

Political Losers

Kenneth A. Shepsle
Harvard University

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It would be criminal to deliver the inaugural William H. Riker Lecture without making as its main focus something of substantive and theoretical significance, for William Riker was a substantively and theoretically inspiring political scientist, perhaps the most inspiring of the twentieth century. I do not intend to disappoint on this score. But, as one of Bill's early PhD students whose professional and personal life was profoundly influenced by him and the Rochester School he founded, I cannot resist talking first about the man. Indeed, I hope to convey, especially to those who never knew him personally, some of Bill's attributes and qualities - especially his intellectual curiosity, vision, and honesty. Bill Riker was, at one and the same time, a scholar of impeccable classical training and traditional interests *and* the most modern of social scientists. Let me begin somewhere near the beginning.

Riker the Man

William Harrison Riker had, to turn a phrase, a beautiful mind. It was nourished during his childhood and youth of the 1920s and 30s in Iowa, Michigan, and Indiana by loving parents. His father, a well-known book dealer and bookstore owner in the Midwest, taught Bill to read at age three. Bill's intellect was further shaped by his teachers at Depauw University (whom he fondly remembered throughout his life), where Bill received a degree in Economics in 1942. As Bruce Bueno de Mesquita and I wrote in a biographical memoir of Bill prepared for the National Academy of Sciences last year, "Bill's ability to learn, precociously revealed, continued with him until his last breath" (Bueno de Mesquita and Shepsle, 2001, p. 4). On graduating Depauw, and with a war in progress in which Bill was ineligible to fight for medical reasons, he deferred plans to go to the University of Chicago for graduate work in Political Science, participating as best he could in the war effort as a time-and-motion analyst for RCA. In 1944, the academic sands had shifted and it became apparent to Bill that Harvard, not Chicago, was now the place to study politics.

He spent the next four years in Cambridge with his lovely wife, Mary Elizabeth, whom he had married the previous year. (Sadly, M.E. as she was known passed away in mid-March of this year.) Not one to let much grass grow under his feet, he finished his doctoral studies at Harvard in 1948 - surely making the Time-to-Completion gods in Byerly Hall smile. While at Harvard he was exposed to Political Science as it was then and, in many places alas, still is - a philosophically and historically grounded scholarship rarely based on analytical argument or empirical evidence. Two dominating luminaries at Harvard at the time were the political philosophers, William Yandell Elliot and Carl Friedrich, the latter of whom became a nemesis of Bill's throughout his career. While Bill was never very sanguine about his Harvard years - more on this shortly, he did have the good fortune to attach himself to an influential scholar, something of an outlier at Harvard, named Pendleton Herring.

Herring is best known for his book, *The Politics of Democracy*, and for his early leadership of the Social Science Research Council. At Harvard, Herring had leaned against the grain (and

didn't remain on its faculty for very long) by innovating an approach to the study of public administration - what came to be known as the *case study method*. Regarded today as rather old-fashioned, though still prominent in business, public policy, and law schools, in the late 1940s it constituted something rare - a self-consciously methodological approach to politics that emphasized the focused accumulation of specific facts and regularities to shed light on a more general argument. Herring imagined a constant flow of graduate students preparing individual case studies on groups and events in contemporary American politics that would add up to an empirically crafted picture of political life. The flow, unfortunately, never materialized, though the method became one of the standard tools of early postwar political scholarship, and still is common today in all the fields of Political Science. Bill's dissertation was on the Congress of Industrial Organizations (CIO) and an essay drawn from it made it into the distinguished Bobbs-Merrill reprint series (for those of you old enough to remember it!).

At Harvard he had excelled in what was the best place in America to study politics and government (still is!). One of his classmates, Richard L. Park, thought of Bill as an independent-minded and innovative intellect, reporting that his fellow graduate students "thought Bill brilliant and extremely odd," a description eerily reminiscent of Sylvia Nasar's of John Nash. The appearance of oddness derived from Bill's rebelliousness in reaction to a deep sense of dissatisfaction with what Harvard had provided him.

Let me give you a flavor of this in Bill's own words. (These words are drawn from an interview of Bill that I conducted for the APSA Oral History Project on June 4 and 5, 1979 in Rochester, NY. A 150-page transcript of this interview is available from the Special Collections and Archives of the University of Kentucky Library.)

Shepsle: How did you balance an almost case orientation a la Pendleton Herring with an anti-descriptive mentality of Carl Friedrich? Was that a problem for you?

Riker: No, it wasn't a problem because these were little compartments. The way the Harvard system was constructed was you had to pass the test in each field. There was a little compartment consisting of a field and you prepared in that and ultimately passed an exam. There were a few people around who rebelled at that compartmentalization and some of them were more sophisticated than I was. Dave Easton, for example, was willing to fight with both Friedrich and Elliot about matters of this sort...Most everybody else just followed the tradition and did what they had to do to get out. And if you go back and look at what a large number of people who were in my class studied, you find that many of them studied things that they have not subsequently been identified with at all.

Shepsle: Did you see your professors as mostly teachers or were they role models in terms of scholarship for you?

Riker: Well, they were certainly not role models for scholarship. Because they [a pause follows] really weren't scholars, except for Herring who had some clear views, not the view that I have ultimately adopted but it was a clear view of what he was doing. Aside from him they were all quite confused about what they were doing and so I had no clear models. Friedrich simply regarded it as his function to turn out a book every year or every two years or something of that sort. And the problems with the whole subject of Political Science at that time was that... well, here's the kind of thing that bothered me when I looked back on it after a few years. It never even occurred to me that one ought to be writing papers for journals. I don't believe anybody at Harvard wrote papers for journals. It never even occurred to me that one ought to be trying to find out sentences that one might try to verify in some sort of scientific fashion. The idea was to have a clever interpretation of some event

or a clever interpretation of some historical development and not to have a scientific approach to politics; it was totally absent...I had no sense of [Political Science as a research profession]. The profession, in so far as there was one, was a teaching profession, you see. And you were simply getting the credentials for a teaching profession, and what you did as a teacher really was of no interest whatsoever to the people who were teaching you how to be a teacher. Presumably you would go off to Podunk and they would have no interest in what you did at Podunk. And indeed they had no interest in anything except a kind of journalism.. ..You were supposed to write books that would be read by other professors of Political Science and that was about the size of it. These other professors of Political Science would all be guys teaching at Podunk and they would be kind of interested in news about the state of the world. And that's what you were supposed to be producing....

Well, let me get back to Friedrich, because he is the bete noire and at the same time the important figure. It's very strange the impact he had on that generation of graduate students, because he was an extremely vigorous person and insisted upon having a very clear simplified picture of the development of political ideas in the post-Reformation period, and yet he had no idea of how to use that information to understand politics. It was a terribly frustrating thing as I look back on it. Here you have this nice clear picture of lots of things and nothing to do with it....People got out of Harvard without having any sense of doing anything in Political Science. Consequently nobody had a sense that you ought to write journal articles that would develop some sentence or try to explain some sentence. And most of the people in my group did not write journal articles for a long time. I guess the picture that we had was that we should write books. But the subject matter of books was not clarified. They were just supposed to be books. I guess you counted the books you'd written and that was the way that you got ahead in the world. Six books, six new jobs, or something like that.

So this is how Bill Riker found his professional world as he began his climb up the academic ladder in 1948. He had returned to the Midwest to a small college. Much of his time and professional orientation revolved around teaching - teaching many different subjects in a very small department. He appreciated that to get ahead he must write a book, but about what? Again, Bill's words:

Riker: The first book that I wrote...was called *Democracy in the United States* [1953] and was a textbook intended for semester or shorter courses in American politics, in American constitutional structure, and so forth. And that had been written under the influence of Mannheimian notions of social science which I picked up in graduate school. I picked up very little about Political Science as a field in graduate school, almost zero. But, I did pick up Mannheim, I don't know where, certainly never got it in a class, and was very impressed with Mannheim and wrote this textbook which started out with a moral position and then asked what can we do to bring this moral position into reality. And that seemed like a worthwhile exercise at the time. And, I worked on that for about three years, and was quite pleased with the result when I published it. But then I began to think that once you raise the question of what can you do to bring a particular moral position into some sort of effective institutional operation why you also raise the question of whether or not institutions accomplish what they are intended to accomplish. And, that is not a normative question at all. That is just a plain straightforward descriptive question about institutions. And I began to ask myself then whether or not the more or less descriptive sentences which I had uttered in this textbook were in fact true, simply as

descriptive analysis of the way institutions worked. And, I rather reluctantly, but nevertheless certainly, concluded that there was hardly a true sentence in it, which is a disconcerting experience to say the least. And so I began to think seriously about what Political Science was and whether or not it could utter true sentences because that then is the next step, you see. Can you describe things in the real world at all?

Most of us today read *Democracy in the United States* and think it a remarkable textbook. But, as it turns out, most of us who read it read the 1965 edition, rather than the original 1953 edition. In the preface to the second edition, Bill claims that he has left most of the argument as it appeared in the first edition, and is clearly proud of that argument. But, though I have done no textual exegesis or comparison across the editions, I suspect that the events and experiences in Bill's intellectual life during the decade between editions found their way into the revision. Bill discovered logic, particularly the works of Quine (and claims to have worked his way during that period through the first volume of Whitehead and Russell, "a disconcerting, tedious job" as he describes it). Bill discovered Duverger's Law - a discovery exhilarating for illustrating that one could summarize disparate data into a regularity, but disconcerting because of the many counterexamples to it. (Duverger's Law was to be a life-long interest for Bill, and he assembled a fine history of this law in a paper for the *American Political Science Review* years later.) Bill discovered Arrow. Bill discovered von Neumann and Morgenstern. The latter in particular was very significant.

Riker: Von Neumann's book was the one that really turned me on, because it seemed to me that there were some generalizations there that one could look at in nature and see if those generalizations turned out to be true.

Shepsle: As a political scientist, was it an intimidating book to confront?

Riker: Well, I had also at the same time as studying logic [was also] studying linear algebra and I had had enough mathematics in college to take a linear algebra course. I took two of them at Lawrence, and I also took another refresher calculus course and so I have been going back to mathematics that way. And I think that you couldn't do that in a huge university now because you wouldn't get tenure if you played around that way [laughter]. At any rate I didn't find von Neumann particularly intimidating; it was slow reading, of course, but then I suppose it wasn't intended to be any other than slow reading [more laughter]. As a matter of fact von Neumann is very carefully written so that it doesn't require anything except a certain facility with set theory and then linear algebra. And it is not easy but it doesn't require knowledge of operations and so forth that are rather arcane. And so I began. Then I developed the notion of the size principle out of von Neumann and then began to consider what sort of behavioral evidence one might adduce for such a principle.

Riker goes on to report how he struggled with this issue for several years, then secured an invitation to the Center for Advanced Study in the Behavioral Sciences at Stanford in 1960, where he wrote *The Theory of Political Coalitions*, a book that now occupies a place in the pantheon of positive political theory.

In the thirty or so years Bill had remaining, he revolutionized Political Science. It is an amazing accomplishment in retrospect. But I am incredulous of any claim that this was a well thought out plan. The world of Political Science from which Bill came was intellectually primitive. It was a world in which most political scientists were teachers. It was a world in which the elite institutions that produced each succeeding generation of new college teachers of Political Science consisted mainly of gentlemen scholars, commentators on the world, public intellectuals, political historians, and students of political thought. It was a world in which one

wrote books, mainly for other political scientists, about subjects in the contemporary world or of historical fascination, but with no pretense to adding to the accumulation of knowledge. For someone like Bill Riker, it must have been an isolated and lonely experience. Consider the following exchange in my 1979 interview with him:

Shepsle: Well before your arrived out at Stanford at the Center for Advanced Study, were there any other sources of encouragement or discouragement along the way? Was there any response from professional colleagues about your new research [on coalitions]?

Riker: Well, I think that the answer is no...Ken, you have to realize that there wasn't anybody then...I believe I must have been the very first political scientist to read von Neumann's book and surely the very first political scientist to read Arrow's book...Uh, maybe Robert Dahl read that book sooner than I did [laughter]. I don't think Dahl read von Neumann's book, however, so I must have been the very first. So there was nobody to talk to about it.. You never taught in a small college so you don't know what it is like not to have anybody to talk to...At Lawrence the only persons that I had to talk about the subject at all were an economist who has subsequently mostly been a lobbyist in Washington...and an anthropologist who was interested in economic anthropology. And those were the only professional colleagues...that I had to talk about this subject... When I gave a paper at [the Midwest Political Science meetings in Ann Arbor]...I don't think anybody said anything to me about the paper afterwards, one way or the other.

Shepsle: Was this period of germination a luxury in this particular case? Could you have written [*The Theory of Political Coalitions*] in a more heated academic environment of the 60s or 70s?

Riker: No, I don't think so, at least I couldn't, because now such a book would not be undertaken except by somebody who had much more training in formal methods than I had. And for me to do this, I had to sort of feel my way around the formal methods part of it....[T]here was nobody to say, "You don't know enough," and so I just kept on doing it, you see. Now I think that very quickly I would be told I didn't know enough [laughter]. And I wouldn't write the book.

I'd best bring the more biographical bit of this lecture to a conclusion, though I have left my development all the way back in the early 1960s with the audience hanging. Quickly fast-forwarding, having risen through the ranks at Lawrence College, and convinced that no one paid any attention to his newest work, especially at the elite universities, Bill accepted a professorship and the post of department chair at the University of Rochester in 1962. He hired some brilliant young junior faculty members - Richard Niemi, Jerry Kramer, Arthur Goldberg, John Mueller, and created a brand new and highly original Ph.D. program organized around decision theory, game theory, and econometrics - namely, one that was about as far from his Harvard experience as he or we could imagine. As was his gift as a shrewd wheeler and dealer, he did all this innovating in a collegial manner that kept on board more conventional scholars in the Department like Dick Fenno. He then proceeded to admit to this new graduate program a group of failed engineers, failed physicists, and failed mathematicians - Ordeshook, McKelvey, Aranson, Rohde, Fiorina, Aldrich, and me, among many others, while at the same time reaching out beyond Rochester to influence other young scholars with a technical bent of whom John Ferejohn, Norman Schofield, Joe Oppenheimer, and Tom Schwartz are the exemplars. Needless to say to *this* audience, at about that time he joined other distinguished scholars in founding the Public Choice Society. Over the next three decades the Rochester program flourished, both within Political Science and throughout the social sciences. Acknowledgement of Bill's decisive

innovations came in the form of election to the National Academy of Sciences, to the presidency of the Public Choice Society, to the presidency of the American Political Science Association, and appointment to the Rochester Zoning Board of Appeals! Numerous honorary degrees were bestowed upon him as well, the one from Lawrence College (now Lawrence University) of which he was proudest.

Throughout all this institution building and organizational maintenance, Bill never lost sight of the fact that he was a scholar. *The Theory of Political Coalitions*, of course, is a classic and coalition theory is a topic on which he continued to work after he came to Rochester. Another subject that he had begun earlier but on which he continued to make contributions throughout his Rochester career was *federalism*, a subject that today constitutes one of his most durable contributions. He was among the very first political scientists to begin using experiments. His 1961 essay on Arrow foreshadowed many papers and his very successful book, *Liberalism Against Populism*, centered on the paradoxes of social choice. Finally, it should be underscored that his interest in political history never waned, with special attention given to ante-bellum electoral politics, to the politics of the Founding Fathers, and, as his last large project published posthumously, to the ratification campaign of the Constitution.

But the subject on which he spent perhaps the most time in the last decade of his life, a subject that continues today to fascinate scholar and student alike, revolves around his invented concept of *heresthetic*. And it is on this theme that I shall devote the remainder of this lecture.

Riker the Inventor of Heresthetic

I must, unfortunately, engage in a bit more throat clearing. Bill Riker began teaching a graduate seminar on positive political theory in the mid-1960s at Rochester. Indeed, I believe he coined the term to distinguish scientific theory from the normative political theory he associated with Harvard and Carl Friedrich. He was unwilling to concede that only *they* did theory. He taught Downs's *Economic Theory*, Olson's *Collective Action*, Black's median voter theorem, Arrow's *Social Choice*, Buchanan and Tullock's *Calculus of Consent*, and Simon's bounded rationality over the first ten or twelve weeks of the term. He would then query the students about what other things they'd like to read to fill in the remaining weeks. (In those days, when men were men and we walked twenty miles each way to school, both ways uphill, semesters consisted of 15 three-hour seminars.) Thirty-five years ago, when I was a student in that seminar, there just wasn't enough material to fill out a full syllabus. Ah, how times have changed! Terms are shorter, seminars are briefer and, thanks to so many in the Public Choice Society, the list of materials is much, much longer.

In any event, what became evident in all the material that *was* available at the time was the centrality of the possibility (or impossibility) of *equilibrium*. Political events and outcomes were conceived of as the equilibrium of some process, though the game-theoretic foundations of strategic interaction were typically left implicit. In effect, an equilibrium described a *winning* outcome or candidate or event. Positive political theory owed much to Vince Lombardi - winning wasn't everything, it was the only thing - and the faculty and students in the Rochester department were the Green Bay Packers of Political Science. (We even had our own Jerry Kramer - for those of you who know your professional football!) And we understood equilibrium to be, in effect, the game-theoretic *core* of some process - a result that could not be undermined, a winning platform that was king of the hill, a candidate who could prevail against any opponent, a bill that could fend off amendments.

But results from social choice theory had a truly jolting effect on this orientation. These were the pre-folk theorem days when an equilibrium was a rare and beautiful thing. The social choice results were discouraging. They did not hold out much hope of equating winning with equilibrium. The former could surely be specified in any particular circumstance - something or

someone actually *won* - but in many of those same circumstances it was difficult to denote the particular winner as the *equilibrium* of an underlying process, for from social choice theory this rarely existed. This was the content we took from the results of Arrow and others. So it would appear that disequilibrium is the human condition in politics. Riker remarked on this in my 1979 interview with him:

Shepsle: What are the great ideas that have been formulated during your professional life that have changed the way people think about politics?

Riker: Well, I suppose, because I have been thinking about this one more than anything else for the last few years, it seems to me that the things that will have the deepest impact on the science are the ideas of disequilibrium. That fundamentally brewed in [Duncan] Black's work. Black found the [Condorcet] paradox and then said, "What condition can I devise that will prohibit the paradox?" He looked for a sufficient condition to prevent the paradox and he arrived at single-peakedness. And that, of course, is what Downs exploited. That is to say, the median voter idea is the single-peakedness condition... But Black, no sooner had he gotten the median voter [result] then he began to think, "Well, what if there are two dimensions?" And he produced a little book with Newing. And that is the beginning of all the material on disequilibrium. And from that point on, it seems to me that the development pretty much determined that somebody is going to see all the disequilibrium in the world once they begin to look at it in the way that Black and Newing looked at it. So that is why I think Black is really a distinctive figure.

Shepsle: How was that idea, the idea of disequilibrium, a useful idea scientifically?... In fact, most of physical science is predicated on the observation of equilibrium, not disequilibrium.

Riker: Well, the idea of disequilibrium is very close to the notion of the absence of a core. They may be identical in certain circumstances. It would be unfair to identify them perfectly because they come out of different models and so forth. But practically they have very much the same meaning. And given the fact that these two rather different models cover a very large part of the world of human behavior, it seems to me to force us to then start thinking about what sorts of things generate [equilibrium]. The thing that is impressive to me, Ken, is this: We, in this century, have been looking for some kind of equilibrium that is comparable to the equilibrium of tastes that one finds in the market, the price, the notion of a market price. That is the only good equilibrium that we know about in social science. And it is so valuable because it does permit us to predict things. We can just predict a huge amount knowing something about the market equilibrium. And it would certainly be nice to do the same thing in Political Science. But suppose we know from the theory that we are not going to find that equilibrium. Then we are driven to look at the world for less certain equilibrium, a sort of temporary equilibrium, a kind of local equilibrium. And that is what a lot of our science is right now.

Shepsle: I think Duncan Black in fact in an essay wrote something to the effect that there is a thread of harmony running through Economic Science that does not exist in politics.

Riker: It is in the first or second chapter of *The Theory of Committees and Elections*. There is a paragraph to that effect, and he says that Economic Science is based on equilibrium and that economists of the last generation he says are perhaps overly impressed with the natural harmony. And then he goes on to say that that harmony seems to be absent from politics.

This was clearly a subject of enduring interest to Bill Riker. There are really two elements I would like to emphasize. The first, evident in the passage I just quoted, is that Bill had concluded that politics may not reflect "natural harmonies" (as Black put it). I believe this is a view Bill later abandoned, at least partially, in response to two developments. The first development was the non-cooperative revolution in game theory. The renewed interest in the extensive form of a game, the invention of new solution concepts like sub-game perfection and sequential equilibrium, the latter reflecting advances in incorporating incomplete information, were just being felt in both Political Science and Economics. From these developments it became increasingly apparent that the problem was no longer the dearth of equilibria, but their multiplicity. The second development, and perhaps it really amounted to a corollary of the first, was Bill's realization that empirical regularities may indeed be identified with equilibria, but only if the scope of the model is properly enlarged. There may not be a voting equilibrium as *conventionally* defined, for example, but one might emerge if the model is enlarged to take agenda-setting into account, or rules of procedure and other exogenous restrictions on comparisons.

The second feature of Bill's concerns I want to emphasize is his belief that neither models with equilibrium nor those lacking one are sufficiently attentive to the *process* by which outcomes are produced. This is no more apparent than from the fact that many theoretical models begin with a statement something like "Assume a set N of agents, a set A of actions, a set O of outcomes, a set of functions U mapping outcomes to utilities for each of the agents, and a mapping that takes agent choices of actions into the set of outcomes." This is a hell of a place to *start* the analysis! Too many things either are taken as exogenous or are suppressed in reduced-form mappings. The "art of political manipulation," as Bill entitled a book of stories he produced in 1986, entails the maneuverings of political entrepreneurs on all these matters.

- First, politicians seek to influence *who* the relevant set of agents is - the set N should not be taken as given. (In one of the stories in *The Art of Political Manipulation*, Riker tells of a city manager who arranged a gerrymander of the city council districts to ensure that the set N of city councilors would possess a partisan majority favorable to keeping her in her job.)

- Second, they invent *new* actions - the set A is not fixed and exogenous. (Another of Riker's stories describes how nineteenth century Speaker of the House Thomas Brackett Reed redefined the manner in which quorums were counted - something no speaker before him had done - thereby eliminating a common practice, known as the *disappearing quorum*, by which the minority could frustrate majority objectives.)

- Third, politicians *frame* the evaluation of outcomes by others in order to improve the chances of the ones they most desire - they seek by rhetorical and other devices to alter agent preferences (the utility functions in the set U) by changing the interpretation of what is at stake. (In another wonderful story, this one about political maneuvering in the U.S. Senate, Riker tells of how Warren Magnuson (D-WA) transformed the transportation of nerve gas canisters across his state by the Department of Defense - something he wanted to block - into an instance of the executive failing to consult the Senate on a matter of foreign policy. In framing the issue in this way, Magnuson could claim that this was a case of the President denigrating the Senate. He was able to pick up a few decisive votes by this maneuver and ultimately prevailed.)

- Fourth, politicians *re-invent* political processes to give favor to the outcomes they prefer. (Many in the Public Choice Society are aware of another of Bill's

stories depicting how Mike Levine and Charlie Plott, members of this Society, re-packaged alternatives in their flying club - an artful revision of the agenda - in order to achieve the fleet of airplanes they most desired.)

Bill invented the term *heresthetic* to describe these manipulations, whether consisting of novel participation criteria, the innovation of fresh and original actions, the re-framing of conflicts, or the invention of altogether new ways of conducting political business. Clever politicians do not take the political world as they find it. If that world possesses no conventional equilibrium, they engage in search behavior to find a preferred outcome that can defeat the status quo. If that world *does* possess an equilibrium, then by definition there is nothing within the conventional framework to be done. But this does not preclude inventive activity - an endeavor in which the politician finds some new way to accomplish what is blocked by existing ways of doing things.

Political Winners: Playing Heresthetical Defense

Bill had great faith in the politician's savvy, just as he admired the entrepreneurial cunning of businessmen. The theme is Schumpeterian, emphasizing the idea that the politician engages in an act of "creative destruction." If already a winner, the politician maneuvers to destroy the opportunities of his or her opponent to reverse the tables. If a loser, the politician invents new dimensions of political conflict and controversy, or re-frames old dimensions, all in an effort to deny the winner the political basis of his or her present domination. The key is that politicians *want to win*, a theme that goes all the way back to Riker's *The Theory of Political Coalitions*. In Riker words, politicians may be found "continually poking and pushing the world to get the result they want" (Riker, 1986, 142).

The logic of heresthetic - "the art of constructing choice situations so as to be able to manipulate outcomes" (Schofield, 2000) - applies both to winners and losers, as I've just suggested. But Bill Riker, I believe, had a soft spot in his head for "losers." They are the desperate ones; they are the ones whose survival is at stake; they are the ones driven by their despair to seek ways to triumph; they are, therefore, the inventors. Defeat is the mother of invention. In this sense it is losers that provide a political dynamic in public life - innovating and strategizing to become winners, on the one hand, and inducing the incumbent winners to anticipate and deter, on the other. But it is often the case that the present winners are locked into the status quo, their credibility and reputation constraining them to deliver as they have previously promised. This puts the ball in the loser's court, so to speak.

Let me first take up political manipulation by incumbent winners. At a very basic level, winners often don't need to do very much, for they already have claimed the high ground. They mostly play heresthetical defense. The Jeffersonian-Jacksonian coalition of early 19th century America, for instance, defeated and then discredited the Federalist Party, winning six straight presidential elections from 1800 to 1820 as the opposition fractured. (They did lose in 1824 under very peculiar circumstances in which Jackson had both a popular and electoral plurality, but lost in the House of Representatives to the so-called "corrupt bargain" between John Quincy Adams and Henry Clay.) Having secured the winning side of the agrarian expansion/commercial development issue, the Democrats retook the White House in 1828 and managed to hold the opposition at bay, partly by focusing on party-building pursuits in a period in which mass political activity proved profitable, an organizational heresthetic that Aldrich (1995) attributes to Martin Van Buren, Jackson's chief political operative and ultimate successor to the Presidency. The fractured opposition eventually re-formed as the Whig Party that sought time and time again to dislodge the winners, clearly succeeding only by nominating popular generals (William Henry Harrison in 1840 and Zachary Taylor in 1848), and almost once again when Clay's bid fell 10,000 votes short in New York City to a third-party candidate which cost him New York State

and an Electoral College majority in 1844. The dominant Democrats worried incessantly about the potential for their undoing by the slavery issue ("a firebell in the night," Jefferson likened it to), but it was not for another generation that the political stars would be properly aligned for a heresthetical move to split the northern and southern wings of the Democratic Party.

Raising new issues by which to dislodge an entrenched incumbent is not an easy matter, as the long string of successes by Jefferson and his political progeny suggests. Losers have many obstacles to overcome. They may be the victims of the clash of factional ambitions. They may be hampered by uncertainty, not knowing which buttons to push. In a world of diverse interests and imperfect communication, they surely face difficulties in amplifying the salience of new issues, even if they are able to discover the "right" ones. So incumbents may only need to govern competently to ride out any prospective storm the losers try to engineer.

Incumbent winners may also be insulated by electoral arrangements. In a lovely theoretical paper by Cantillon (2001), it is demonstrated that the electoral rule affects the attractiveness to the opposition of trying to introduce new issues - one of the standard heresthetical maneuvers in Riker's tool kit. In a one-dimensional setting under a set of standard spatial conditions, she demonstrates that the plurality rule induces "inside competition" - electoral jousting for pivotal groups on the existing issue dimension. In contrast, the proportional rule, especially with a low electoral threshold, is more favorable to so-called "outside competition" involving new issue dimensions on which opposition politicians seek to pick off parts of the previous winner's voting coalition. Incumbent politicians thus are advantaged by the plurality rule, since the rule itself deters entry by politicians with new issues to peddle.

Cantillon's interesting result complements those of Palfrey (1984) and Greenberg and Shepsle (1987) on *entry deterrence*. Palfrey's model, a standard one-dimensional spatial model of plurality rule, shows that two existing parties - let's loosely call them the *winners* inasmuch as they more or less alternate in office in Downsian fashion - can deter a third-party entrant without too much effort. They merely must avoid converging "too much." With too few votes between them, and too few votes on either flank, there is no place for a third party to enter and win the most votes. Greenberg and Shepsle take a slightly different view. Like Palfrey, theirs is a one-dimensional spatial model with plurality rule. However, they ask what if the objective of a third party is not to win the most votes as is conventionally assumed, but rather to become one of the "winners" as defined above - namely, one of the two *major* parties. This would displace a current major-party incumbent, and position the new party to win a future contest. This was ultimately the objective of the British Labour Party by the end of World War I, rejecting merger overtures from Lloyd George and the Liberal Party after a twenty-year courtship. It was also surely the ambition of the "Gang of Four," high-profile centrists who split from a mid-1970s Labour Party that had been captured by the "loony left." (Iain McLean, in a project directly inspired by Riker's agenda on historically based studies of heresthetic, has written about both of these cases, and many more besides, in his wonderful collection of stories, *Rational Choice & British Politics*.) Entry deterrence in the case where a prospective entrant just wants to finish among the top two is more difficult - indeed, Greenberg and Shepsle establish that there are *always* voter distributions for which it is possible for a third party to enter and achieve success as defined this way.

To sum up, then, plurality rule has the tendency to discourage the use of new issues, encouraging "inside competition" instead. One form of this inside competition can in turn be discouraged by slight maneuverings by the two major parties, straddling but not converging to the median position. So, winners may manage to stay winners just by governing competently under existing competitive circumstances. If, however, the entrant is not deterred by the fact that it can be prevented from winning the most votes *this time*, and is willing to settle for second prize in the

hopes of capturing the first prize next time, then today's winners cannot be so passive and may need to engage in some political manipulation of their own.

When winners must engage in heresthetical maneuvers of their own to counteract those of losers, or to discourage them in advance, these manipulations can take many forms. In the electoral arena a short list includes fiddling with voter eligibility, altering party and candidate qualifications, affecting rules for campaigning (and the financing thereof), invention of new ballot forms (did anyone say "butterfly"?), and gerrymandering. They may, because they are the incumbents, affect who may vote, who may run, how voting is conducted, how votes are counted, how the counts are aggregated, how appeals are handled,...the list is endless. And this just covers Florida in 2000! There are undoubtedly limits, at least in the mature democracies, imposed by constitutional prescription *ex ante* and reputational considerations *ex post*.

Political Losers: The Heresthetical Offensive

If heresthetical maneuvers by incumbent winners are primarily defensive, then those of losers must be much more pro-active; political losers play offense - sometimes grinding out gains an issue at a time, other times relying on a Hail Mary pass. It does seem to me that not enough attention is given to losers. We teach and write political history by focusing on the winners. Indeed, we often assume that the winners themselves actually write the history, thereby underemphasizing the often significant role played by losers. (This piece of folk wisdom may be wrong. John Ferejohn pointed out to me recently that think tanks like Brookings or AEI and enterprises like the Hoover Institution and the Kennedy School of Government serve as home bases for defeated politicians, where *they*, not the winners, write the history!) But it is the loser's motivation to win that sets the tone of a political conflict. As Bill Riker put it (Riker, 1986, 1): "For a person who expects to lose on some decision, the fundamental heresthetical device is to divide the majority with a new alternative, one that he prefers to the alternative previously expected to win. If successful, this maneuver produces a new majority composed of the old minority and the portion of the old majority that likes the new alternative better. Of course, it takes artistic creativity of the highest order to invent precisely the right kind of new alternative." McLean (2001, 21-22) sees things similarly: "Persistent losers...always have an incentive to repackage the issues so that they come together in a way that turns the tables." And, he notes, "on critical occasions - however rare they may be - politics goes seriously multidimensional" (McLean, 2001, 10).

One of the most compelling examples of a sure-fired loser who found a way to "repackage the issues" and create the occasion in which politics went "seriously multidimensional" is the brilliant story of the events surrounding Robert Peel in his successful effort to repeal the Corn Laws in 1845-46. The entire account is given as one of McLean's (2001) wonderful stories of strategic maneuvering in British politics. I commend it highly, and give only the briefest of summaries.

Peel, the Tory prime minister whose party was not keen on freer trade, had two battles to fight. He had to find some way to win majority support for repeal of the Corn Laws in Commons, where his own protectionist-leaning Tories held 367 of the 658 seats (56%). And then he needed to succeed in the House of Lords, where anti-free trade agricultural interests were even more entrenched. He accomplished this by re-defining the issue in several ways, each of which picked up a piece of support. The Whig and Radical opposition needed no inducement, supporting it out of ideological preference and in the hopes of using the issue to batter the government party at the next election. The Irish MPs were brought on board by the promise of famine relief tied to repeal. On the one hand, government revenues were expected to *increase* with a decline in tariff rates allowing more generous financing of relief. On the other hand, prices

for substitute foodstuffs in Ireland, artificially high because of tariff protection and the failed potato crop, would come down with repeal, allowing the relief aid to stretch further.

But this was insufficient in the Commons, much less in the Lords. Peel needed to convince Tory partisans that more than trade was at stake. His main ingredients were the immediate fate of the Queen's government and the long-term prospects of the Tory party. The government would surely fall at the next election if Corn Law repeal failed, bringing in Whigs and Radicals who would not only repeal the Corn Laws but do all kinds of other nasty stuff as well. So a defeat now was no more than putting off the inevitable plus further unpleasantness to boot. Moreover, with Peel discredited, the Tory party would, it was expected, fall into "less reliable" hands - in particular, it would elevate Disraeli who, in some quarters, was thought to be too clever by half. This was sufficient to woo a third of the Tory vote, and thus to forge a 327 to 229 majority for repeal in Commons. Remarkably, two-thirds of the majority party followed Disraeli against repeal and against its own prime minister, against Peel and repeal, so to speak. The auxiliary issues were also sufficient to induce uncompromising loyalty from the Duke of Wellington, leader in Lords and former military hero who, despite his opposition to free trade and his lack of sympathy for the Irish, nevertheless put loyalty to the Queen and the Queen's government first. As McLean (2001, 40) reports, "Once [Wellington] was convinced that the question was not corn but the Queen's government, he never wavered from the self-imposed task of getting the Queen's government's measure through the House of Lords..." He managed to persuade a majority to support repeal by not mentioning a word in favor of free trade. It probably didn't hurt that the repeal package included property tax reductions to compensate landowners for the loss of protection.

On free trade, Peel was a loser who figured out a way to become a winner. He did it by inventing new dimensions, "repackaging issues," and providing at least some of his colleagues with an unconventional interpretation of what was at stake. He used all the arrows in the heresthician's quiver. He *could* do it because the prime minister, in cahoots with the leader in Lords, held all the institutional cards. They could run their maneuver unheeded by procedural roadblocks, something unimaginable in the 19th century House of Representatives (before Speaker Reed), not to speak of the 21st century Senate.

Conclusion

Let me conclude this lecture on an institutional note by making a bit more of this last comment. It helps a great deal for someone to be in an institutional position to implement his or her heresthetic. Even if all the obstacles to *finding* the right heresthetic to do the trick are overcome, it will be useless if the maneuver cannot be implemented. What if the city manager in one of Riker's stories to which I referred earlier were not in a position to induce a partisan gerrymander of city council districts? What if Speaker Reed lacked either authority or support for his novel interpretation of how to count a quorum in the House chamber - another of Bill's stories? What if Senator Magnuson tried his maneuver to block the shipment of nerve gas canisters, requiring a powerful speech on the Senate floor re-framing the issue, in a legislature that radically restricted speeches? What if Mike Levine and Charlie Plott were not in a position to set the agenda for their flying club? What if the powers of the Prime Minister and leader of Lords in mid-19th century Britain did not enable them to bundle the various pieces of what became the Corn Law repeal measure as they did?

Put in a more positive form, my claim is that the prospects for success of strategic manipulation are improved by conducive institutional arrangements. Indeed, institutional arrangements often provide precisely the opportunities that the master heresthician may exploit. This suggests that we separate Riker's heresthetic into two facets, the psychological and the institutional. While so many of Bill's stories in his 1986 collection emphasize the institutional

alongside the psychological, his final work on the ratification campaign for the U.S. Constitution (Riker, 1996), published posthumously, put psychological framing and persuasion at the center of his analysis. (It is not surprising, therefore, that "rhetoric," not "heresthetic," appears in the title of that book.) I am mildly disappointed by his emphasis on the psychological. Surely framing is part of the heresthetical story. I certainly agree that politicians must master the art and science of persuasion. But they must also master the institutional resources at their disposal. The punch line here is that institutional arrangements provide both obstacles to and opportunities for strategic maneuvering (a point made recently in Kedar and Shepsle, 2001).

The heresthetician whom Riker has conceived figures out how to avoid the obstacles and exploit the opportunities of the institutional environment. As McLean (2001, 231) so lyrically puts it, "Once in a while there comes a politician who sees further than the others. Such a politician can see opportunities where others do not..." In forcing us to pay attention to politicians who maneuver around obstacles and exploit their situations, "who see further than the others," and who, in the process, transform themselves from political losers into political winners, Bill Riker reminds us not only that politics is a game in which shrewdness, cunning, sagacity, and resourcefulness are rewarded; it also rewards *vision*. This is the gift not only to see farther down the game tree than anyone else, but also to imagine how it might be transformed into another game, one which the politician wins.

Indeed, as I hope I have established in this lecture, Bill Riker himself is the embodiment of that very same idea of heresthetic, the idea he invented. He possessed shrewdness, cunning, sagacity, resourcefulness, *and vision*. He could "see further than the others"; he saw "opportunities where others [did] not." His rich endowment of personal assets, the ones I honor today in this inaugural lecture carrying his name, produced a profound transformation - in a scholar, a department, and an entire discipline.

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