

**CHRONOLOGY OF A CONFLICT:
UNETHICAL EDITORIAL CONDUCT AND ABUSE OF POWER
AT *POLITICS AND THE LIFE SCIENCES****

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Introduction. In 1996, after conducting twenty-six years of research on the topic, I published *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives*. This book contained numerous statistical analyses, including a meta-analysis of the birth-order literature. Meta-analysis is a complex statistical procedure for combining previously published results. Through meta-analytic amalgamations of previous findings of a similar nature, statistical error is reduced in the same way that sampling errors are reduced in polling when using large samples. To accomplish this meta-analysis of more than a hundred previously published studies, I spent months classifying outcomes in theoretically meaningful ways and then tabulating the results. To verify my methods and to discuss relevant technical issues, I consulted with meta-analytic experts at Harvard University, including Robert Rosenthal and Donald Rubin. In connection with my analyses of more than a hundred historical events in science, politics, and religion, I also recruited 110 expert historians to provide nominations and to perform ratings, in an effort to facilitate reliability and validity.

In 1997, a year after my book appeared, Frederic Townsend--a commodities trader and lawyer who appears to have little or no advanced training in behavioral science or statistics, much less the complicated methods of meta-analysis--felt qualified to write a review of my

* [Http://www.sulloway.org/chronology.html](http://www.sulloway.org/chronology.html)

book, which purported to make important criticisms of my methods and results. Not understanding the complexities of the various statistical procedures used by me, Townsend resorted to numerous unscientific, intemperate, and wild assertions.

I wrote to Townsend, detailing the numerous statistical and other errors in his interpretations and methods, in a twelve-page letter (4 November 1998). By this time, Townsend had expanded his naïve and misguided statistical comments into a longer manuscript. In the spring of 1998 this manuscript was submitted for publication in a journal called *Politics and the Life Sciences*. The many errors that were identified in my letter had been carried over into the new manuscript. Townsend's expanded treatment also contained at least one wild, clearly defamatory statement. Townsend's mistaken analyses and defamatory claims were based on his failure to understand even the most basic statistical methods, as well as his failure to consult relevant scientific publications.

Although Townsend had been informed about many of the problems in his second manuscript, he made no effort to correct them before publication, instead writing to say it was too late. His manuscript was scheduled to be circulated to commentators as part of an open peer commentary format, but the manuscript had not in fact been sent out, so changes were still possible. My 4 November 1998 letter to Townsend had included a three-page list of the most important errors and inconsistencies in published summaries of data that he was using to criticize my meta-analysis of the birth-order literature. There were more than forty such errors as well as other serious technical problems. Without first correcting these errors in the published literature summary (which required consultation of the original publications), Townsend's attempt to replicate was impossible (Suloway, 2000, [2002a](#)). Townsend at first ignored this list of errors and other corrective information that I sent to

him, which called into question his whole argument about these data, as well as other matters. Instead, Townsend attempted to publish his original claims without alteration. In addition, he made no reference to my prior communication of this corrective information and continued to claim that I had not "released" my data. Hence Townsend had made clear that he either did not understand, or had decided not to follow, typical ethical and publication norms in science. These norms dictate that one does not knowingly publish errors. If one discovers that there are serious errors in a manuscript, prior to publication, it is the ethical standard that one corrects these errors.

Based on Townsend's unwillingness to follow what I considered basic norms and ethical standards in science in this particular episode, together with his lack of understanding of even the most basic statistical concepts and methods, I decided not to continue to treat him with certain time-consuming courtesies I might have afforded to my scientific colleagues. I was no longer willing to spoon-feed him data that were already available in the public domain, which he could readily find and examine himself. To determine the correctness of this decision, I consulted experts on scientific publication and ethics. My decision to cease scientific exchange of data with Townsend was fully supported by senior officials whom I consulted at the American Psychological Association, the American Psychological Society, and the National Science Foundation, as well as by numerous scientists I conferred with at the Center for Advanced Study in the Behavioral Sciences (Stanford University) and around the country.

Gary R. Johnson, the editor of *Politics and the Life Sciences*, decided to publish Townsend's error-ridden manuscript, including libelous claims, even though Johnson had been previously warned by at least one peer reviewer of the inappropriateness of publishing

such an unscientific diatribe. On 2 December 1998, Johnson sent Townsend's target piece to me and many of my peers, for commentary before publication. A few days later, upon superficial review of Townsend's target manuscript, I saw that it contained many of the same errors that I had already identified in his book review and about which I had written to him. Hence I sent that correspondence to Gary Johnson, along with a cover letter alerting him to the many misleading accusations that Townsend had already published about my book. On careful reading of Townsend's target manuscript, I realized that he had stepped over the line between misguided criticism and out-and-out libel. In a 10 January 1999 letter sent to Johnson--still before publication of Townsend's target manuscript and the commentaries on it--I objected to the circulation of Townsend's outrageous libelous remarks, raised legal concerns, and stated that I would take unspecified action to protect myself if Johnson did not correct the offending portions of his manuscript. Townsend's conduct could not simply be explained by the excuse of naivete. He had already been told in detail about his errors and had made no effort to correct them. This was a striking departure from the norms of responsible scientific criticism. What follows is a chronology of the events that transpired thereafter at *Politics and the Life Sciences*.

Unfair Format and Rules of Exchange. Instead of publishing Townsend's 1998 manuscript as a book review, which would have been the customary practice for a criticism of a book, Gary Johnson decided to make Townsend's critique into a so-called "target article." Historically, a target piece is the original work or a synopsis of the original work, not one person's critique (much less a review authored by someone with no scientific or statistical knowledge and expertise).

This seemed an unfair and biased editorial move on Johnson's part. Johnson himself later admitted to me that, in nearly a decade as a journal editor, he had never employed this unconventional format before. Analogous to a biased trial, this format is about as fair as asking jurors (the commentators) to reach a verdict, having only heard one side of the argument. I protested, to no avail, that if my work was to be critiqued, it would normally be the target article, and the discussion would follow standard journal debate rules. However, if one were to use this odd format, it would seem to be more fair and balanced to at least present both the critique and the author's rejoinder to the commentators at the same time, so the commentators could consider both sides of the arguments before responding. Johnson refused to permit this more balanced format.

In addition to the biased format, Johnson's actions were in other ways unjust to me from the start. In his hurry, Johnson did not perform normal editorial duties of redacting what appeared to be defamatory content, before circulating Townsend's critique to anyone. Instead, he sent unjustified libelous statements to not only reviewers but later to commentators. I have read many other reviews of *Born to Rebel*, and my previous book on Freud (Sulloway, 1979), that contained criticism I considered incorrect or unduly harsh. However, this particular instance went far beyond academic criticism to become a reckless, inflammatory attack filled with needless innuendo and apparent libel.

Not only was the format biased, but initially I was not even invited to reply to the commentators (only Townsend was to be allowed to do so). According to these unusual rules of the exchange, I could be criticized by anyone in the discussion, but could only respond to Townsend. After I protested the bizarre unfairness of this additional feature of the already

unbalanced format, Johnson relented, promising to allow me to reply to all the commentators, as well as to Townsend. This promise Johnson subsequently failed to fulfill.

I was also very constrained in my ability to explain my side of the arguments, because I was allowed less than half of the total number of words allotted to Townsend and the commentators. Why was this a problem? As an analogy, imagine that an amateur scientist with no training in cosmology submitted an article to *Scientific American* critiquing a respected cosmologist and accusing him of scientific fraud and deceit. (In fact, *Scientific American*, along with many other scientific journals, occasionally does receive such manuscripts.) Imagine also that *Scientific American* elected not only to publish this amateur's article, but then invited a team of other cosmologists to comment on it, and also then invited the first cosmologist himself to comment. Because the amateur would likely not understand the physics and mathematics behind this cosmologist's theories, the cosmologist himself would have to allocate a large amount of his space just to explaining the basics of his own theory and why the amateur had misunderstood it. This scenario is ridiculous, of course, but no less so than what happened at the journal *Politics and the Life Sciences*.

In retrospect, I should not have agreed to participate in this exchange, because the deck was stacked from the beginning. However, the mere publication of such defamatory accusations can damage an innocent person's reputation. Scientists are particularly vulnerable to such smear tactics, as their profession is that of "searching for the truth." Even entirely false allegations of sloppiness or intentional wrongdoing can besmirch a scholar's reputation and can cast doubt on the credibility of that scientist's work. Hence, editors have a professional responsibility not to recklessly publish unsubstantiated accusations, especially

those that are being made by individuals who lack the training and expertise to judge the underlying work.

Another unfair aspect of the format involved the element of time. As a busy scholar, then at the Center for Advanced Study in the Behavioral Sciences (Stanford), I was involved in other prior publishing commitments with their own deadlines. When I asked Johnson to postpone publication of this exchange to the next issue of his journal, so that I could have adequate time for a reply, Johnson informed me that he did not have a sufficient backlog of submissions that could supply the current issue and that publication would have to proceed with or without my reply. Had I not been the target of criticism, I would have simply declined to participate. But under the circumstances, I was forced to drop what I was doing and turn my attention to the *PLS* issue, despite adequate time for a thoughtful reply.

Several journal editors I consulted at this time agree with this general assessment about the unfair nature of Johnson's peculiar roundtable format and rules of exchange. One of these journal editors (Stevan Harnad, of *Behavioral and Brain Sciences*) wrote to Johnson to recommend a significant change to correct this "unbalanced" format (namely, allowing me to write a *Precis* of *Born to Rebel*, as is customary with scholarly exchanges involving books, to precede Townsend's critique). Johnson rejected this recommended change.

Apparent Defamation. My six-page letter to Gary Johnson (10 January 1999) stated that I thought the Townsend manuscript--especially the Abstract--was defamatory. It is very important here to differentiate between the intellectual content of Townsend's critique--however critical and misguided it may have been--and any carelessly or recklessly written portions of manuscript that had crossed over the line into libel. As becomes apparent from my subsequent actions, I had no objection to the publication of Townsend's criticism, devoid

of a small number of defamatory passages, and I explicitly encouraged Johnson to publish this critique of my work in a cleaned-up (legal) version. I was fully aware, moreover, of the potential harm that could be done to my reputation if I was falsely portrayed as having tried to squash publication of another person's intellectual criticisms--no matter how ignorant these criticisms might be. Accordingly, I took extensive steps to be certain that it was clear to Johnson and to everyone that I was not trying to subvert criticism, just defamation. Science is not above the law. As every journal editor knows, freedom of expression does have certain legal limits, and it is the job of the editor to enforce those limits--but Johnson had not done so.

Johnson himself was clearly aware of the problem with Townsend's manuscript. In December 1998, he told me by telephone that Townsend, who has apparently has no advanced training or degree in science, had little idea what kind of language was permissible in a scientific publication. At least two of the four reviewers had warned Johnson about the intemperate language contained in the target manuscript. Johnson also said that the next issue of *PLS* had gotten behind schedule owing to the extensive work required in trying to edit Townsend's intemperately worded manuscript.

Intemperate language was not the only problem with Townsend's manuscript. Townsend claimed, for example, to be performing independent replications of my research (and to have failed to obtain the same results). Owing to his ignorance of basic statistical methodology and practices, he was doing no such thing. A replication is not something that vaguely approximates the original study. To the extent humanly possible, it must be an exact duplicate, so that explanations of different outcomes do not fall on these confounding factors that have changed. If the person who is conducting the replication changes the methods

and/or the analyses, he or she has not replicated the original work. Townsend did just this. He conducted a different study with different methods and analyses and then declared that it was a “replication,” which it was not. Failure to find the same results in a different study may be interesting, but such an outcome does not refute the original study, because the methods and statistics change and hence can change the outcome. This would be like altering the ingredients and cooking time in a famous chef’s dessert recipe and then complaining that the chef had done something amiss because one's own dessert did not taste the same.

Townsend had failed to recognize an essential feature of scientific research. Different researchers sometimes come to different conclusions for completely justifiable reasons. These sources of discrepancy include differing samples, differing methods, sampling error, disparities in statistical power, inadvertent mistakes, insufficient training or expertise, and tacit knowledge (Rosenthal and Rosnow, 1991; Collins, 1992). Furthermore, differing results from efforts at replication are often a source of scientific advance, because such differences may point to important moderator variables.

Responsible scholars attempt to rule out such methodological issues before jumping to the conclusion of any wrongdoing. When I have encountered problems in replicating other researchers' findings, or when I encountered what I thought to be significant errors in their reported findings, I have contacted such investigators for clarification, before publishing on the subject. Other scholars have occasionally contacted me for the same reasons. By contrast, Townsend, who had already received corrections of his work from me by letter, before publication of his own manuscript and before its dissemination to *PLS* commentators, made no effort to amend his various analyses in order to ascertain whether such technical

issues were responsible for his inability to replicate. As I have shown elsewhere (Sulloway, 2000, [2002a](#), [2002b](#)), Townsend's repeated failures to perform even remotely accurate replications, together with his disregard for the requisite methodological information, has led him to numerous incorrect and often reckless conclusions (Townsend, 1997, 2000). When these numerous methodological oversights and associated errors are taken into account, Townsend's own data yield consistent replications and confirmations of my previous results.

My findings have also been replicated and confirmed by other researchers (Numbers, 1998; Salmon and Daly, 1998; Paulhus, Trapnell, and Chen, 1999; Zweigenhaft and Von Ammon, 2000; Hertwig, Davis, and Sulloway, 2002; and Rhode et al. 2003). Null findings also continue to be reported in birth-order research, as they have for the last half century (see, for example, Jefferson, Herbst, and McCrae, 1998; and Freese, Powell, and Steelman, 1999). Such null results appear to reflect differences in methodology, samples, statistical power, and behavioral context, as well as other factors that are normal sources of disparities in behavioral research.

Shortly after I first examined Townsend's (1998) manuscript, two lawyers informed me that the manuscript was libelous, and four respected journal editors told me that it contained language and assertions that were unacceptable in a scientific publication and that also appeared, to some of them, to be libelous. Arie Kruglanski, editor of the Attitudes and Social Cognition section of the *Journal of Personality and Social Psychology*, was one of the four journal editors who examined the original Townsend manuscript. Another was Michael Shermer, editor of *Skeptic* magazine and a regular columnist for *Scientific American*. Lawrence Pervin, former editor of *Psychological Inquiry*, wrote to me on 11 February 1999 that certain sentences in Townsend's Abstract were objectionable and ought to be deleted,

and he hoped that Johnson would not attack me for making such a legitimate request. The fourth (former) journal editor was Neil Smelser, then Director of the Center for Advanced Study in the Behavior Sciences, at Stanford University, where I was spending the year in 1998-1999. The manuscript was also examined by Lynn Gale, the Center Statistician, who found it to be "so flawed in content, unacademic in style, and outrageous in its language that it should *not* have passed any competent, unbiased peer review team."

In addition, Martin Daly (McMaster University), who had been a peer reviewer of the Townsend target manuscript, described it to me (and to Johnson in his peer review) as "a bloated, biased rant." After reading a draft of my 10 January 1999 letter of complaint to Johnson, before it was sent to Johnson, Martin Daly's response was that he had previously pointed out some of these same problems in language and content to Johnson. (Daly is coeditor of *Evolution and Human Behavior*.)

These collective judgments by five journal editors and a professional statistician underscore an unfortunate reality about peer review in science, namely, that it is an imperfect mechanism for adjudicating publication merit, especially in less prominent journals. As one observer of the problem has wryly noted: "Let's admit it--and this is the real dirty secret of academic publishing--one can publish just about anything if one goes far enough down the list of [journal] impact factors. There are papers all around us containing problems glaring enough to fail their authors in undergraduate midterm exams" (Svetlov, 2004). The publication of such papers usually causes little harm, as errors of any significance will generally be corrected by other investigators. But the publication of error-ridden papers, and the making of false allegations based on such errors, is another matter entirely.

One might wonder why I reacted the way I did to Gary Johnson's early editorial conduct. If Johnson had fulfilled his editorial responsibilities, he would have insisted that Townsend remove any wild, unsubstantiated allegations and not distribute the defamatory form of Townsend's manuscript to the reviewers and commentators, who are my professional colleagues. However, Johnson had shirked this most basic obligation not to circulate libelous material. By the time I saw Townsend's paper, it had already been widely disseminated in its defamatory form. Hence, from the start Gary Johnson was remiss in his editorial responsibilities, violating my professional and legal rights. This circumstance just added to the fact that Johnson, from the beginning, had shown bias, unfairness, and a lack of editorial neutrality. The standard exchange starts out with a neutral target article that is then lauded or criticized by a balance of commentators. By making Townsend's hostile diatribe the center of the discussion, Johnson had created an inequitable format with a distinctly negative bias at the outset toward me and my work. This format was potentially prejudicing responses by the commentators, who were being denied access to my response.

Johnson Mischaracterizes My Position. Gary Johnson interpreted my 10 January 1999 complaint as a threat to sue him and/or his publication. While I was asserting that I had normal legal rights, which he had a responsibility not to recklessly violate, I had no intention of litigating anything. Somehow Johnson seemed to believe that because I had become an academic, I had waived my legal right not to be libeled. Academic or scientific criticism is one thing. Outright lies and personal attacks are another.

Instead of reflecting on the illegality of his conduct and rectifying the situation, Johnson got on his high horse and launched a self-righteous defense that I was trying to interfere with the progress of science and trying to suppress free speech. This was absurd. I have been

criticized by many scholars and lay reviewers in my career. Some of these criticisms were well founded; others, I believe, were unfounded. But according to five journal editors and two attorneys, Townsend's manuscript went way beyond the bounds of scientific discourse into the unfettered accusations of someone who knew little about which they wrote.

However, I had no intention of suing anyone, so I repeatedly wrote Johnson to reassure him on this point. Instead, I wanted Johnson's actions and Townsend's writings to be lawful--not to violate my normal legal right not to be defamed. In two different letters I wrote in February 1999 I reiterated that I had no intention of suing Johnson, his journal, or anyone associated with it in connection with Townsend's manuscript and that I very much wished for publication to go forward, with the few reckless, defamatory statements modified or removed. For example, I stated on 18 February 1999: "As I have said before, it is not my intention to sue you or your journal over the publication of Townsend's manuscript." I did quite obviously want Johnson to edit out any material that he found, after consultation with attorneys, to be libelous, as is customary for editors to do.

In February 1999, rather than to respect my rights to fair treatment and solid criticism, Johnson threatened to entirely cancel publication of the roundtable debate unless I signed a legal release indemnifying all concerned parties for all future damages, even for text that had not yet been written and that I would not be able to see before publication. After being told by an attorney that "no attorney in the country would advise you to sign that release," I declined to sign away all of my past, present, and future legal rights with respect to whatever Johnson might choose to publish. Several of the journal editors I had previously consulted, as well as other professional colleagues, uniformly supported me in this decision. No one ever asked me to sign a more reasonable and limited indemnification.

To assure that the intellectual debate was published, given Johnson's threat to cancel it, I then approached the editors of two journals, *Psychological Inquiry* and *Behavioral and Brain Sciences*, and proposed that a roundtable discussion be published there and that Townsend specifically be invited to participate in this exchange, so that Townsend's own right to free expression would be preserved. I believed that these two mainstream journal would follow legal and customary scientific editing practices, while allowing legal free speech and criticism. At this time *Psychological Inquiry* had a backlog of previously proposed roundtable discussions. But my proposal to *BBS*, made to editor Stevan Harnad on 19 April 1999, was encouraged (subject to the customary procedures of peer review). The possibility of being considered for a multiple commentary in *BBS* was ruled out when I received Johnson's letter of 30 April 1999, announcing that the roundtable exchange was not going to be cancelled after all, but rather would go forward.

Johnson Proposes New Ground Rules for the Exchange and Then Covertly Changes these Rules. In a letter dated 30 April 1999, Johnson informed commentators that publication of Townsend's manuscript was going forward and that he and Townsend would "reexamine his manuscript to see if any passages might be usefully rephrased" and that the manuscript, "with or without changes," would then be published. Any changes, moreover, would be individually indicated for the commentators, in case these changes affected anything that the commentators or I had already written in response to Townsend. By implication, the changes would be modest. Based on this assurance, which seemed to resolve the whole problem, I proceeded to complete an 16,000 word reply to Townsend's target critique.

Instead of doing what he had promised, at some unknown time after 30 April 1999, Johnson covertly gave Townsend permission to make wholesale changes in the substance of the manuscript. These changes went far beyond the deletion of the wild, defamatory remarks, and they occupied Townsend over the next year, while we all waited, expecting to receive a slightly more temperate, revised manuscript from Townsend. Instead of receiving a revised manuscript with rephrased passages highlighted, all of us received an almost entirely new manuscript.

Not only did Johnson allow a wholesale rewrite of Townsend's target manuscript, but he communicated my confidential correspondence to Townsend: sending Townsend a sneak preview of my rejoinder, which I had outlined in a six-page letter I had sent Johnson as editor. I had explicitly stated that this communication was to be held in confidence, with the qualification that Johnson could show this letter to his lawyers. By betraying this confidence and sending Townsend many of my intellectual and scientific responses, Townsend was able to incorporate changes in his manuscript to dodge these issues. This foreknowledge of much of my rejoinder allowed Townsend to take full advantage of my positions, *without my permission and, more astonishingly, without any acknowledgment of this fact by Townsend.*

This illicit misappropriation of my unpublished scientific work rendered much of my response irrelevant, and forced me to start almost from scratch to respond to the new Townsend manuscript. The responses of some of the other commentators also appear to have been undermined by this unauthorized transfer of information to Townsend. Johnson claims that he needed to send Townsend my correspondence to help him remove any potential legal problems. An editor's red pen was all that was needed to accomplish *this* task. Furthermore, if Johnson's actions were forthright and correct, why did he hide the fact that he had

breached my confidence, until after Townsend had rewritten his book review incorporating my ideas, and irate commentators began withdrawing from the exchange because their own critiques were now invalidated. Not only was this an apparent breach of editorial neutrality and responsibility, but I suspect it was illegal (and certainly unethical--i.e., editorial misconduct) to circulate my unpublished scientific thoughts and to allow them to be used without acknowledgment by Townsend. In essence, Johnson had invited me to reply to a manuscript that he privately knew he was never going to publish, thereby insuring that my initial reply, on which I spent many months, would also not be published. Because of these ongoing editorial lapses on Johnson's part (which included misappropriation of intellectual resources and breach of promise), I finally protested to his superiors at this university, stating that I thought his actions rose to the level of misconduct. In doing so, I was also taking steps under the state of Michigan's Whistleblower Protection Act, trying unsuccessfully to halt the retaliation that had already started and continues to this day. (Johnson had previously retaliated against me in a letter sent to all of the commentators, as well as by subsequent actions.) My complaint was ethical in nature.

In justification of his secretly sending Townsend not only my itemization of the defamatory items, but also my preliminary rejoinder to the substance of the critique, Johnson has claimed that I demanded that Townsend's manuscript be completely revised. This is untrue. Although I did say that the manuscript might require extensive changes to remove the embedded defamatory remarks, I also stated very clearly in writing that I would be satisfied with the deletion of just one particularly outrageous, defamatory phrase from the Abstract of the manuscript. Irrespective of what was needed to edit this document to make it a legal and scientific criticism, not a slanderous attack, Johnson had no right to mislead me

and the other commentators about what steps he was taking. Nor had he any right to forward privileged communications to Townsend. I drafted my response based on Johnson's 30 April 1999 assurance that the substance of Townsend's target article would be essentially the same, with a few minor revisions or rephrasings highlighted for the benefit of commentators. In addition, Johnson also had an editorial responsibility toward all of the other commentators, who had already written their replies to the original Townsend manuscript.

A Majority of the Commentators Withdraw in Protest. Over a year later, in July 2000, commentators finally received the new Townsend manuscript, which had become a "moving target" and bore little resemblance to the original manuscript. What Townsend and Johnson had done was to take advantage of the situation in order to evade legitimate criticism, after the peer commentary process had already been completed. Soon thereafter, eleven (more than half) of the commentators told Johnson they would withdraw in protest over Johnson's allowing Townsend to submit a mostly new article. All of these commentators were hoping that the editor would agree, in the face of these formal protests, to publish an edited version of the original target article, with only the defamation removed, instead of the largely new manuscript, to which their already written critiques failed to respond. Any additional material Townsend might wish to publish should have been published at a later date, as a normal part of the follow-up discussion.

Johnson was unmoved by these protests and refused to publish any form of the original target article. Therefore, some commentators refused to continue to participate. Other commentators (those who were the most critical of the original manuscript) were forced to withdraw their original critique--now rendered largely irrelevant--and reluctantly chose to drop out of the exchange because they were unwilling or unable to take the time and energy

to write a new commentary on the new critique. Martin Daly (McMaster University) wrote an angry letter of protest to Johnson on 2 August 2000, withdrawing with his coauthor Catherine Salmon. Daly later told me that, in his opinion, Johnson had done the wrong thing every step of the way. An independent perspective on the whole matter was provided by Stevan Harnad, editor of *Behavioral and Brain Sciences*, who wrote on 11 August 2000: "It is not viable, nor is it a reasonable thing to ask of busy people, to go through multiple iterations of this--forcing commentators to read a revised target article, and to revise their commentaries accordingly, or forcing authors to read revised commentaries, and to revise their Responses accordingly. The rules of the game have to be made clear to everyone in advance, and they must be obeyed by all, otherwise precisely the sort of divergence and frustration described below [in Martin Daly's 2 August 2000 protest to Gary Johnson] is likely to occur."

Similarly, Dean Simonton, another of the commentators, complained to Johnson on 6 September 2000: "The manner in which this journal section is being put together departs significantly from professional norms. . . . As a consequence, I would like to withdraw my commentary from the special section. On the other hand, if you are willing to publish Townsend's original essay, then I would be glad to have the original version of my commentary also published. I was a little irked, anyway, that one of the major criticisms in my first comment was rendered completely irrelevant after Townsend was permitted to write what must be considered an entirely new critique--a frustrating experience that I evidently share with other commentators." At another point, Simonton also wrote to me about Johnson's permitting such extensive substantive changes: "It just makes no sense, and

violates the norms of scholarship. It's simply unprecedented and patently unjust. Well, that's my opinion, and you can quote me."

The withdrawal of Martin Daly, Catherine Salomon, Dean Keith Simonton, Lawrence Nyman, Delroy Paulhus, Paul Wehr, Paul Trapnell, Cécile Ernst, B. G. Rosenberg, Allison Rosenberg, and myself created a major problem. If those scientists most critical of Townsend refused to participate, Townsend's new critique would be published largely without commentaries by myself and some of the most critical commentators. In addition, no one would be responding to criticisms of my work by those commentators who had not withdrawn from the exchange. This was particularly problematic, because it was those commentators who had largely ignored Townsend's target article and turned their critical scope on my own work, who were not forced to undertake major rewrites of their commentaries, and hence had not withdrawn in protest. In short, Townsend's substantial rewrites had created yet another bias in the exchange.

Therefore, I reluctantly agreed to write a new response to Townsend and a response to the critiques of my work written by the commentators. I also encouraged the other commentators who had withdrawn to rejoin the debate. My participation required throwing out most of my previously written 16,000-word response, and the authoring of a new one, along with a brief response to the new commentaries, which wasted many weeks of my time.

Of the eleven commentators who had withdrawn from the exchange in protest, eight agreed to reenter the exchange. Two additional commentators, who had been very critical of the first Townsend critique, were unwilling to rewrite their critiques, which had been rendered almost irrelevant by Townsend's substantive changes. A third commentator declined to participate on ethical grounds (in protest over the ethics of Johnson's actions), and

a fourth commentator apparently withdrew at this time. The last of the revised commentaries among the remaining members of the roundtable exchange were submitted for publication in February and March 2001. According to Johnson's letter to the commentators dated 10 July 2000, I was to be given the last word in this round of the exchange.

Another Breach of Editorial Promise: Amazingly, three more years went by before Johnson, in February 2004, finally published the September 2000 issue of *PLS*. Was it over? Did I get the last word, as promised? Having waited three years to publish, Johnson took it upon himself to write a tendentious and scathing editorial blaming me for everything, taking no responsibility for his own part in this sorry saga of unprofessional and biased editorial process. Moreover, the three-year delay in publication, which Johnson has blamed on me, was obviously significantly due to the time Johnson needed to conduct new research for, and to write, this Editorial. In this Editorial, Johnson also published, without permission, carefully selected portions of my letters to him that gave a very misleading portrayal of my own position in this controversy. One of the most infuriating aspects of this Editorial was Johnson's attempt to portray me as trying to silence my critics by stifling freedom of speech through legal threats, even though Johnson knew full well that, when he nearly cancelled publication, I had sought another forum for the debate to preserve freedom of speech. I was not invited to respond to Johnson in the same publication, nor in any future issue.

It is worth noting that Johnson's editorial attack on me constitutes breach of promise, since I was assured, in writing, that if I participated, I would have the last word in the exchange in the September 2000 issue of *PLS*. In his 10 July 2000 letter to all of the commentators, Johnson promised about the format: "Mr. Townsend will be responding, in a limited number of words, to the commentators only--he will not see Dr. Sulloway's response

before completing his own response. *This effectively gives Dr. Sulloway the last word in the initial exchange* [italics added]. Dr. Sulloway and Mr. Townsend may be allowed an additional and final response in the succeeding issue of the journal." (This promise is also noted in Johnson's published Editorial.) My participation in this exchange was contingent on the rules as presented by Johnson, which, like other rules before, were then changed after my submissions were finalized and Johnson had read them (bait and switch). (In my own contributions I chose to take the high road and did not attack Johnson.)

Ironically, not only did Johnson fail to act as an unbiased editor, and not only did he fail to give me the last word in the exchange, as he had promised, but he also decided to abandon any remaining semblance of editorial neutrality and to become a commentator himself. Continuing his ongoing pattern of bizarre and unethical behavior, he published lengthy, but highly selective, portions of my letters, which may be an infringement of my copyright (U. S. copyright gives special protection to unpublished materials, including letters). Clearly showing the bias in editorial process that was there from the beginning, Johnson wrote a lengthy personal attack attempting to exonerate his own editorial misconduct, but did not allow me to respond in any way in his journal. Oddly, Johnson stored up his normal editorial comments about a portion of my text, withholding them in order to deprive me of the opportunity of clarifying my discussion, so that he could augment his critique. He also ended his Editorial criticisms of me with a fund-raising effort, which makes one wonder whether a principal purpose of this criticism was to generate publicity and funds for his floundering journal, which completely ceased publishing for several years. As Dean Simonton, one of the commentators, has observed of Johnson's Editorial: "It's clear to me from reading the abstract that it was inappropriate to the extreme. Editors in putting together target-article-

commentary are supposed to be forthrightly disinterested. To include that long essay casts doubt on the objectivity of the whole business, including his selection of commentators."

Staying true to his biased editorial conduct, Johnson subsequently allowed his Editorial to circulate in electronic form on the Internet in an effort to foment one-sided online discussion of the controversy. By contrast, my own published response to Townsend and to the other commentators, where I deal with the relevant scientific matters, was available only by purchasing a copy of the same *PLS* issue whose sales Johnson has apparently sought to augment through an ongoing campaign aimed at generating news stories for himself and his journal.

Conclusion: The protests of eleven of the commentators, and the informed judgments of five independent journal editors and numerous other scholars, testify to the various inequities, broken promises, misappropriation of intellectual resources, misrepresentations of my position, and unprofessional editorial tactics adopted by Gary Johnson in his efforts to displace responsibility for his own repeated editorial failings. In particular--contrary to Johnson's allegation--I did not try to suppress Townsend's legal free speech, nor the publication of Townsend's intellectual criticisms. Instead, I sought to have Townsend's manuscript published elsewhere, when Johnson threatened to cancel publication altogether. I did, however, insist that I be treated at least legally, given that there was no chance I would be treated fairly.

It should be emphasized that a series of bad editorial decisions are the primary cause of the four-year delay in publication of the September 2000 issue of *Politics and the Life Sciences*. That journal issue should have been published with other material, if necessary, in place of the delayed round table discussion of Townsend's target manuscript. Alternatively, a

modestly revised version of the original Townsend manuscript, as originally proposed by Johnson to commentators on 30 April 1999, could also have been published on time. Of the four years involved in the delayed publication, one year was taken up by Townsend's extensive revisions and nearly three additional years were apparently taken up by Johnson's conducting new research to include in his Editorial, as well as the writing of this Editorial. Following the revision of Townsend's manuscript, my own contributions were completed and sent to the editor on time, as requested, in March 2001. At that time I was informed by Johnson, in writing, that the roundtable exchange would be published with little or no further delay.

In April 2004 a reporter for a prominent scientific publication spent many days interviewing participants in this dispute, including all of the journal editors who I consulted about the original Townsend manuscript (1998). Having begun with the impression conveyed by Johnson's Editorial--namely, that I had tried to stifle free speech--this reporter eventually came to the conclusion that the dispute stemmed mostly from a bizarre editorial lapse on the part of Gary Johnson. At this point, the reporter's editor canceled the story. Of particular interest is the fact that Johnson and Townsend refused to show this reporter a copy of Townsend's original manuscript.

Apparently in an effort to gain further attention for himself and his journal, Johnson filed a complaint of misconduct against me with the National Science Foundation in September 2004. This complaint alleged that I had manipulated and falsified data in connection with my research on birth order. The Inspector General's Office at the National Science Foundation conducted an inquiry into Johnson's allegations and subsequently rejected them (as reported in *Nature*, 435 [2005]:137). Below is the text of the ruling I received, dated 28 December

2004, by the Office of the Inspector General on this matter:

We received an allegation¹ that the subject's² NSF-funded research³ resulted, in part, in publication⁴ of materials⁵ that included falsified and manipulated data.⁶ We reviewed information related to the publications and the data.⁷ We found no evidence of data falsification or manipulation. The focus of this case involved a very controversial theory that included considerable disagreement within the community about the interpretation of the data. A disagreement over the interpretation of data is not research misconduct.

This case is closed and no further action will be taken.

1. [Footnote redacted by NSF.]
2. The subject, Dr. Frank J. Sulloway, is a Visiting Scholar at the Institute of Personality and Social Research, University of California, Berkeley.
3. NSF proposal DIR-9112305.
4. The allegation in this case was discussed in an article in Nature (Vol. 431, 21 October 2004, p. 889).
5. Born to Rebel, (Pantheon, 1996; Vintage, 1997) authored by the subject as well as other related publications (www.sulloway.org).
6. [Footnote redacted by NSF.]
7. This includes considerable material provided by [phrase redacted by NSF] the subject.

Note: All of the individuals whose views are quoted in this Chronology, based on unpublished sources, are quoted with permission.

Sources Mentioned in this Document

The following is a list of people who have first-hand knowledge or are otherwise qualified to comment on the facts contained in this chronology.

1. Journal editors who were examined the original Townsend manuscript (1998--which was never published) and who thought it contained language and assertions that should not be permitted in a scientific publication and that appeared, to some of them, to be defamatory:

Professor Arie Kruglanski, University of Maryland, Department of Psychology.

Dr. Michael Shermer, Director, Skeptics Society (Altadena, CA), and *Scientific American* columnist.

Professor Lawrence Pervin, Rutgers University, Department of Psychology.

Professor Neil Smelser, University of California, Berkeley, Department of Sociology.

2. Commentators who withdrew from the exchange because of Johnson's decision to allow secret and sweeping revisions of the original Townsend manuscript, and who also protested against this editorial decision in writing:

Professor Dean Keith Simonton, University of California, Davis, Department of Psychology.

Professor Martin Daly, McMaster University, Hamilton, Ontario, Department of Psychology. In their commentary on Townsend's (1998) manuscript, which they were forced to withdraw from publication owing to Townsend's extensive revisions, Daly and coauthor Catherine Salmon (1999) commented: "*Born to Rebel* should not be considered the final word on birth order effects. However, it has advanced the topic both conceptually and empirically, whereas Townsend's critique misrepresents Sulloway's work and contributes little. We hope that scientists will read *Born to Rebel* and judge for themselves" (quoted from "Birth order has real effects that indignant denials cannot banish," unpublished manuscript).

Professor Del Paulhus, University of British Columbia, Department of Psychology.

Professor Lawrence Nyman, Professor Emeritus, City College of New York, Department of Psychology.

3. An expert on science fraud who has frequently advised me on how to respond to Johnson's unprofessional editorial actions, breached promises, and misrepresentations:

Dr. Carolyn Phinney, U.C. Berkeley Ph.D. psychologist, Orinda, CA.

4. A respected journal editor who wrote to Johnson about the unbalanced nature of his format and who also subsequently criticized Johnson's decision to allow Townsend to make wholesale substantive changes in his 1998 manuscript, after commentaries had been written and submitted for publication.

Professor Stevan Harnad, University of Southampton, Cognitive Sciences Centre.

5. Other scholars who are familiar with the original Townsend manuscript (1998) and Johnson's unprofessional editorial actions and can comment them:

Dr. Lynn Gale, Center Statistician, Center for Advanced Study in the Behavioral Sciences, Stanford. Dr. Gale, along with Center Statistician Lincoln Moses, provided extensive statistical advice in connection with my meta-analysis of the birth-order literature and can comment on the ways in which Townsend's supposed replication of my research was not a replication at all and also fails to follow accepted scientific procedures and basic statistical methodology. She can also comment on other aspects of Townsend's scientific and statistical shortcomings, as well as Johnson's biased editorial actions. As Gale has asserted in print, "four other [journal] editors, many colleagues, and, more recently, an independent science reporter investigating the entire affair, have all concluded that Johnson's irregular and questionable editorial practices were the crux of the problem" (Gale, 2004).

Professor Michael Shanahan, University of North Carolina, Department of Sociology. Shanahan was closely involved with my exchanges with Johnson from the beginning, when we spent the year together at the Center for Advanced Study in the Behavioral Sciences (1998-1999). See Shanahan (2004).

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