Douglas Hibbs
2007 08 25c

Quarterly Journal of Political Science:
Submission, Reviews, Re-submission and Rejection History

1. Submission

From: douglas hibbs [mailto:douglas@douglas-hibbs.com]
Sent: Tuesday, November 21, 2006 9:12 PM
To: 'krehbiel@qjps.com'; 'mccarty@qjps.com'
Subject: RE: QJPS inquiry

Dear Profs Krehbiel and McCarty (aka Profs Keith and Nolan)

Attached please find my paper "The Economy, the War in Iraq and the 2004
Presidential Election" which I submit to QJPS.

Yours, Doug Hibbs

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2. Cover Letter with Reviews

From: Nolan M. McCarty [mailto:nmccarty@Princeton.EDU]
Sent: Wednesday, April 04, 2007 3:49 PM
To: douglas hibbs
Cc: admin@qjps.com; Keith Krehbiel; Alan Gerber
Subject: QJPS Decision

Doug:

We now have two reviews on your paper "The Economy, the War in Iraq, and the 2004
Election." Both referees endorse publishing your piece in some form. The editors'
preferences over form are closer to those expressed by reviewer 2.   In other words, we
would be most interested in publishing a shorter comment on Nordhaus. The essence
of the comment should be that if Nordhaus had used the "Bread and Peace" model
there would be no underperformance to explain. If you accept this offer to revise and
resubmit, we will evaluate the revised manuscript in-house.

The manuscript shoud be pared back in a number of ways. The portion that explicates your original model and details its performance can be substantially condensed. For example, Table 2 can be omitted. The discussion on pages 7-9 can be reduced to a footnote (or we could publish it as an on-line appendix). We also agree with R2 that the model cannot really say much about the effect of the Iraq War, so that part of the paper should be deemphasized.

Regarding the comment on Nordhaus, we ask that you omit editorializing and nonessential background tidbits. Simply state each material erroneous claim and then say what you think the truth is and why you think you are right. If verification requires information from Nordhaus which he does not supply, please say so succinctly and avoid adding unverifiable details. Please refrain from speculation about what Nordhaus may have "just consulted" or whatever. If Nordhaus is not telling what he did, leave it at that. Present your evidence and the readers can draw their own conclusions.

Regarding the discussion of the Fair model(s) you should condense your complaints into a couple paragraphs and refer the readers to Bartels (1997). R1 might enjoy the revised paper less, but we insist on keeping things civil.

We are including a copy of the *QJPS Style Guidelines*. Adhering to its provisions would ease the publication process if we decide in favor of the manuscript. Pay particular attention to the replication policies. A complete replication by our replication editor is necessary prior to publication.

We appreciate your interest in the *QJPS* and look forward to seeing your revised manuscript.

Best,
Nolan, Keith, and Alan

Review #1

Review of "The Economy, the War in Iraq, and the 2004 Presidential Election"

I suggest QJPS publish this paper. HIbbs' methodology for election forecasting is sophisticated, and certainly better (and more prsimonius) than Ray Fair's over-hyped quadrennial exercises in data mining.

Question: What does the number '400' signify in the description of R in the equation on page 3?

I presume that lambda is estimated based on the full model with KIA in

the equation. Hibbs' earlier estimates of the quarterly weights
(e.g., 1982, 1984) obviously did not.   This point about lambda
should be made clear. I note that lambda has grown from about .8 (Hibbs
'84) to .91, where higher numbers approaching 1.00 suggest voters have
long memories. The shifting lambda and its implications might be
worthy of a footnote.

I enjoy reading the catfight with Nordhaus and I have no reason to
doubt Hibbs' position.

Review #2

Subject: The Economy, the War in Iraq and the 2004 Presidential Election
The specific impetus for this piece seems to be "Nordhaus on Hibbs vis-a-vis
Fair." Since Nordhaus's piece appeared in *QJPS*, Hibbs's response may be more
interesting to *QJPS* readers than it otherwise would be. Whether it is interesting enough
to warrant publication is less clear to me. Having disliked Nordhaus's piece when I
served as a referee (partly on the grounds that there was too much Fair and not enough
Hibbs) I haven't gone back to look at the published version. If the comparison of Hibbs
and Fair looms large, then a detailed response may be appropriate. If not, then a very
brief research note may be sufficient to cover the most important points of contention.
(In either case, the substance of what Hibbs has to say about these points seems
generally convincing to me.)

The preceding eight pages of Hibbs's piece provide a summary of the "Bread and
Peace Model" and an application to the 2004 election. The former is, not surprisingly,
competent and clear. On the other hand, it doesn't really add anything to Hibbs's
previous publications describing and justifying the model. The discussion of 2004, which
focuses mostly on Iraq, strikes me as less successful. If one asks how the economy
influenced the 2004 election, it does not seem farfetched to multiply the relevant
economic indicators from 2004 by regression coefficients derived from past elections
(though perhaps not from 2000?). To apply the same approach to Iraq, where the
coefficient is estimated on the basis of *three* previous cases, the most recent almost 30
years in the past (plus some suspect fiddling with definitions of wars, "grace periods,"
and the like) does seem farfetched. Hibbs (inspired by Nordhaus) spends a page
addressing whether casualties should be scaled to population. Fine. But what about
differences in mobilization patterns, budgetary impacts, media coverage, political
rationales, impressions of success or failure on the ground? It is hard to believe that
anyone interested in the political impact of Iraq in the 2004 election would take this
seriously.

What Hibbs *can* say, on the basis of his calculations, is that Bush did not do much
worse than one would have expected on the basis of economic conditions. Perhaps he

did not do much worse because Iraq did not matter much. Perhaps Iraq mattered four times as much as Hibbs claims (accounting for the -.92 prediction error). Perhaps it mattered *ten* times as much as Hibbs claims (and the "real" residual, attributable to Karl Rove's brilliance or John Kerry's hairstyle or whatever, was +1.5 rather than -.92). All of these possibilities, and many more, are perfectly compatible with Hibbs's results. There is just very little information in this sort of analysis for answering questions about the political impact of such specific events as the war in Iraq.

Perhaps the best solution would be to shrink this manuscript down to five or six pages: two on the model, one on its application to the 2004 election (including a very brief discussion of Iraq), and two or three on the various sins of Nordhaus and Fair.

3. Re-submission

From: douglas hibbs [mailto:douglas@douglas-hibbs.com]
Sent: Thursday, April 19, 2007 10:11 PM
To: 'mccarty@qjps.com'; 'krehbiel@qjps.com'
Subject: Hibbs QJPS Revision

Dear Editors

Attached in both doc and pdf formats are the revision I submit to QJPS of "The Economy, the War in Iraq and the 2004 Presidential Election."
I also attach the Figure appearing in the paper in tiff format, and the Stata program and data files used to generate all results. And I also attach an Appendix to the main paper which, as noted below, I wish to have accessible at QJPS should you decide to publish.

Some remarks on the reviewers' comments, the editors' comments and on what I did and did not do in response to same follow.

Concerning R1:
I spelled out the reason for multiplication by "400" in the computation of/formula for quarter-on-quarter real income growth rates (the reason being that it makes the coefficient interpretable in terms of annualized percentage points of growth).
I think it is clear enough from the Bread and Peace equation that the *Fatalities* term (earlier called KIA) is estimated in the presence of the lag sum of real income growth rates, so I have taken no action about the remark on this point.
I add a mention of and a reference pertaining to R1's point about the increase to estimated lambda when the variable Fatalities/KIA is included in the model.

Concerning R2:
    I was somewhat confused by the lines about using coefficient estimates excluding the 2004 observation (in order to generate an out-of-sample, one-election-ahead

prediction of the 2004 outcome). This just replicates the procedure Nordhaus applied to Fair's equations (which is quite standard) in order to obtain a prediction from my model that was comparable to what Nordhaus obtained for Fair's equations. I still do not know for sure how Nordhaus got the numbers attributed to me, but he clearly did not use the same procedure he applied to Fair in generating the "Hibbs" numbers discussed in the text and in his Figure 2 – a point I emphasize in my paper.

R2's remarks about "farfetchedness" of saying anything about Iraq because of the small number of prior cases (what my old econometrics teacher and thesis advisor Art Goldberger calls "micronumerousity") contributing to my estimate of the electoral effect of *Fatalities* (a view endorsed by you – the editors) is misguided. What matters for precision of estimates is not the number of cases but the amount of independent information carried by a variable. One sees this in the standard formula for the estimated variance of a coefficient estimate (the square root of which yields the estimated coefficient standard error that upon division into the point estimate gives the t-ratio from which one can find the p-value), to wit:

$$Var\left(\widehat{b}_k\right) = s_e^2 \Bigg/ \left[\sum_t \left(X_{kt} - \overline{X}_k\right) \cdot \left(1 - R^2_{k \cdot 1, \ldots, k-1, \ldots, K}\right) \right].$$

As depicted in my Figure 1, the large deviations of the vote shares one would have expected from the economy at the (high KIA/Fatalities) elections of 1952 and 1968 mean that my Fatalities variable carries a lot of information despite the small number of non-zero cases – hence the low p-value of 0.003 reported in Table 1 of my paper. I have added a note to the paper, however, that gives the 95% confidence interval for the Fatalities coefficient estimate and I point out that even at the upper extremity of the interval the Iraq effect was much too small to swing the election.

As for other aspects of the Iraq adventure – fiscal effects, media coverage and what not, well sure. Yet that single variable is no weaker then any standard aggregate measure of economic performance – unemployment, real output growth shortfalls, etc – which summarize a whole range of underlying effects like fiscal costs, psychic costs, lost human capital formation and so on. To target Fatalities/KIA along those lines is again misguided in my view. Further, I could extend my paper by adding a review of research done back in the 1970s and early 1980s by Kernell, Mueller, Ostrom and Simon and myself about the effects of American military fatalities on public opinion toward Vietnam and Korea and Gallup presidential popularity ratings, but I do not think this would be of central relevance to the present paper.

Concerning the Editors' Remarks:
I believe I have gotten rid of all bits of tid.
As you suggested, I have deleted Table 2 and the discussion about and estimates of the effect of Fatalities/KIA scaled to population and placed those in an Appendix. If you decide to publish my piece I would like the Appendix made available at QJPS, as noted at the beginning of my message.
Concerning my laundry list of Fair's re-specifications – and their implications for Nordhaus' claims about the superior fits and standard errors of Fair's setups and the relevance of Fair's equations to the electoral effects of "fundamentals" – I have not

shortened. In fact that section now probably runs a bit longer than before. On this score I am not willing to compromise. Although Larry Bartels' correspondence to the JEP complains about Fair's ad-hoc style of model development, his main discussion is about the relevance of Fair's orientation to ex-post prediction and fit to what political scientists are mainly interested in. And to my knowledge no one has ever listed out sequentially the full catalog of changes to the various Fair equations as one election followed the next. In fact when I pieced that sequence of changes together myself from various documents at Fair's web site I was surprised at just how many modifications Fair had made, and I have been following this literature quite carefully for more than 30 years. All by itself I regard this section of my paper as providing useful perspective on the Fair equation(s) which economists at least always take to be the benchmark model of the effects of fundamentals on Presidential voting outcomes (well illustrated, of course, by Bill Nordhaus' QJPS article).

A Small Error in Nordhaus' Background documentation:
The Regression table at page 4 of the document QJPS archived for Nordhaus' paper (earlier posted also at Nordhaus' web site) should show "Sample 1916-2004" (yielding 23 observations), not "Sample 1916-2000." The Sample 1916-2000 (22 observations) pertains to the Regression table on page 3 and is correctly labeled there.
I hope you will come to a speedy decision so that I can expeditiously seek to publish elsewhere if the QJPS sky falls down upon me.
Best wishes from the North, Doug Hibbs

4. Rejection

From: Krehbiel, Keith [mailto:krehbiel@stanford.edu]
Sent: Thursday, April 26, 2007 7:52 PM
To: douglas hibbs
Cc: Alan Gerber; Krehbiel; McCarty; QJPS
Subject: QJPS manuscript [6058]

Dear Doug,
Thank you for the revision of "The Economy, the War in Iraq and the 2004 Election." The manuscript has improved as a result of your responsiveness to referees and editors, but there are two outstanding issues.
The first issue is the tone of the manuscript which we continue to regard as unnecessarily acerbic. Although this could probably be edited away in due course, we had hoped the problem would have been taken care of in one revision. (And while we concede that this matter is subjective, for what it's worth, we three independently reached the same assessments.)
The second issue is your decision to decline our request that you cut back on the catalogue of Fair's shortcomings, an issue about which you write: "On this score I am not willing to compromise." We, of course, respect your autonomy on such matters and

regret that we may not have been sufficiently clear about our wishes to make the comment shorter and more focused.

The upshot is that it seems improbable that we can reach a state in which you can make the points you wish to make the way you want to make them and in which we can publish a comment that we regard as appropriately brief and Nordhaus-germane. We regret that things did not work out as well as they might have, but we nevertheless appreciate your interest in the QJPS and extend our best wishes as you pursue publication of your manuscript elsewhere.

Sincerely,

Keith, Nolan, and Alan