1/13 FAQ (Rogoff 2013), or elsewhere do put in writing a specific denial regarding the data exclusions we initially identified in our 4/13 working paper. RR did clearly make these data exclusions in both versions of "Growth in a Time of Debt." They may well have an explanation for having chosen to make these selective data exclusions. The critical point is that, to date, they have not provided an adequate explanation for having done so. In particular, as we state at the conclusion of our *CJE* article, "RR have not addressed their decision to include data for the US in the early post-World War II period while explicitly choosing to exclude data for Australia, Canada and New Zealand for the same years. The US figures for these years support their hypothesis while those from Australia, Canada and New Zealand weaken their hypothesis," (HAP 2013B, p. 21). As Herndon, Ash and I show in our replication, these selective data exclusions do have a significant impact on their overall results, when they are combined with the other two major problems we have identified.

3B) Did Reinhart and Rogoff use inappropriate methods for weighting summary statistics?

On this question, RR have offered no substantive explanation for, or defense of, their methodology subsequent to the posting of our 4/13 working paper. This follows from their having also provided no explanation for their weighting method in either version of "Growth in a Time of Debt" themselves. Rather, they have provided only superficial assertions in support of their methodological choices. Thus, in their 4/25/13 *NY Times* technical article, other than one appeal to authority, their full defense of their methodology in a nearly 2,500 word article is this one-sentence assertion: "It is the accusation that our weighting procedure is unconventional that is itself unconventional," (RR 2013B). Rogoff's 10/1/13 FAQ provides little more of substance, when he writes, in full, that "the weighting scheme we use is completely normal in international finance and many other fields, intended to avoid overweighting possible errors and peculiarities from any one country, and to reduce the effects of high serial correlation," (Rogoff 2013, p. 3).

Rogoff's concern over "overweighting possible errors" and "peculiarities from any one country" seem especially pertinent under the circumstances, given the specific problems my co-authors and I have identified with the RR weighting methods. It therefore seems especially significant that RR have yet to explain the obvious issues of "overweighting" and "peculiarities from any one country" that result through their own weighting approach.

Herndon, Ash and I explain these problems in detail in our 4/13 working paper, and in still further detail in our 12/13 *CJE* article. Fortunately, the gist of the problem can be identified quite easily through examples. The first example concerns a single year's experience in New Zealand, in which public debt was greater than 90 percent of GDP while GDP growth collapsed to *negative* 7.9 percent. As we have shown, with RR's chosen weighting scheme that they continue to defend, this single year's experience in 1951 in New Zealand carries equal weight with 19 years in the UK in which public debt exceeded 90 percent of GDP but annual GDP growth averaged positive 2.4 percent. But the problem is not exclusive to their 1946 – 2009 dataset. With their 1790 – 2009 dataset, as we described in both the Ash/Pollin 4/29/13 "Supplemental Technical Critique" (Ash and Pollin 2013) and more fully in our *CJE* article (HAP 2013B), Norway spent only one year (1946) in the 60 – 90 percent public debt/GDP category during the total 130 years (1880 – 2009) in which data for Norway are included in the sample. Norway's economic growth in this one year was 10.2 percent (due to rapid recovery after occupation during World War II). Norway's one year in the 60 – 90 percent public debt/GDP category, in which GDP happened to grow at an extraordinary rate, receives an equal weight in the RR approach with, for example, 23 years for Canada, 35 years for Austria, 42 years for Italy, and 47 years for Spain.

In short, especially given Rogoff's stated concern over "overweighting possible errors" and "peculiarities from any one country," it would appear incumbent on RR to provide an explicit explanation and defense as to how their method deals with the peculiarities of the New Zealand case in 1951 relative to, say 19 years of high debt in the UK; and the Norway case in 1946 relative to the 23, 35, 42, and 47 years of experiences for Canada, Austria, Italy and Spain respectively in the 60 – 90 percent public debt/GDP categories. To date, RR have provided no such explanations, despite their "adamant" denials that their method is inappropriate.

3C) The Need for Robustness

There is a broader issue at stake in addressing the impact of RR's selective data exclusions and inappropriate weighting methodology. This is the issue of robustness of their findings relative to alternative specifications. Herndon, Ash and I address this point in the conclusion of our *CJE* article, writing that "Whatever happen to be RR's preferences with respect to data exclusions and weighting methodology, they do not acknowledge the need for their main findings to be robust across reasonable alternative methodologies and data choices....We strongly support what we take to be a consensus view of research standards: that any major empirical conclusions need to hold up consistently when one moves from using one method of calculation to another. RR's findings do not meet this standard test for robustness," (p. 22).

4. ARE DIFFERENCES IN RR VS HAP MINOR?

In their 4/25/13 technical *NY Times* article, RR describe what they conclude are the "very small actual differences between our critics' results are ours," (RR 2013B). In making this claim, RR argue in this same article that my co-authors and I had neglected to give attention in our 4/13 working paper to their findings with respect to their median estimates of growth, as opposed to the mean figures; and to their long data series from 1790 – 2009, as opposed to just their modern 1946 – 2009 data.

It is true that our original working paper did not explicitly examine their median figures or their results with their 1790 – 2009 dataset. At the time, Herndon, Ash and I made this decision purely in the interests of space. Our original replication was already roughly twice as long as the published RR 2010 paper itself. Moreover, the basic problems that we had identified with the mean GDP growth figures with the 1946 – 2009 dataset had equally distorted their results with both their median growth calculations for 1946 – 2009 as well as all of their growth calculations with their 1790 – 2009 dataset.

Reinhart and Rogoff appeared to not be aware of the comparable data problems affecting their median and long-period calculations in writing their 4/25/13 *NY Times* response. However, Ash and I first presented the relevant evidence in detail on these matters in our 4/29/13 *Supplemental Technical Response* (Ash and Pollin 2013) which we then also summarized in our *NY Times* rejoinder published on the same date (Pollin and Ash 2013B).[7] We then integrated our analysis of their median growth and long-period calculations in our 12/13 *CJE* article.

Reinhart and Rogoff claim that using either our corrected mean figures or the medians from their 2010 paper, average annual GDP growth declines by about 1 percentage point when the public debt/GDP ratio crosses the 90 percent threshold. But this is not accurate. Some of the key findings are as follows:

4A) Median Growth Figures

According to Reinhart and Rogoff's own recalculations in their 5/5/13 Errata memo, when they include New Zealand data and utilize the New Zealand historical GDP statistics, the

[7] RR make no mention of our Supplemental Technical Analysis in either their 5/5/13 "Errata" document or Rogoff's 10/1/13 FAQ, despite the fact that we refer readers to this supplemental analysis in our 4/29/13 *NY Times* rejoinder.

difference in median GDP growth for countries in the > 90 percent public debt/GDP category over 1946 – 2009 is not about 1 percentage point, but rather 0.4 percentage points (RR 2013C). That is, according to their own recalculations, median GDP growth is 2.9 percent for their 60 – 90 percent public debt/GDP category and 2.5 percent for their > 90 percent category. RR report this result in their Errata memo, but provide no discussion of its possible relevance as regards their claim that median annual GDP growth in their > 90 percent category is about 1 percentage point lower than in their lower public debt/GDP categories.

4B) Long-Period Growth Figures

Using the corrected figures for mean growth over 1790 – 2009 generated by Herndon, Ash and I, the difference in mean GDP growth for countries in the > 90 percent public debt/GDP category relative to the 60 – 90 percent category is, again, 0.4 percentage points, not 1 percentage point. The difference here in average annual GDP growth is between 2.5 and 2.1 percent. Moreover, when we perform the simple robustness check of creating two new public debt/GDP categories, 90 – 120 percent and > 120 percent, we then find that the average annual GDP growth rates for the 60 – 90 percent and 90 – 120 percent categories are actually identical. Average annual GDP growth for both public debt/GDP categories is 2.5 percent (HAP 2013B, p. 18).

4C) Growth Figures for 2000 – 2009

Herndon, Ash and I showed, initially in our 4/13 working paper in and subsequent documents (including HAP 2013B, p. 20), that there is no evidence in these most recent years for any drop off at all in average annual GDP growth when public debt exceeds 90 percent of GDP relative to when the public debt/GDP ratio ranges between 30 – 90 percent. Specifically, we show that mean annual GDP growth over this decade was 1.9 percent for the 30 – 60 percent category, 1.3 percent for the 60 – 90 percent category, and 1.7 percent for the > 90 percent category. RR have yet to acknowledge this pattern in any of their responses to our critique. This is despite the fact that, relative to experiences from 60 or 200 years ago, such recent patterns on GDP growth under high public debt levels are likely to be more informative for assessing present-day policy concerns.

5. IS THIS DEBATE A MERE "ACADEMIC KERFUFFLE?"

This is exactly what RR claim in concluding their 4/25/13 non-technical *NY Times* article "Debt, Growth, and the Austerity Debate," (RR 2013A). Herndon, Ash and I reach a different conclusion. In fact, we believe that the results that have emerged out of this debate carry considerable significance for the setting of economic policy. Consider a situation in which a country is approaching the threshold of a 90 per cent public debt/GDP ratio. It is simply not accurate to assume that these countries are reaching a danger point where economic growth is likely to decline precipitously or even by something approximating a full percentage point.

Rather, our corrected evidence shows that a country's growth may be somewhat slower once it moves past the 90 per cent public debt-to-GDP level. But we cannot count on this being true under all, or even most, circumstances. Are we considering the US demobilization after WWII or New Zealand experiencing a severe one-year recession? Our evidence shows that one needs to ask these and similar questions, including whether slow growth was the cause or consequence of higher public debt, before we can draw meaningful conclusions. Most generally, as my co-authors and I write in concluding our *CJE* paper, "policymakers cannot defend austerity measures on the grounds that public debt levels greater than 90 percent of GDP will consistently produce sharp declines in economic growth," (HAP 2013B, p. 21).

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