THE "BANZHAF FALLACY" FALLACY

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In a recent paper, Howard Margolis (1982) disputes the conclusion — implied by application of the Banzhaf (or Shapley-Shubik) power index to Presidential elections — that the Electoral College system, and its winner-take-all or unit-rule feature in particular, favors voters in larger states. More precisely, such analyses have shown that the power of individual voters under the Electoral College system (or similar systems) is approximately proportional to the square root of the population of the states in which they vote. Thus a voter in a state with a population of 4 million has --according to these indices --about twice the voting power of a voter in a state with a population of 1 million.

Margolis labels this conclusion the "Banzhaf Fallacy" on the grounds that it is derived from an assumption about individual voting behavior that implies a distribution of aggregate election outcomes wildly at variance from reality. He argues that if the assumption about individual voting behavior is modified in a reasonable way the "Banzhaf effect" evaporates and that individual voting power is then uniform across states of different population.

I review these issues in this note and conclude that the "Banzhaf Fallacy" argument of Margolis is itself fallacious. What Margolis shows is uniform across states (under his more reasonable voting behavior assumption) is the probability that an individual voter will cast a critical vote (i.e. , break what would otherwise be a tie) *within a state* times the electoral vote (or Electoral College voting power) of the state. But this is not the same as the probability that an individual voter will cast a critical vote *in the Presidential election as a whole* (i.e., break what would otherwise be a popular vote tie in his state which in turn breaks what would otherwise be a deadlock — neither candidate winning an absolute majority — in the Electoral College). And this likelihood is not uniform across states; instead — even under the more reasonable voting behavior assumption — it exhibits the "Banzhaf effect" and is essentially proportional to the square root of the population of the voter's state.

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For some time it has been widely believed that one consequence of the winner-take-all feature of the Electoral College is to increase the power of voters and groups in larger states at the expense of those in smaller states.

The development in recent decades of several quantitative measures of voting power-especially those due to Shapley and Shubik and to Banzhaf — has permitted more systematic testing of this conjecture.

The first step has been to evaluate the power of the states in the 5l-state (plus D.C.) weighted majority voting game. One thing the power indices have clearly shown is that, in such games, the voting power of the players — though (non-strictly) monotonically related to voting weight — is by no means simply proportional to voting weight. Significant departures from proportionality are especially likely if the number of players is small and/or if there is a great inequality of voting weights. However, the Electoral College game has relatively many players (51) and no small subset of players has close to a majority (270) of votes, and various types of (necessarilyapproximate or indirect) calculations have shown that the voting power of states (measured by either index) in the Electoral College game is closely proportional to voting weight (i.e. number of electoral votes) , with a small bias in favor of larger states (so that the largest states have about 10% more voting power per electoral vote than the smallest states). Electoral votes are approximately proportional to population (significant disproportion occurring only among the smallest states where the "integrity" and "Senate" factors (cf. Owen, 1975) in electoral vote allocation are most significant) and thus also to voting population. In sum, then, the voting power of states is approximately proportional to their voting populations.

With respect to the Electoral College game, therefore, the traditional conjecture is not borne out on the basis of power indices. But the Electoral College game is a chimera, each state being a mere agent of the popular voting majority within the state. Within each state, we have a simple (unweighted) majority voting game that determines how that state's bloc of electoral votes is to be cast. If there are n_k voters in state k, then obviously (on the basis of common sense and either power index) each voter has link of the voting power within the state. Since each n_k is approximately proportional to the state's voting power P_k in the Electoral College, it would appear at first blush that the voting power (in the full 100,000,000-person Presidential election game) of all voters throughout the country is just about the same, i.e. P_k / n_k is approximately constant over all k. But closer analysis of the properties of the power indices shows that this apparent uniformity of individual voting power does not hold.. A "composition" of games does not lead to a simple composition of power values, and a number of somewhat indirect evaluations (it is impossible, even with the most powerful computer, to calculate power index values directly in games with millions of players) have shown that the power of a voter in state k is (under either index) instead approximately proportional to $\sqrt{n_k}$. (See in particular Riker and Shapley, 1968; Banzhaf, 1968; and Owen, 1975; and cf. Brams and Davis, 1974; and Lake, 1979. Also see Penrose, 1946, for an early study — predating the power indices — with similar implications.) In this way, the power indices rather powerfully confirm the traditional conjecture concerning effects of the Electoral College system.

Let us look at the Banzhaf power index to see how this conclusion is reached. The Banzhaf index is defined in this way. Player i is *critical* for coalition S if: (i) i belongs to S; (ii) S is winning; and (iii) $S - \{i\}$ is not winning. The (absolute) Banzhaf power index for i is equal (or, if normalized, proportional) to the ratio of the number of logically possible coalitions for which i is critical to the number of all logically possible coalitions to which i belongs. In a simple majority voting game, with an odd number n of voters, this is equivalent to the proportion of voting combinations of players other than i that result in a tie, i.e. ,

$$\frac{\binom{n-1}{\frac{(n-1)}{2}}}{2^{n-1}}.$$

It can be seen (by analysis or illustrative calculations) that, as n increases, this ratio decreases but does so more slowly than $1/n$ — more precisely the rate of decrease (rapidly) approaches $1/\sqrt{n}$.

As we saw earlier, if one state has four times the population of another state, it has about four times the voting power of the other state (or just a bit more than that) in the 5l-state Electoral College game. At the same time, we now see the proportion of voting coalitions in the larger state for which any voter in that state is critical is one-half the proportion of voting coalitions in the smaller state for which any voter in that state is critical. To speak a bit loosely, then, a voter in the larger state has half as great a say about four times as much Electoral College voting power compared with a voter in the smaller state, and the large-state voter therefore has on balance twice as much voting power in the full Presidential election game. And, in general, an individual's voting power in the Presidential election game is proportional to the square root of the population of the state in which he votes.

Howard Margolis (1982) has recently disputed this conclusion, labelling it the "Banzhaf Fallacy." Margolis argues that in fact individual voting power in Presidential elections is essentially uniform across states. The purpose of the remainder of this note is to consider Margolis's argument critically. I conclude that the "Banzhaf Fallacy" argument is itself fallacious.

Margolis's argument is based not on the Banzhaf index per se but on a probabilistic interpretation of its definition. Consider an election with two candidates, D and R. Let P_i be the probability that voter i votes for D; and let $q_i = 1 - p_i$ be the probability that voter i votes for R. Implicit in Margolis's paper is the following commonsense definition of voting power: the voting power of an individual voter is the probability that D will win the election if the voter votes for D, but R will win the election if the voter votes for R, i.e. the probability that the voter will cast a critical vote that determines the outcome of the election. (This is what Straffin (1977) , in a characteristically lucid article, calls the *Question of Effect on Outcome*.) Obviously this probability is minute in an election of any size. But the question is whether this minute probability varies from voter to voter in a Presidential election under the Electoral College system according to the populations of states they live in. If, in the full Presidential election game, the probability is essentially uniform for voters in all states, then it may well be that the Banzhaf argument is fallacious.

Clearly, this Question of Effect on Outcome cannot be answered in the absence of some behavioral (presumably probabilistic) model of how voters vote. Margolis argues, essentially correctly, that the Banzhaf argument models voting behavior in the following way: for every voter i, $p_i = q_i = p = 0.5$. Actually, Straffin (1977) shows that the Banzhaf index answers the Question of Effect on Outcome under this theoretically more general but effectively equivalent model (the *Independence Assumption*): each voter i selects P_i independently from the uniform distribution on [0,1]. (Thus the expected is $p_i = 0.5$ for all voters. Indeed, it appears that the Banzhaf index answers the Question of Effect on Outcome if each voter i selects p_i independently from any distribution on [0,1] symmetric about 0.5.)

Margolis then points out, again quite correctly, that this model of individual voting behavior implies a distribution of aggregate election outcomes over time wildly dissimilar from that which we actually observe. For if $p = .5$ and remains constant from election to election, the only source of variation in popular vote percentages is the standard deviation of the binominal distribution, which in percentage terms is negligible for n of any great size — let alone n of 1 million or more. (In elections with 1 million voters, by the normal approximation, the standard deviation in election outcomes would be $\sqrt{2.5/1{,}000{,}000} = 0.0005 = 0.005\%$. As Margolis observes, such an election won with 50.2% of the (two-party) popular vote would be a landslide of fantastic rarity.) Instead, in a sequence of Presidential elections, we see the Democratic (or Republican) percent of the two-party popular voting varying far more than this — a reasonable model for this percentage would be a (more or less) normal distribution with a mean of (about) 50% and a standard deviation of (say) 5%. (Thus it is uite unusual for either party to win more than 60% of the vote and virtually unheard of for a party to win more than 65% of the vote.)

Now the only uniform probabilistic model of individual voting behavior that will produce aggregation variation of such magnitude is one in which p is itself a random variable. (Implicitly, we are assuming that p is being influenced by the essentially random appearance of more or less charismatic candidates, better or worse economic conditions, etc.) If p is normally distributed with a mean of 0.5 and a standard deviation of 0.05, the distribution of aggregate outcomes will be likewise distributed with a mean of 50% and a standard deviation minutely greater than 5% (the minute increment being the negligible variation discussed in the previous paragraph) .

Therefore, Margolis argues, in order to meaningfully estimate an individual's voting power, we must, in effect, average his probable effect on the electoral outcome over a reasonable range of values for p, rather than calculate it at $p = .5$ only. Put otherwise, the question is this: *what is the probability that a given voter's vote will determine whether a candidate is elected or defeated* (in a two candidate contest) *if some value of p is drawn out of* (say) *a normal distribution with a mean of* 0.5 *and a standard deviation of* 0.05 *and all voters vote for the candidate with probability p*? In particular, *in a Presidential election under the Electoral College system, will that probability be the same for all voters or will that probability be a function of the size of the state the voter votes in*?

I believe that this is the question Margolis is addressing. If so, the answer he gives is the first — the probability is the same for all voters. I believe the correct answer is the second — the probability is a function of the size of the state the voter votes in. Indeed, I believe that the probability is given very accurately by the voter's Banzhaf (or Shapley-Shubik) value and is thus proportional to the square root of the size of the state.

Recall that voters in larger states have greater Banzhaf values in the Presidential election game than voters in smaller states because the proportion of voting combinations in which a voter is critical is not inversely related to the number of voters but to the square root of that number. Thus, *if all voting combinations are equally likely* (e.g., if p = .5) , the probability that a voter will be critical (will cast a tie-breaking vote) is inversely related to the square root of the number of voters. But, as Margolis quite correctly points out, *the relationship between the probability of casting a tie-breaking vote and the number of voters changes entirely when p deviates* (even very slightly) *from* 0.5. When p deviates from .5, two distinct things happen. First, for any given number of voters n, the probability that a voter casts a tie-breaking vote decreases, the more so the more p deviates from 0.5. Second, for any given value of p different from 0.5, the probability that a voter casts a tie-breaking vote falls more rapidly as n increases than in the case in which $p = 0.5$. In addition, the more p deviates from 0.5 the more powerful this effect is (cf. Owen and Grofman, 1981). These effects are illustrated in Table 1, which shows for combinations of selected values of p (equal to and greater than .5) and for small values of n the probability that a voter will cast a tie-breaking vote.

The significance of these figures is probably more apparent if they are transformed in the manner displayed in Table 2, where each probability is multiplied by the associated value of n to suggest relative voting power in an Electoral College type of system. (Obviously, the absolute magnitude of these figures is meaningless; what is meaningful is the relative magnitude of any pair.) In the first column ($p = 0.5$) , the "Banzhaf effect" is fully evident, as voting power increases proportionally with $\sqrt{n}$ (approximately — the relationship is essentially exact for larger values of n). But in the second column (p = 0.6), voting power increases more slowly, eventually plateaus, and then begins to decline. In the other columns (larger values of p) , the plateaus occur at smaller values of n and the subsequent decline is more abrupt. Thus Margolis (p. 5) is certainly right to reject the possible objection to his argument that the special case p = 0.5 may be "representative enough of the total expectation of getting a tie that the Banzhaf result remains essentially correct." Indeed, that special case is highly unrepresentative. (It seems a plausible conjecture that, for any p even slightly different from 0.5, voting power such as is displayed in Table 2 will plateau and begin to decline for some large enough n.) In fact, *given any* $p \neq 0.5$, voters in larger states may have not only no greater but actually less voting power than voters in small states (because ties in state popular votes, though terribly unlikely in any case, are far more likely to occur in smaller rather than larger states — in terms of Margolis's analogy, if you are aiming off target, you are far more likely to hit the bullseye with an unreliable rifle than with a reliable one).

So how does the answer to the Question of Effect on Outcome change when we allow p to vary over some range? There is one exact, though partial, answer to this question. This has been provided by Straffin (1977). Straffin asks the Question of Effect on Outcome given the Homogeneity Assumption, i.e., if p is the same for all voters and is selected from the uniform distribution on [0,1]. The *Homogeneity Assumption*, like the Independence Assumption, implies a distribution of aggregate election outcomes over time greatly dissimilar from what we actually observe, but the dissimilarity is opposite in character. Now, instead of the winning party (virtually) never winning by more than a fraction of a percent, the winning party is as likely to win with 95-100% of the vote as to win with 50-55% of the vote. But the Homogeneity Assumption does turn p into a random variable, as Margolis wants, even if its distribution is not what Margolis suggests. But, if Margolis is right that the "Banzhaf effect" virtually disappears when we allow p to vary somewhat, it should disappear totally — or be reversed — if we allow p to vary to the extent entailed by the Homogeneity Assumption. But it does not disappear at all. Straffin, drawing on work by Owen (1972), shows that, under the Homogeneity Assumption, the Question of Effect on Outcome is given by the voter's Shapley-Shubik index value. That is, the probability that a voter's vote will be critical if all voters vote for a party with a probability p and p is drawn from the uniform distribution on [0, 1] is equal to the voter's Shapley-Shubik index value. But while the Banzhaf and Shapley-Shubik indices may evaluate voting power radically differently in certain circumstances, e.g. near-oceanic games, it is widely agreed that they evaluate voting power in Electoral-College types of games virtually the same way (as noted at the outset) .In any case, in Electoral College type of voting games, individual voting power measured by the Shapley-Shubik is proportional to $\sqrt{n_k}$ (Riker and Shapley, 1968), just like Banzhaf voting power.

If Straffin's theorem concerning the Question of Effect on Outcome under the Homogeneity

Assumption is correct, this would strongly suggest that Margolis's argument concerning the “Banzhaf Fallacy” is incorrect. Moreover, if we can see why — despite the considerations discussed earlier for $p \neq 0.5$ — Straffin's theorem is correct, we will also be able to see why the “Banzhaf Fallacy” argument is fallacious.

The seeming paradox is this: if $p = 0.5$ (or if p is extraordinarily close to 0.5), individual voting power is proportional to $\sqrt{n_k}$, but if p deviates at all substantially from 0.5, this relationship changes entirely, so that large-state voters lose their advantage. How then can it be that if we allow p to vary over some range (including 0.5) — let alone uniformly over the unit interval, the likelihood that an individual will cast a critical vote is still proportional to the square root of the population of his state? The illustrative calculations displayed in Table 2 show that over almost all of the range of p values the relative power values actually favor small-unit voters over large-unit voters. This seems to raise not only the question of how the “Banzhaf effect” can hold under the Homogeneity Assumption, but even how voting power could even be constant across states of differing size (as Margolis asserts).

The answer to the second question, fundamentally, is this (which Margolis clearly recognizes): if p is anything other than 0.5 (or something extraordinarily close to 0.5) the likelihood that anyone will cast a critical vote is fantastically small (even compared with the highly unlikely event, given a large electorate, that a critical vote will be cast when $p = 0.5$). It is true that, if p deviates from 0.5, small states voters may be far more likely to cast critical votes than large state voters. But for both the probability is essentially zero (e.g. 10^{-100} or far less). This is evident even in Table 1 for very small values of n, where the last two entries (shown rounded off as .0000) are actually 7.64×10^{-7} and 2.57×10^{-12}. But it is more relevant to consider less extreme values of p but larger (election-sized) values of n: if $p = .51$ and $n = 1{,}000{,}000$, the probability of a tie (of casting a critical vote within the state) is about 10^{-90}; if $p = .55$ and $n = 1{,}000{,}000$, the probability is about 10^{-2185} (Beck, 1975, p. 77).

Thus for large values of n, the only relevant values of p are those very close to 0.5. As p deviates slightly from 0.5, the relative advantage of large-state voters disappears and is then reversed. But at the same time the absolute probability that even small state voters will cast a critical vote falls off rapidly. So, in assessing the overall relative advantage of large-state vs small-state voters (in terms of the likelihood of breaking a tie in the state popular vote), two countervailing effects must be balanced against each other. It so happens that they balance out exactly. That is, *if p is allowed to vary about* 0.5 *by any substantial amount, the probability that a voter will cast a vote that breaks a tie in the state popular vote is simply inversely proportional to the size of the state* (and not to its square root) .This is what Margolis demonstrates in his paper, most particularly by means of the example discussed on p. 8 in the Appendix.

Table 3 displays calculations pertaining to this example. Consider voters in two states, one with $n = 1{,}000{,}000$, the other with $n = 4{,}000{,}000$. If $p = 0.5$, the probability of a tie in the popular vote of a state is $P(X = n/2) \approx \sqrt{2/\pi n}$ (by Stirling’s approximation of n!; cf. Beck, 1975, p. 76) , i.e. , .0007979 in the smaller state and .0003989 in the larger. (Consistent with the “Banzhaf effect” — undisputed in the special case of $p = 0.5$ — the second probability is just half of the first.) Now we examine how these probabilities change as we allow p to deviate from 0.5 in increments of 0.0001. The distance between the expected popular vote and a tied popular vote, expressed in standard deviation units (and shown as A in the Table) is thus increasing in increments of 0.2 for the small state and 0.4 for the large state. The corresponding entries in the small-state and large-state columns show the probability of a tie in the popular vote of the state, expressed as a fraction of the probability of a tie in the special case of $p = 0.5$. (Thus if $p = 0.5005$, the probability of a tie is $.60653 \times .0007979 = .0004840$ in the small state and $.13534 \times .0003989 = .00005399$ in the large state.) In terms of their derivation, these entries are merely heights of the ordinates of the normal curve (excellently approximating the binomial distribution for large n), expressed as a proportion of the maximum ordinate at the mean, at the specified standard deviation distance from the mean. The effects previously discussed are apparent in these columns: as p increases, the probability of a tie falls rapidly but especially rapidly in the large state.

Now suppose all values of p shown are equally likely. (This would be precisely true, of course, under the Homogeneity Assumption, and it would be essentially true if p were normally distributed about 0.5 with a moderately large standard deviation of, say, 0.05.) Then, as Margolis argues, the relative probability of breaking in tie in the large state compared to that in the small state is approximated by:

$$\frac{.0003989}{.0007979} \quad \times \quad \frac{3.63331}{6.76634} = 0.5 \times 0.53697 = 0.26849.$$

That is, the relative probability is inversely proportional to state size (1 to 4), not the square root of state size (1 to 2). (The ratio of sums is slightly greater than 0.5, i.e. ,0.53697, primarily because of the approximation involved in taking 0.0001 increments, rather than finer ones. The fact that the table is chopped off at $p = 0.5020$ also increases the ratio but this effect is negligible. If we took finer increments, the ratio of sums would approach 0.5 exactly because in any such table the k^{th} entry in the large state column appears as the $(2k-1)^{\text{th}}$ entry in small state column. If we integrated over the range of p values, the ratio would be exactly p = 0.5.) This is what Margolis aims to show and does successfully show. If p is allowed to vary about 0.5, the probability that a voter will break a tie in his state's popular vote is inversely proportional to the size of the state. Thus this probability times the state's voting power in the Electoral College (which is approximately proportional to the size of the state) is approximately constant forall voters in all states. On this basis, Margolis concludes that the "Banzhaf effect" is fallacious.

But how can we square this conclusion with the Owen-Straffin theorem that the Shapley-Shubik index answers the Question of Effect on Outcome when p is uniformly distributed over the unit interval? The answer is that the conclusion and theorem are quite compatible, but the conclusion can be easily misinterpreted. To say that *the probability that a voter breaks a tie in his state's popular vote times the state's electoral vote* is constant across states is not to say that *the probability that a voter casts a critical vote in the Presidential election is a whole is constant across states* — but Margolis implicitly assumes this.

The possibility that a voter casts a critical vote in a Presidential election is the probability that the voter breaks what would otherwise be a tie in his state's popular vote times the probability that his state's electoral vote breaks what would otherwise be an Electoral College deadlock (i.e., neither candidate receiving the required majority.) And, just as allowing p to deviate from 0.5 greatly reduces the probability of a tie in the popular vote within the state, *allowing p to deviate from 0.5 greatly reduces the probability of a deadlock in the Electoral College*. Indeed, the effect is considerably greater because aggregation is taking place over some 100 million voters, rather than a few million.

The remaining columns on the right hand side of Table 3 show the magnitude and consequences of this effect. The first of these shows, for each value of p (the probability that an individual voters votes for D) , the probability that a majority of voters in a state vote for D. For purposes of this calculation (made by the normal approximation), I assume that the "average" state has 1 million voters — a plausible though somewhat conservative assumption.

Next we must calculate, for each state probability of casting its electoral votes for D, the probability of a "tie" or deadlock in the Electoral College. There seems to be no straightforward way to do this. My intuition tells me that the probability of tie in a voting body with a given (moderately large) number of units with (not too unequally) weighted votes is somewhat greater than the probability of a tie in an unweighted voting body of the same size. In any case, in the next column I have estimated the probability of an Electoral College deadlock by assuming that the states have equal weight but that there are only 30 of them. The next-to-last column of Table 3 expresses probabilities in the previous column as a fraction of the estimated probability of an Electoral College tie in the special case of $p = 0.5$ (in a manner comparable to the entries on the left side of the table duplicating Margolis's analysis).

What is now clear is that there is a significant probability of an Electoral College deadlock only over a range of p values even more minute than that that produces significant probabilities of a tie in the popular vote within a (moderately sized) state. And in estimating the relative probabilities that a voter in the small state and a voter in the large state *will cast a critical vote in the Presidential election as whole*, we do not take the ratio of the sums of the columns through about p = .5020, as Margolis did. Rather we look only at entries through about p = .5005 (i.e. those that give at least about one chance in a thousand of an Electoral College tie) and these few entries must be weighted according to their relative likelihood in producing such ties (shown in the final column of Table 3).

The result is, instead of producing a ratio of small state vs. large state entries of a little more than one-half (.5369) , we get a weighted ratio of a little less than unity (.9503), approximately the same as if we give all the weight to the special case of $p = 0.5$. (The .9503 figure, like the .5369, figure is biased upwards a bit due to the relatively unrefined approximation. Refining the increments fivefold to .00002 changes the figure only to .9375.)

Therefore, even if we allow p to vary to any degree about 0.5, the "Banzhaf effect" remains evident. The probability that a voter casts a critical vote in the Presidential election as a whole varies from state and is at least approximately proportional to the square root of state population.

In claiming that the "Banzhaf effect" is maintained as p is allowed to vary, we do have (for the example presented, in any case) a 5 to 7% "gap" to account for (i.e., the difference between .9503 — or perhaps .93 with a more refined approximation — and unity). In accounting for this, two considerations should be mentioned, though I do not claim that they necessarily account for the full discrepancy. First, in computing the probabilities in the righthand side of Table 3, certain simplyfying assumptions were made that were somewhat conservative in nature. More exact calculations would probably give still greater weight to the first couple of rows in the table. Second, voting power in the Electoral College is only approximately proportional to voting weight; in general, there is a small bias in favor of larger states. According to Riker and Shapley (1968, p. 209), a more accurate approximation is that the voting power of state k is proportional to $w_k\ /(\sum w\ -\ w_k)$. By this approximation, and if the states depicted in Table 3 had 5 and 20 electoral votes respectively out of a total of 538, the ratio of their voting powers would be about 4.116 to 1, rather than 4 to 1.

Finally, we should note that we have used the model of individual voting behavior suggested by Margolis (uniform p for all voters, but p varies from election to election) and that this model implies a distribution of popular vote election outcomes over time that conforms with forms with reality (if we select appropriate parameters for p). But this model in turn is wildly at variance from reality if we disaggregate particular outcomes to the state level. For the model implies that the popular vote division will be essentially the same in every state and that, if the national popular vote division deviates from 50% by more than a few hundredths of a percentage point, the winning candidate will carry every state. Indeed, it is essentially this implication of the model that preserves the "Banzhaf effect." No doubt it would be possible to come up with some rather complex model of voting behavior, involving some rather ad hoc assumptions about political geography, that would imply the elimination or reversal of the "Banzhaf effect" (essentially by implying close expected popular vote outcomes in small states and lopsided ones in large states). But real-life political and geographical consideration seem to reinforce rather than attenuate the importance of big states in Presidential elections. (In the 1976 election, for example, for every additional 1 million popular votes cast, the state winner's percentage victory margin fell on average by over 1 point; the correlation between state popular votes and winner's victory margin was -.33.) In any case, a major purpose of the Shapley-Shubik and Banzhaf indices is to evaluate characteristics of voting system per se, independent of any particular political-geographical context.

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n	p = .5	p = .6	p = .65	p = .75	p = .9
1	1.0000	1.0000	1.0000	1.0000	1.0000
3	.5000	.4800	.4550	.3750	.1800
5	.3750	.3456	.3105	.2109	.0486