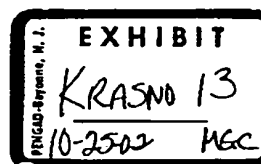


Rebuttal to Professor James L. Gibson

Jonathan S. Krasno
Yale University

Professor James L. Gibson raises a series of concerns – some serious and some less so – about the 1998 & 2000 editions of *Buying Time* and the data sets from which they were derived. In this rebuttal, I address many of these issues by drawing on my experience as lead author of *Buying Time: Television Advertising in the 1998 Congressional Elections (Buying Time 1998)*, author of the grant proposal that produced it, and author of the coding instrument used in 1998 and later adopted with little revision for 2000. I confine my remarks to Professor Gibson’s discussion of *Buying Time 1998*, although they may also be relevant to his critique of the later volume.

I organize my comments into four sections. The first clarifies some confusion about the origins of *Buying Time 1998* by briefly recounting the relevant history behind it. These clarifications, I believe, relieve many of Professor Gibson’s concerns. The second section takes up the purported difficulties replicating its findings, a major theme of Professor Gibson’s report. The third and fourth sections address his two most serious claims in turn: that the analysis of “pure” issue ads ignores a more appropriate means to designate these ads, and that the analysis of the impact of McCain-Feingold in *Buying Time 1998* misses the relevant theoretical questions and is wrong. In both cases, a careful examination of Professor Gibson’s claims reveals important errors in his reasoning and computations, and shows that the original findings in *Buying Time 1998* are correct.



1. The origins of *Buying Time* 1998

Professor Gibson notes that the grant proposal that produced the first *Buying Time* is an advocacy document (fn. 3). As its author, I can confirm that that is largely true. Like many other political scientists, influenced by the commercials we had seen on television and the vast litany of complaints and comments from politicians, journalists, and judges, I had come to believe that political parties and interest groups were using the magic words test as cover to sponsor thinly-veiled campaign ads masquerading as issue advocacy. What no one knew – at least not for certain – was the scope and content of candidate-oriented and “pure” issue advocacy.¹

The *Buying Time* study was meant to supply that information, but to be authoritative it had to do so in a way that was as value-free and defensible as possible. In short, it had to be able to stand up to accusations, like Professor Gibson’s, that I and others associated with the study stacked the deck to find what we were looking for. The fact that we expected certain results (and that those expectations were largely realized) loads the issue emotionally, but misses the point. Scholars rarely embark upon research without some expectations as to its results. But more than most scholars, we had a compelling reason to insure that our results could withstand allegations of bias. Thus, some of the very things that Professor Gibson questions – the presence of alternate codings of q11 in the 2000 data set, the extensive e-mail discussions about various cases and issues, the comprehensive presentation of the results from the 1998 study without commentary – could and should be viewed as part of concerted effort to make certain that the results and data were valid and reliable.

¹The Annenberg Public Policy Center estimated \$135 million was spent on issue advertising in 1996, but this estimate does not distinguish between candidate-oriented and pure issue ads.

The other imperative of the *Buying Time 1998* project was timing. To satisfy the demands of the funders and policy-makers, the results of the study had to be available in the shortest possible time. The political calendar dictated that the publication date of *Buying Time 1998* be in late April, 2000 – approximately six months after the data were delivered. That abbreviated timetable created enormous difficulties, and Professor Gibson and others are welcome to question whether the sheer amount of analysis conducted over this short period could be entirely free of errors (see Section II below). But, at the same time, the need to meet a short deadline explains away a number of concerns raised by Professor Gibson. These include:

- The lack of text accompanying the tables in *Buying Time 1998* (p. 4, 5). The main reason was to allow readers to draw their own conclusions, but the secondary reason was more practical: lack of time.
- The role of Daniel Seltz (p. 6). Mr. Seltz organized an enormous numbers of tasks relevant to the preparation of *Buying Time 1998*. His myriad responsibilities did not include data analysis and Professor Gibson's fear that he contributed findings to *Buying Time 1998* are unfounded.
- The constant consultation with Brennan Center attorneys (fn. 3). Brennan Center attorneys – along with a larger number of political scientists – were consulted in preparing the coding instrument in order to make certain that the resulting analysis addressed policy concerns and proper legal categories. There was no consultation with attorneys about the results of the analysis, both for reasons of propriety and practicality. The short time window left no opportunity to request or receive feedback on results, even if Mr. Seltz and I had wanted to consult. Indeed, it left no time to adjust the coding instrument at all once the process had begun.²
- The lack of peer review (p. 4, 5). The need to release the results made the normal peer review process – which adds months and, sometimes, years to publication dates – impossible. In fact, subsequent publications by myself and by Professor Goldstein have

²A meeting about coding the storyboards in the late winter/early spring of 1999 led to a first draft of the coding instrument. Comments were solicited via e-mail in March, then a final version was created the following month. No changes occurred after that point. Actual coding began in late April, but results were not available until the fall when the broadcast data were purchased.

withstood the peer review process.³ Furthermore, peer reviewers are rarely, if ever, given the authors' data to evaluate, instead focusing on their theoretical arguments, supporting analysis, and presentation. The absence of peer review no more automatically disqualifies *Buying Time 1998* than the presence of peer review automatically guarantees the quality or accuracy of every book or article published.

- The "data set in constant flux." The need to release results after a few short months inevitably meant that small changes to the data set would continue even after the release of *Buying Time 1998*. This process, far from being unusual, is extremely ordinary. Virtually every provider of large data sets, from the National Election Studies to the Commerce Department, prepares versions of their data and continues to fix problems in subsequent releases. Any problems with the CMAG data set arise from the sheer complexity of the enterprise, from the gradual filling in of missing data and the discovery of internal contradictions. There is no evidence at all in Professor Gibson's report that any of the changes in the successive versions of the data that he examined had more than a trivial impact on his results or on those reported in *Buying Time 1998*.

This background also sheds light on the role of Professor Goldstein, a topic which Professor Gibson raises repeatedly in his report. Professor Goldstein's responsibilities for the 1998 project included supervising the coding of the storyboards, providing additional contextual information about the ads, merging this information with the broadcast data, and advising on use of the data set. No item on this impressive list includes any responsibility for the grant proposal or the results of the data analysis that I performed several thousand miles away. This division of labor certainly reduces, if not eliminates, any incentives Professor Goldstein could have had to tilt the results of the analysis in any direction. In fact, Professor Goldstein's prospects in academia – his chosen career – would make such behavior inimical to his interests and to the project itself. Professor Gibson's persistent questioning of Professor Goldstein's motives somehow manages to lose sight of this simple and obvious fact.

Professor Goldstein's work adding contextual information to the data set warrants special attention. Since the coders received no formal training and were not experts on the several

³See, for example, Jonathan Krasno and Kenneth Goldstein, "The Facts About Television Advertising and the McCain-Feingold Bill," (PS 35: 207-212), 2002.

hundred federal candidates who appeared in 1998's advertisements, it fell to Professor Goldstein to add information about the actual race in which an ad appeared and its partisan direction.⁴ Both tasks were relatively straightforward exercises which Professor Goldstein, with his knowledge of American politics, could easily accomplish. It is worth noting that Professor Goldstein's contextual coding occurred independently of the content coding, thus providing a useful way to check the accuracy of both processes. This division of labor revealed occasional inconsistencies in the 1998 data set which resulted in some additional correction of errors.

Finally, Professor Gibson raises a slew of objections to various aspects of the CMAG data from the fact that it covers just 75 media markets (that reach 75-80 percent of the public) and the accuracy of the CMAG system, to the missing video frames in the storyboards (see particularly p. 5-6). All of these characteristics are painstakingly explained in *Buying Time 1998* and elsewhere. None strikes me as particularly serious. Indeed, it is noteworthy that Professor Gibson advances no hypotheses to show why these alleged shortcomings would affect the results. Absent any reason to think otherwise – such as an argument that the content of political advertising in the 75 markets covered by CMAG is systematically distinct from political advertising in other markets – none of these objections undermines the findings in *Buying Time 1998*.

⁴While the decision to forgo formal training has drawn some criticism from Professor Gibson (p. 10), it is likely that a training program would have caused complaints that Professor Goldstein and I were attempting to impose our standards on the coders. Given the alternatives, I felt the first course was preferable, especially since we were hoping for a (reasonably informed) ordinary viewer's impression of the ads. Limited pre-testing of the coding instrument showed that training was unnecessary because coders were apparently able to understand and answer the questions without further explanation. Mr. Seltz and I coded information about the sponsor and type of ad.

II. Replicating the results of *Buying Time 1998*

Professor Gibson reports difficulty reproducing the “specific numbers” reported in *Buying Time 1998*, concluding that this “undermines tremendously any confidence one should place in the findings produced” (p. 5). Professor Gibson is certainly correct to point out that replication is a core precept of science, but he rather overstates the case by insisting on “exact” replication. By its very nature, social scientific concepts are subject to nuances of measurement and model specification that make exact replication difficult, if not impossible, in many instances. For example, Professor Gary King notes in his introduction to a large symposium on replication (to which Professor Gibson contributed an essay) in the September, 1995 issue of *PS*:

As virtually every good methodology text explains, the only way to understand and evaluate an empirical analysis fully is to know the exact process by which the data were generated and the analysis produced. Without adequate documentation, scholars often have trouble replicating their own results months later. Since sufficient information is usually lacking in political science, trying to replicate the results of others, even with their help, is often impossible. (Emphasis in original).⁵

Professor King goes on in the next paragraph to provide examples of 18 questions political scientists might ask when attempting to replicate one another’s results.

This example suggests several problems with Professor Gibson’s argument that *Buying Time 1998* violates important principles of political science and ought to be disregarded as a result. If ability to reproduce precise results is the standard, then it is clear that vast chunks of the literature must also be jettisoned. Most of all, though, Professor King’s comments alert readers that replication is usually difficult to accomplish, and may require time and communication with the authors of the original article. In this case – thanks to the rules of litigation – Professor Gibson had neither much time nor was he able to consult with me. If he

⁵Gary King. 1995. Replication, Replication. *PS* 28: 444.

had, I could have shared the original command files used to produce the numbers in *Buying Time 1998*.⁶ In addition to these hindrances, Professor Gibson also had to work with a large and complex data set, replete with “nuances and peculiarities” (p. 6). Nonetheless, a careful examination of Professor Gibson’s attempt to reproduce various findings of *Buying Time 1998* shows him to be extraordinarily successful, a testimony both to his ability and, I think, to the data set and the report drawn from it.

Professor Gibson’s report notes three discrete instances in which he attempted to replicate specific findings in *Buying Time 1998* on page 24.⁷ How close does he come to the results reported in the volume? Very close, indeed. Professor Gibson reports that the data set includes observations from 76 media markets (p. 24); *Buying Time 98* claims 75.⁸ Professor Gibson reports the number of airings in the data set is 307,028 (p. 24); *Buying Time 98* discusses 302,860. Professor Gibson reports 21,926 ads sponsored by interest groups in the data set (p. 24); *Buying Time 98* claims 22,151. In short, all of these numbers are virtually identical (or identical) to those in *Buying Time 1998*. More importantly, not only are these differences statistically insignificant in a data set with more than 300,000 cases, but none lend themselves to

⁶I spot-checked an assortment of figures and can confirm that I am able to reproduce the precise numbers in *Buying Time 1998* when using these command files with the data set available at the time.

⁷Professor Gibson also notes the presence of two variables with similar names, WHAT and WHAT4, that do not generate identical results (p. 26). While the documentation in the version of the data set that Professor Gibson received from Professor Goldstein provides no explanation, the CD-rom attached to my affirmative report explains that the former is the appropriate variable to use in replicating *Buying Time 1998*. WHAT4 is an interim variable created from WHAT; in the data set Professor Gibson used, it does not reflect the addition of several issue ads originally coded as missing data.

⁸The question about the number of media markets in the data set stems from the fact that Birmingham, AL is listed both as “BIRMINGHAM” (2955 times) and as “BIRMINGHAM-ANNIS” (86 times). The correct number of markets is 75.

different substantive interpretations. To be sure, Professor Gibson alleges numerous other discrepancies (p. 24), but these are the only examples that he provides.⁹ I know from experience that much of the confusion likely results from different ways of handling missing data, the small percentage of storyboards that were either missing and not coded or the small percentage of items that were not coded.¹⁰

The question, of course, is whether “close” is sufficiently exact to confirm the results reported in *Buying Time 1998*. There is no doubt that in the social sciences, where laboratory replication is impossible, the prevailing standards lean heavily toward the ability to reproduce the essential nature of the results, not numbers. For example, Professor King asserts that “(f)uture scholars, with only your publication and other information you provide, ought to be able to start from the real world and arrive at the same substantive conclusions.”¹¹ The data set with which Professor Gibson and I have both worked is maddeningly complex and he uses other coding items and algorithms to argue for completely different conclusions. But to the extent that he has attempted to reproduce the results of *Buying Time 1998* in his report, his efforts confirm the substantive findings contained in the volume and come tantalizingly close to confirming the exact numbers.

⁹I do not count Professor Gibson’s discussion of replicating *Buying Time 1998*’s calculation of BCRA’s impact on pure issue ads because this analysis, as I explain below (see section IV), cannot be replicated by depending solely on the machine-readable data in the data set.

¹⁰Furthermore, it appears that Professor Gibson worked with a slightly different version of the data set than that used to create *Buying Time 1998*. See fn. 6.

¹¹Gary King. 1995. Replication, Replication. *PS* 28: 444.

III. The purpose of ads (q6) v. their emphasis on policy or personal factors (q22)

A main thrust of Professor Gibson's comments is his contention that another item on the coding instrument, q22 in 1998, is a better indicator of "pure" issue ads than the one used in *Buying Time 1998*. If true, this is a potentially major problem for the analysis of BCRA's effect in both volumes makes extensive use of a different variable, q6. cursory examination of Professor Gibson's claims, however, reveal both conceptual and empirical problems with his argument and confirm the validity of the approach taken in *Buying Time 1998*.

The two items in question are:

Q6: "In your opinion, is the purpose of this ad to provide information about or urge action on a bill or issue, or is it to generate support or opposition for a **particular candidate?**" (emphasis in original).

Q22: "In your judgement, is the primary focus of this ad on the personal characteristics of either candidate or on policy matters?"

The first is a "forced-pair" item, giving coders the choice of the options in the question plus "unsure/unclear." The second asks them to choose between the options in the question, or "both" or "neither." Professor Gibson argues that the second is the superior question both for its use of the word "primary" and for giving coders a wider set of answers to choose from (p. 31-4).¹²

Putting aside this critique, the most important question about these items is whether they measure the relevant distinction between candidate-oriented and pure issue ads? The matter of

¹²Professor Gibson's faith in q22 is ironic since it is clear that coders' responses to this item exhibit a great deal of positivity bias, that is, coders showed a marked tendency to characterize ads as pertaining to policy regardless of the actual topics raised in an ad. This is evident when examining the questions immediately following q22 which asked coders to list up to four themes for each ad from a list that included seven described as "personal characteristics of the candidate(s)" (like their background or integrity) and another 48 described as "policy issues" (like social security or taxes). Even ads for which coders listed only themes related to the personal characteristics of the candidates – and no policy issues at all – still generated mixed responses on q22 with 20 percent of these commercials scored as being primarily focused on policy issues and another 12 percent coded as referring to both personal and policy issues.

whether an ad is a campaign ad or not would, on its face, seem to go to purpose of the commercial. Campaign ads are intended to help candidates win election; intention is the essence of the definition of "electioneering" and certainly the essence of the commercials that candidates air.¹³ Thus, one of the important pieces of supporting evidence for the validity of q6 is that coders rated 99 percent of candidate ads (and 93 percent of party ads) as generating support or opposition for a candidate (*Buying Time 1998*, p. 41).¹⁴ My affirmative report provides a series of other results showing that issue ads rated by the coders as electioneering in their purpose were quite similar to candidate ads.¹⁵

Whether an ad focused primarily on personal characteristics of a candidate or on policy matters is a different issue entirely. Most importantly, it is not clear why the focus of an ad demarcates candidate-oriented issue ads from pure issue ads. While some observers may particularly decry the emphasis on personal characteristics in campaigns – especially in attack advertising – this style of campaigning has not, at least to my knowledge, been equated with the practice of electioneering. Indeed, political scientists routinely take the view that politicians

¹³This, of course, is one reason for a result that Professor Gibson finds suspicious, that 97.7 percent of group ads coded as electioneering mention candidates (p. 30, also p. 17). Given their goal of helping candidates, it would be surprising to discover that electioneering ads do not identify candidates. Professor Gibson expresses concern that these items are cognitively connected so that when coders see an ad mentioning someone who appears to be a candidate (they would not be able to identify specific contenders in most races) they assume that the ad must be electioneering. This theory, however, ignores the fact that candidate mentions are coded *after* the purpose of the ad, and that coders did score a number of ads that mentioned actual and apparent candidates in reasonably neutral ways as genuine issue advocacy.

¹⁴This is particularly relevant since coders were not responsible for determining whether an ad came from a candidate, party or group, and because the disclaimers were often difficult to read on their copies of the storyboards.

¹⁵See Jonathan S. Krasno and Frank J. Sorauf, "Evaluating the Bipartisan Campaign Reform Act (BCRA)," p. 55-58. From here on, this is referred to as Krasno and Sorauf.

frequently adopt and advertise policy positions in order to appeal to voters.¹⁶ Thus, there seems little reason to imagine that campaign ads are characterized by an emphasis on personal characteristics and issue ads with an emphasis on policy.

The best illustration of this point appears in *Buying Time 1998* (p. 42): coders rated 11 percent of candidate ads as focused on the personal characteristics of the candidates, 64 percent as policy-related, and the remaining 25 percent as neither or both. If one assumes, as both common sense and the FECA indicate, that candidates are wholly motivated by their desire to win election, then the problem with using q22 as Professor Gibson would use it becomes obvious: it miscategorizes at least two thirds of candidate ads as not being electioneering. This is the same criticism that both editions of *Buying Time* level at the magic words test, that it does not work for the one group of ads whose purpose and category are already known, regardless of their language or style. Professor Gibson's approach is empirically contradicted by the data themselves.

IV. Measuring BCRA's impact

Professor Gibson's most serious claims involve *Buying Time 1998*'s estimate of the effect of BCRA. He makes several assertions. First, he argues that the approach taken in *Buying Time 1998* has "no theoretical meaning" (p. 38). Second, he adopts a different approach to produce estimates of BCRA's effect on pure issue ads that are orders of magnitude higher than any previous estimate. Neither his theoretical argument nor his computations withstand scrutiny.

The sponsors of BCRA acted under the assumption that there are two kinds of issue ads:

¹⁶For instance, see Anthony Downs, *An Economic Theory of Democracy*, (New York: Harper and Row), 1957.

candidate-oriented and pure issue ads.¹⁷ The first are those at which the legislation is aimed, the campaign ads that attempt to pass themselves off as nonpartisan discussions of policy. The second are the ads that everyone agrees ought not be materially affected by campaign finance laws, the sincere attempts by citizens and groups to address the issues of the day using the mass media.

Given this division, two obvious questions emerge: what is BCRA's impact on candidate-oriented issue ads, and what is BCRA's impact on pure issue ads. The first goes to the effectiveness of the law, while the second asks whether it inadvertently sweeps many pure issue ads into FECA's regulatory structure. Both are obviously important questions. There is little need for a bill that does not fulfill its own intention of treating a great portion of candidate-oriented issue ads like other electioneering by candidates, parties, and groups.¹⁸ But it is really the second question that goes to the heart of the main political and legal questions surrounding BCRA. It is widely understood that a bill that treats too many pure issue ads as campaign ads risks rejection by legislators and jurists for being an infringement on political speech. The strategy that BCRA's sponsors adopted uses both the timing of ads and their identification of federal candidates to try to minimize its impact on pure issue ads. Does this strategy work?

Buying Time 1998 addressed that question by calculating the percentage of pure issue ads

¹⁷The first are referred to as "sham" issue ads in *Buying Time 1998* and as "electioneering" issue ads in the work of Professor David Magleby. The second are called "genuine" issue ads in *Buying Time 1998*.

¹⁸My affirmative report shows that 58 percent of candidate-oriented issue ads in 1998 and 67 percent in 2000 identified federal candidates and appeared within 60 days prior to the general election (Krasno and Sorauf, p. 60). These figures, of course, can only grow when the pre-primary period is taken into account.

affected by BCRA out of the universe of all pure issue ads that appeared in 1998.¹⁹ In other words, this figure is the result of simple division whereby the numerator (the number of pure issue ads sponsored by interest groups that identified federal candidates and appeared in the last 60 days of the campaign) is divided by the denominator (the number of pure issue ads sponsored by interest groups that appeared in 1998).²⁰ Due to lack of time, Mr. Seltz and I did not factor in the impact of the additional 30-day period before the primaries.²¹ In my report for this litigation I confirmed that the pre-primary period has no effect on this calculation of BCRA's effect in 1998.²² In short, had BCRA been in effect in 1998, 6 percent of the pure issue ads aired by interest groups that year would have been subject to FECA's regulation.²³ Conversely, 94 percent of these ads would have been unaffected.

Professor Gibson raises different sorts objections to both the numerator and denominator in *Buying Time 1998*, and produces a rather dizzying array of estimates of BCRA's impact on pure issue advocacy. The effect of his discussion, whether by design or not, is to create the mistaken impression that the data lend themselves to practically any estimate of BCRA's effect, including the admittedly unacceptable notion that more than four of every ten pure issue ads

¹⁹This calculation included pure issue ads aired by parties. Parties have been removed from the analysis and several small errors have been corrected in the description of this calculation reported in the Appendix to Krasno and Sorauf.

²⁰From this point on, I confine my discussion to issue advocacy by groups only.

²¹As I note below, determining whether a pure issue ad identified a candidate requires inspection of the storyboards, not the data set, making it much easier to consider the smaller set of pure issue ads that appeared within 60 days of the general election than the larger group that aired throughout the year.

²²No additional pure issue ads that mentioned a federal candidate appeared within 30 days of a primary, leaving both the numerator and denominator unchanged.

²³This slightly adjusted figure comes from the Appendix to Krasno and Sorauf.

would be touched by the legislation. Both of these conclusions are entirely unfounded. In fact, close examination of Professor Gibson's analysis reiterates that, with proper understanding of the data, BCRA's effect can be measured with precision and certainty, and also reveals conceptual and computational errors in Professor Gibson's calculations that enormously inflate his estimates.

The simplest way to understand this array of estimates is to separate the discussion of Professor Gibson's calculations into two parts, looking first at the dispute over the appropriate denominator before turning to the more mundane question of how to compute the numerator. Professor Gibson's criticism of the denominator of the equation used in *Buying Time 1998* is theoretical in nature, claiming that it relates to a "virtually meaningless question" (p. 39). His complaint involves the timing of the ads included, asking "(w)hy not include ads from December 1997, or even the entire election cycle beginning in November 1996" (p. 38). Professor Gibson is correct to suggest the universe of pure issue ads that could be affected by BCRA in 1998 includes all of these commercials that appeared between elections. In fact, my affirmative report notes that for presidents and senators the appropriate period is even longer, corresponding with the four- and six-year terms of these officials. This use of the calendar is a key component of BCRA's design to make certain that only very few pure issue ads are touched by the legislation.

The answer to Professor Gibson's question, of course, is that we had no data from 1997 or the last weeks of 1996 to include in the denominator. That does not mean that this calculation – the percentage of pure issue ads affected by BCRA – should be abandoned, but the effect of any omitted ads merits consideration. Adding missing ads from 1996 or 1997 would only increase the size of the denominator, leaving the numerator unchanged since none these ads could have appeared near an election. This, in turn, would decrease the percentage of pure issue

ads affected by BCRA. The figure reported in *Buying Time 1998* thus should be understood as the upper bound of BCRA's impact on pure issue advocacy. Put another way, BCRA would have affected at most 6-7 percent of pure issue ads that appeared during the 1997-8 election cycle.²⁴

Professor Gibson's alternative is to use a different denominator, the number of all issue ads sponsored by groups that appear in the last 60 days and mention a candidate. This figure essentially comprises the universe of issue ads affected by BCRA (at least in the last 60 days) and thus consists of both pure issue ads and candidate-oriented issue ads.²⁵ This choice of denominators has several empirical consequences. Most important, its size varies considerably with the amount of candidate-oriented issue advertising before an election. This is particularly relevant because of the volume of candidate-oriented issue ads devoted to presidential campaigns. The result, of course, is highly unstable estimates of BCRA's impact from year to year.

The matter of which is the appropriate denominator, however, is driven by the question to be answered, not the results. Professor Gibson characterizes his choice as responding to the following question:

I will assume that all ads aired within 60 days of an election and which depict a candidate for public office have a particular characteristic (i.e., they are engaged in electioneering). What percentage of time would this assumption result in an error in the sense that the assumed characteristic is not the same as the actual characteristic? (p. 38-39)

Put perhaps more simply, Professor Gibson gauges the percentage of issue ads affected by

²⁴The 7 percent figure comes from *Buying Time 1998*; the 6 percent figure is the result of a careful reestimation described in the Appendix to Krasno and Sorauf that takes advantage of some additional information revealed through this litigation.

²⁵*Buying Time 2000* takes the same approach and the resulting estimates are discussed in Krasno and Sorauf.

BCRA in the 60-day window that are genuine issue ads, the sort of ads BCRA is not supposed to affect.²⁶ This is a strikingly different question than the one answered by *Buying Time 1998* – what percentage of pure issue ads aired would be affected by BCRA? – and asked, I believe, by the legislators who considered the bill.

As the Appendix to my affirmative report shows, this dispute over the appropriate denominator accounts for estimates of BCRA's impact in 1998 that range from 6.1 percent (using the formula from *Buying Time 1998*) to 14.7 percent (Professor Gibson's approach). This latter number is well short of the estimate he produces, 44.4 percent (p. 39), and smaller than the even smallest in his wide range of estimates (p. 37-43). Indeed, Professor Gibson himself notes "(t)here is quite some distance between a figure of 7 %, reported in *Buying Time 1998*, and my calculation of 44.4 %" and asks how this has come about (p. 40). The denominator, with all of its theoretical significance, accounts for a relatively small portion of this discrepancy; the bigger part is attributable to the numerator itself.

In every calculation that Professor Gibson and I present, the numerator is the same: the number of pure issue ads aired in the final 60 days of the 1998 campaign that mentioned a federal candidate. Professor Gibson notes his preference for using q22 rather than q6 to demarcate these ads (see section III), but for the sake of argument he adopts the approach used by *Buying Time*

²⁶In my affirmative report I characterize this mainly as measure of regulatory efficiency, and a highly unstable one at that. A hypothetical example illustrates the problem. Imagine a single media market where no candidate-oriented issue ads appear (perhaps due to a lack of competitive elections). If just a single genuine issue ad is affected by BCRA in this market, Professor Gibson's calculation would yield the result that 100 percent of the ads affected by BCRA are genuine issue ads – even if thousands of other genuine issue ads appeared during the course of the election cycle without being touched by this law. By contrast, the approach in *Buying Time 1998* would use the number of pure issue ads that appeared in that market throughout the available time period to gauge the percentage of total issue ads affected by BCRA.

1998. Thus, the fact that he comes up with a numerator that is four times larger than *Buying Time 1998*'s is not a matter of theoretical dispute but one of simple computation. How does one count the number of pure issue ads that mention candidates in the last 60 days?

This straightforward question is complicated by one factor: information about candidate identification was not coded and is not contained in the data sets with which either of us has worked. Coders were instructed to skip ahead 13 questions if they rated a storyboard as providing information or urging action on a policy matter in order to avoid confusion when confronted with a series of questions about candidates. That leaves it to the analyst to consult the storyboards themselves to see if a candidate is mentioned, an easy if somewhat tedious process. This is precisely what I did when preparing *Buying Time 1998*. Apparently, coders did incorrectly complete items about candidate mentions for at least one pure issue ad – “HMO said no” – so that all 2808 of its airings are listed in the data set as mentioning candidates. Knowing that there should be no data on this item for these storyboards, I never checked the data set for this information and therefore never corrected this miscode, allowing it to survive until Professor Gibson discovered it.

This is an especially important oversight because the spot in question, “HMO said no,” is a “cookie-cutter” ad that aired in thirteen media markets. The commercial, about S(enate Bill)2330, complained that “Republicans in Washington are pushing an empty HMO proposal” and urged viewers to call a named senator to vote against the bill. I quickly determined that Republican senators were running for election in 1998 in just four of the markets in which “HMO said no” appeared, and thus reached the decision to treat airings in those markets as mentioning candidates and to treat the other nine markets as airings where a candidate was not identified since no Republican senators were on the ballot. This logic was well-supported by the

facts at hand at the time, especially the fact that the two storyboards of the ad that we possessed each featured a Republican senator who was not a candidate in 1998, Senators Dan Coats of Indiana (retiring) and Olympia Snowe of Maine (up for reelection in 2000).²⁷ Information subsequently obtained in this litigation reveals that this decision rule was too generous, and that one of the ads we attributed to the numerator actually named the second Republican senator in Pennsylvania who was not up for reelection in 1998. The result, of course, lowers all estimates of BCRA's impact on pure issue ads.²⁸

The miscoding of the items about candidate identification for this single storyboard is certainly a glitch in the data set, but not a glitch that had any impact on the analysis of BCRA in *Buying Time 1998*. It does, however, have a great effect on Professor Gibson's affirmative report for he depends heavily on this miscoding to construct his numerator. That he does so is entirely understandable in the sense that these are data in the data set. On the other hand, Professor Gibson admits on several occasions that he is aware of this error. He writes that "ads coded in Question 6 as 'provide information or urge action' should *not* have been coded on *any of the variables indicating whether candidates were depicted or not*" (p. 29, emphasis in original), and adds an accompanying footnote that some of these storyboards nevertheless are unaccountably coded as depicting candidates (fn. 30). He refers to "various unpublished and undocumented machinations" that led only a portion of the airings of "HMO said no" to find their way into the numerator. He hypothesizes that confusion over this ad stems from the fact that, as a "cookie-cutter" spot, we possessed only one version of the ad (fn. 40), apparently without noticing that

²⁷Commercials mentioning Coats and Snowe account for nearly one fifth of Professor Gibson's numerator.

²⁸For a more detailed discussion see Krasno and Sorauf, Appendix.

this version featured an official (Senator Coats) who was not a candidate in 1998. Professor Gibson, in fact, goes so far as to argue that “it is *important* not to accept the numerator” (p. 41, emphasis in original) used in *Buying Time 1998*, claiming that it has been subject to manipulation by Professor Goldstein. His suspicions, however, do not comport with the fact that Professor Goldstein had no role whatsoever in determining which airings of “HMO said no” mentioned candidates.

In short, Professor Gibson manages, by ignoring the contradictory statements in his own report, to compile a count of the set of pure issue ads affected by BCRA that is dominated by commercials that make no reference to federal candidates. The result is a numerator that is more than four times too large and estimates of BCRA’s impact on pure issue ads that are similarly inflated. So, while Professor Gibson’s estimates that using *Buying Time 1998*’s denominator (with his numerator) reveals that 25.7 percent of pure issue ads aired in 1998 would have been affected by BCRA, the correct number is 6.1 percent. And, while Professor Gibson claims that 44.4 percent of the ads affected by BCRA in the last 60 days are pure issues ads, the correct number is 14.7 percent (see Appendix, Krasno and Sorauf). There are, to be sure, a variety of other statistics presented on pages 35 thru 43 of his report, but these figures appear to be intended to amplify his main – and mistaken – contention that 44.4 percent of the ads affect by BCRA are pure issue ads. In most cases, the interpretation and derivation of these statistics are difficult to understand, as when he divides his estimate of *Buying Time 1998*’s numerator by his preferred numerator (p. 41),²⁹ when he unaccountably introduces a completely new denominator (5064)

²⁹The text implies that Professor Gibson was attempting to calculate the percentage of issue ads affected by BCRA that are pure issue ads using his estimate of *Buying Time 1998*’s numerator (p. 40). He describes denominators in each calculation in nearly identical terms: the “airings that were shown within 60 days of the election and which depicted a candidate” (p. 39) and the “airings shown within 60 days of the election and depicting candidates” (p. 41).

reputedly related to *Buying Time 1998* (p. 42 & 43, fn. 39), and the somewhat tendentious example on p. 43 in which he manages to compute an even larger numerator.

This blizzard of computations, as I note above, creates the false impression that estimating BCRA's impact on pure issue advocacy is an exercise capable of yielding virtually any result. That is clearly wrong. There is no doubt that the underlying data set is unwieldy and complex, but there is also no doubt that a careful and informed analyst can count the number of pure issue ads that would have been affected by BCRA and create from that count a reliable and precise estimate of the legislation's impact. That process, done correctly, shows that just a small fraction of the pure issue ads aired in 1998 would have been subject to FECA's regulation had BCRA been in effect at the time. This is the question that occupied Congress as it crafted and debated the bill, and the data show that it achieved its goal.

However, the two quantities – 6896 in the first case and 3064 in the second – bear no resemblance to one another.