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Book Review: The Federalist Papers and the New Institutionalism. by Bernard Grofman and Donald Wittman, Eds.

Daniel A. Farber

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Does his failure imply that negative rights theories, i.e., theories of the sorts of rights affirmed in the American founding, are simply untenable? Some would say so. I would say instead that the current efforts at establishing negative rights have one very odd feature. There is a strenuous effort to defend Lockean rights, but just as strenuous an effort to avoid Lockean grounds for doing so. We have had Nozick's Kantian theory and now Machan's half-Aristotelian argument, but not a Lockean theory. There are difficulties with Locke's own version of rights theory, I admit, but there was a tradition, much of it in America, which worked at Lockean rights in a Lockean way. I will close with the suggestion that what we need from our rights theorists now is a strenuous effort to think Lockean rights through in a Lockean manner.

**THE FEDERALIST PAPERS AND THE NEW INSTITUTIONALISM.** By Bernard Grofman<sup>1</sup> and Donald Wittman,<sup>2</sup> eds. New York: Agathon Press. 1989. Pp. 296. \$42.00 cloth, \$18.00 paper.

*Daniel A. Farber*<sup>3</sup>

For most readers of this journal, the first question about *The Federalist Papers and the New Institutionalism* is probably, "What on earth is 'The New Institutionalism'?" This is a simple question, which unfortunately has no equally simple answer. The "New Institutionalism" is one of several names for a new school of scholarship about government, which is also known as "rational choice," "social choice," or—most commonly—"public choice." Both the proper name for the school and the exact boundaries of its subject matter are still hotly contested, and already sub-schools have arisen. This is a field very much in flux, and therefore difficult to define.

Still, at least a tentative definition is necessary in order to proceed. One of the editors of the book defines the field as including "analyses using tools derived from microeconomics, game theory, and social choice to the effect of decision-making rules and institutional structure on outcomes." More simply, James Buchanan (who won the Nobel prize in economics for his work in the area) defines public choice as "the application of the theoretical method and techniques of modern economics to the study of political

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1. Professor of Political Science, University of California, Irvine.
  2. Professor of Economics, University of California, Santa Cruz.
  3. Henry J. Fletcher Professor of Law, University of Minnesota.

processes.”<sup>4</sup>

Political scientists are understandably a bit unhappy with Buchanan's definition, since it gives another discipline primary jurisdiction over an important piece of intellectual territory. As they are quick to point out, much of the significant work in the field has been done from the start by political scientists; economists hold no patent on game theory and other mathematical techniques. From a law professor's point of view, however, public choice looks very much like a branch of “law and economics.” Both use equations and fancy graphs to describe a world of perfectly rational utility-maximizers.<sup>5</sup> At least heuristically, public choice can be usefully defined as the use of the economist's techniques to study the political scientist's problems.

Teachers of constitutional law tend not to be favorably inclined toward “Law and Economics,” and some may feel a reflexive hostility at the idea that their own field is being invaded by economists. In part, this reaction may be attributable to the happenstance that constitutional scholars are generally more liberal than those in law and economics.<sup>6</sup> Moreover, many familiar concerns about economic analysis such as its unrealistic assumption of complete rationality, apply at least as strongly to public choice.<sup>7</sup> But while the impulse to reject public choice out of hand is understandable, it is also mistaken. As structural issues of various kinds—particularly regarding the separation of powers—have grown more central to constitutional law, the need for a better understanding of the political process has increased. We should not dismiss any possible source of illumination without serious consideration, and even at this relatively early stage of its development, public choice does have some interesting insights.

Your next question, most likely, is how public choice relates to the *Federalist* papers. The answer is that there is more relationship than you'd expect to find between the writings of two eighteenth century politicians and the latest mathematical models of politics.

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4. Brennan & Buchanan, *Is Public Choice Immoral? The Case for the “Nobel” Lie*, 74 VA. L. REV. 179 (1988). This article is part of a symposium on public choice, which provides a good introduction to the subject for legal readers. A more systematic introduction is provided by D. FARBER & P. FRICKEY, *LAW AND PUBLIC CHOICE: A CRITICAL INTRODUCTION* (forthcoming, University of Chicago Press, 1991). Those seeking a more technical treatment should consult D. MUELLER, *PUBLIC CHOICE II* (1989).

5. Fortunately for potential law school readers, graphs and equations are used sparingly in this book; most of it is written in perfectly lucid English.

6. Public choice has sometimes had a strong conservative bent, though the papers in this volume are happily free of ideological overtones.

7. Some of those criticisms are discussed in chapters 1 and 2 of D. FARBER & P. FRICKEY, *supra*, note 4.

Like the modern public choice theorists, Publius often analyzed behavior as the result of calculated self-interest. Like Publius, public choice theorists conclude that procedure and institutional design can be critical in controlling the behavior of these self-interested individuals. In particular, Madisonian devices such as bicameralism are supported by public choice theorists as mechanisms for increasing legislative stability.<sup>8</sup> So a certain, somewhat surprising resemblance does exist.

On the other hand, the differences are equally substantial, as some of the contributors to this book point out. Madison and Hamilton relied heavily on history as a basis for inductions about government. Public choice scholars, on the other hand, rely almost entirely on formal mathematical models for their theories of government. Despite being less rigorous (or perhaps *because* of their lack of rigor), Madison and Hamilton had a broader and richer concept of political life, in which self-interested calculations were only one element. Scholars who are primarily interested in the *Federalist* will find little here to illuminate their understanding of Madison and Hamilton, while public choice theorists will not find the *Federalist* a particularly rich source for future model-building.

Nevertheless, understanding the connection between the *Federalist* and public choice is useful for several reasons. It may help "public choicers" to see how their model-building relates to the traditional concerns of political science. Putting public choice into this context may also make it easier for political scientists to view public choice as something other than an act of intellectual imperialism by their economist colleagues. It may also make present-day analysts take the *Federalist* more seriously by giving some of its concerns the additional cachet of the latest mathematical methods. Finally, several contributors to the book evidently found re-reading the *Federalist* a useful exercise, helpfully reminding them of those aspects of political life that public choice theory abstracts away. On the whole, then, use of the *Federalist* papers as a thematic device seems justified.

What makes this book valuable, however, is less its overall theme than the generally high quality of the individual contributions. I found three of the papers particularly interesting.<sup>9</sup>

The first is Russell Hardin's "Why a Constitution?"<sup>10</sup> Hardin

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8. This point is explained in the essay by Miller and Hammond.

9. My choice of these three papers is not meant to reflect any invidious judgment about the quality of the other papers. These three just happened to strike my fancy.

10. Hardin may be familiar to many readers as the author of a famous essay, "The Tragedy of the Commons."

points out some of the pitfalls in the familiar concept of the Constitution as a sort of contract. To an economist, a contract is an agreement whereby parties exchange performances over time; the point of having an enforceable contract is to eliminate the temptation for one party to receive the benefits of the contract and then renege. The Constitution is not much like this kind of contract. It does not specify reciprocal transfers of goods between groups. Nor is it subject to enforcement by any external authority. As Hardin says, "[t]here was no general incentive to anyone to enter the agreement in the hope of later cheating and refusing to cooperate, as one might well do with a contract . . . ."

Instead of being best viewed as a contract, Hardin suggests, the Constitution may be more analogous to a "convention"—like driving on the right side of the road. Rather than being exchanges between individuals, conventions function to coordinate their actions, and this is very much the purpose of a constitution. One consequence of Hardin's analysis is to deemphasize consent as a basis for legitimacy. Conventions once in place persist because having some scheme of coordination is in everyone's interest and switching a large group to a new scheme is too difficult.

As Wittman's perceptive introduction notes,<sup>11</sup> many long-term contracts share the traits which Hardin attributes to conventions.<sup>12</sup> Thus, the distinction between contracts and conventions is fuzzier than Hardin suggests. Nevertheless, although the full implications of Hardin's thesis are unclear, it strikes me as potentially fruitful. For instance, it helps explain why, having been adopted by the narrowest of margins, the Constitution soon commanded general support even from its former opponents. The benefits of having *some* convention in place often outweigh the faults of a particular convention, as Hardin points out:

Indeed, one of the most compelling considerations in defense of a particular procedure is merely that it has been in use for a while already. This is often the persuasive force of, say, *Robert's Rules of Order*, which may be invoked to settle debate in some peculiar circumstance. People who strongly disagree on how to proceed typically desist from debate immediately when shown some arcane rule in this tedious book born of long experience.

Once the Constitution went into effect, it was able to profit from what we might call the "Hardin effect."

A different aspect of public choice is highlighted by Steven

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11. This may be an appropriate point to observe that the introductory comments by the editors are uniformly very useful.

12. The Constitution itself might also be usefully considered as being something like a long-term contract. See, *The "Unwritten Constitution" and the U.C.C.*, 6 CONST. COMM. 217 (1989).

Brams's paper, "Are the Two Houses of Congress Really Coequal?"<sup>13</sup> Brams uses a game theory concept, the Banzhaf power value, to assess the relative power of the House and Senate.<sup>14</sup> Somewhat crudely, the Banzhaf value is the relative likelihood that an individual will cast a deciding vote. Individual senators get a Banzhaf index of .00329, so the hundred senators collectively have an index of .32881.<sup>15</sup> With individual indices of .00146, members of the House collectively get a Banzhaf value of .63316.<sup>16</sup> Thus, according to Brams, the House has more collective power than the Senate.<sup>17</sup> He finds some empirical support for this conclusion in statistics showing that a bill which passes the House has a seventy-six percent chance of passing the Senate, while a Senate bill has only a sixty-one percent chance of passing the House.

As Brams himself admits, and as Mark Petracca suggests more forcefully in his response to Brams, this is a pretty crude analysis.<sup>18</sup> A number of other institutional features should be taken into account in the model, such as the powers of committees and of the Speaker of the House, not to mention the leverage created by parliamentary devices such as the filibuster. Moreover, without a better account of why legislation originates in one house or the other, Brams's empirical data doesn't mean much. Petracca also points out that it's not clear what Banzhaf indices mean in this context, and that question needs some further thought.<sup>19</sup> Still, the whole idea of

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13. According to his biographical sketch, Brams is the author of a number of works on game theory and national security, as well as a book with the marvelous title *SUPERIOR BEINGS: IF THEY EXIST, HOW WOULD WE KNOW?*

14. His analysis applies only to measures requiring bicameral approval, and does not consider the Senate's special treaty and confirmation powers.

15. Since the Vice-President can vote to break a tie about as often as any individual senator, presumably his index is about the same. Perhaps his index should be added to the President's, who controls his vote. (Any bill which the Vice President could kill would also be subject to an override proof veto, so it is really only the Vice President's power to vote "yes" that matters.)

16. Another essay by John Chamberlin gives each Justice a power value of .009, about the same as the combined power of three senators. The Court collectively gets an index of .086, while the President gets only .035. Chamberlin himself admits that these figures about the Court probably don't mean a great deal.

17. Brams concludes that the House's advantage is even greater with respect to passing constitutional amendments.

18. Brams has particular difficulty estimating the power of the president relative to Congress. He at first concludes that a president is about as strong as ten senators combined, but much weaker than either house as a whole. A few pages later he backpedals.

19. Apparently, this interpretative difficulty is not limited to Brams's paper:

Both the Shapley and Banzhaf-Coleman indices have been extended to compound games, for which the researcher seeks to compute, for example, the probability that a randomly selected voter in Alaska is pivotal in a presidential election, compared with the probability that a voter from California is pivotal. At this point, however, the relationship of these analyses to game theory grows ever more speculative. The indices reflect no particular game or the modeling of specific sets

trying to measure the relative power of the various branches of government is fascinating,<sup>20</sup> and deserves further refinement.<sup>21</sup>

The third paper relates to the *Federalist's* era rather than its contents. Robert McGuire and Robert Ohsfeldt, in "Public Choice Analysis and the Ratification of the Constitution," reconsider the Beardian thesis that the Constitution is rooted in economic self-interest. Recent historians have been skeptical of Beard's thesis, finding little evidence of an economic pattern in the ratification votes. McGuire and Ohsfeldt are the first, however, to use rigorous statistical techniques to analyze the data.<sup>22</sup> They conclude that economic factors played a dramatic role:

In terms of the magnitude of the effects, for a hypothetical delegate with average values of all other independent variables, owning private securities increases the predicted probability of a yes vote from about .59 to about .84. Being in personal debt, for a hypothetical delegate with average values of all other variables, decreases the predicted probability of a yes vote from about .62 to about .36. Merchant interests increase the predicted probability from about .59 to about .72, with all other independent variables held constant at the sample mean.

If McGuire and Ohsfeldt are right, does that mean that the historians who've studied the same voting records overlooked an

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of individual decisions, and it is difficult, if not impossible, to interpret the differences between the indices in a particular context, or to appreciate their implications, if any, for policy.

P. ORDESHOOK, *GAME THEORY AND POLITICAL THEORY: A INTRODUCTION* 469-70 (1986).

20. Another essay tries to measure another imponderable, the stability of public opinion. One of Publius's concerns was to insulate the government from the fickle winds of public opinion. But in a provocative paper, "Restraining the Whims and Passions of the Public," Benjamin Page and Robert Shapiro conclude that public opinion is remarkably stable. They examine (as nearly as possible) every available survey of American policy preferences from 1935 to 1985, some six thousand questions about public policy in all. With respect to sixty-three percent of the domestic policy issues, they found no significant change at all. Most of the changes that did occur were small (under six percentage points). Page and Shapiro also report that the public relies most heavily on relatively reliable sources of information, rather than on communications by interest groups. In a comment on these findings, Chappell and Keach point out that most of the policy issues studied had been around for a long time and might have reached an equilibrium, while other issues may come and go more quickly. (They also point out the significance of voter myopia: "[T]he overall economic performances of the Carter and the first Reagan administration were quite similar, but the timing of the most satisfactory portions was such that shortsighted voters may have given Reagan an undeserved advantage.") I would like to see a more extended treatment of the evidence relating to stability.

21. Given all of these difficulties, it may be too early even to speculate about implications of these findings for law. Nevertheless, one possible implication relates to statutory interpretation. One way of thinking about statutory interpretation is to focus on the "median" or swing voter, whose assent was needed to get a statute enacted. Brams's finding suggests that the critical legislator is more likely to be found in the House than in the Senate.

22. I won't pretend to attempt an assessment of the validity of their modeling, except to note that their methodology (logit analysis) is commonly used in studies of roll-call votes, and they seem to have included a fairly broad list of variables in the analysis.

obvious pattern? Not necessarily.<sup>23</sup> It's quite possible that both results are right: that is, that economic factors strongly influenced individual voters but did not create clear differences in group votes.

For example, prior historical studies show no clear connection between slaveholding and ratification votes. But McGuire and Ohsfeldt find a strong connection: "For a delegate who personally owned slaves and had average values of all other independent variables, the predicted probability of a yes vote is about .41; for a delegate who owned no slaves and had average values of all other independent variables, the predicted probability of a yes vote is about .75."<sup>24</sup> These results are not necessarily contradictory. Note that McGuire and Ohsfeldt are speaking of the effect of slaveholding on delegates with "average values of all other independent variables." There's no reason to think, however, that slaveholders as a group did have average values of all other variables, and so this finding cannot be directly translated into a statement about the degree of support for the Constitution among flesh-and-blood slaveholders as a group.

All other things being equal, individual slaveholders may have been much more likely to vote against ratification—but all other things weren't equal, because slaveholders as a group were different on average from nonslaveholders. Slaveholders might, for instance, have been more likely to own western land claims, which would have disposed them toward ratification. On balance, for many of them, the pull of their other interests may have outweighed the push of slaveholding. So it could be true both that slaveholding caused individuals to oppose the Constitution, and that there was no clear pattern of opposition among slaveholders as a group.

The slaveholding issue is a particularly interesting one. At least since Garrison called the Constitution a "Covenant with Death and an Agreement with Hell,"<sup>25</sup> there has been debate about whether the framers "sold out" to the slave states. McGuire and Ohsfeldt's results suggest that the Constitution was viewed by the slaveowners themselves as unfavorable to slavery, and that to the extent they supported it at all, they did so for other reasons.

Like the other contributions to this volume, these three papers are not "paradigm shifting" works that will transform the way we think about the Constitution. They don't even have strong immedi-

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23. Although in fact there may have been more of a pattern than those studies revealed, if the case studies discussed in the paper by Eavey and Miller are correct.

24. Oddly, if an individual owned at least one slave, the number of slaves didn't seem to affect his vote on ratification.

25. D. FARBER & S. SHERRY, *A HISTORY OF THE AMERICAN CONSTITUTION* 212 (1989).

ate implications for any current issues of constitutional law. On the other hand, they do have something new and interesting to say, rather than rehashing tedious old debates. That alone makes this volume much more profitable reading than the latest issues of most leading law reviews.