In the White House, among journalists, and for many political practitioners, the idea of a major “political realignment” that might dramatically reshape the face of American politics remains as tantalizing as ever. But among historians and political scientists, no one needs a weatherman to tell which way the wind is blowing. The long-venerated “realignment” approach to election analysis developed by V. O. Key, Walter Dean Burnham, and others is falling increasingly out of fashion.¹

One reason for this is internal to the theory. No realignment theorist ever defended social astronomy, in the sense of expecting realignments to come and go like comets or solar eclipses. But the theory’s communicants certainly believed their pioneering statistical studies. These indicated that since the 1820s, American politics had moved in an irregular, but broadly cyclical rhythm of roughly one generation. Realignment theorists held that this rhythm originated from the slow decay of existing “party systems” in the face

¹ The Journal of the Historical Society V:4 503
of ongoing social change. As the system fossilized, a backlog of unsolved problems built up, creating pressures for systemic change. Eventually, the new realignment would crystallize in one or two unusually dramatic, highly polarizing, “critical” elections.

With the political parties, most unusually, at daggers drawn in a climate of mounting crisis, the electorate suddenly faced real choices. Vigorous campaigns over issues voters really cared about encouraged significant electoral minorities to form new partisan attachments, either by converting or by flooding back into the political system if they had dropped out. If the new regime enjoyed a modicum of policy success, then the new voting blocs firmed. Since the early mass survey studies of electoral behavior, conducted at the height of both the New Deal and Sigmund Freud’s influence on American culture, appeared to show that partisan identifications, once formed, persisted for deep psychological reasons, the roughly “generational” lifespan of realignments became easy to explain. Most voters stuck with their new party identification, providing the basis for each party’s “normal vote” in subsequent elections. A generation or so later, however, enough would have departed to make the system as a whole ripe for another upheaval.

Since everyone agreed that Franklin D. Roosevelt’s New Deal represented a paradigm case of this “big bang” approach, it seemed reasonable to expect the next one in the late 1960s or early 1970s. When elections in the sixties brought plenty of high drama and polarization, but—despite some widely touted forecasts—no manifest realignment, perplexity set in. The sense of bafflement deepened in subsequent decades, as the Republican Party came to dominate presidential politics and dramatically transformed national policy, while steadily whittling away traditional Democratic edges in party identification, Congressional delegations, and state and local politics—all without ever emerging clearly as the national “majority party.”

Not surprisingly, historians and political scientists began to wonder. Even during the heyday of realignment theory, some analysts had sniffed that it was intrinsically untestable. Anyone who had
read Karl Popper’s famous essay on the Pre-Socratics and scientific progress could see through that charge, but the collapse of the New Deal and the ensuing slide into dealignment created fresh problems. With the party identification of many voters weakening or even vanishing altogether, realignment theory’s traditional royal road to explaining both persistence and change in party systems began to look anachronistic. As political coalitions struggled visibly to reinvent themselves, realignment theory’s focus on the long-term effects of “critical elections” appeared ever more problematic.

Increasingly, skeptics derided the realignment approach as “pre-scientific.” Historians such as Richard L. McCormick, Alan Lichtman, Joel Silbey, and Samuel T. McSeveney reexamined past electoral shifts. At first hesitantly, and then with increasing vigor, they voiced doubts about the whole scheme. Along with many political scientists, they began to explore more “existential” views of elections and party identification. The new approaches emphasized the impact of unanticipated events and the ways political leaders continuously update and refashion issues and campaign appeals. An evolving concept of party identification put far less weight on long-term social–psychological attachments in favor of a cooler, more labile, almost “rational” model in which citizens updated “running tabulations” of parties’ perceived net advantages as party appeals and circumstances changed. Together these new conceptions placed in question the very idea of “critical” elections, since the first suggested that sequences of similar elections were unlikely and the second implied that the effects of even a bang as big as the New Deal should probably peter out fairly quickly.

Recent critics are even more strident. Led by David Mayhew, whose work has been commended by Lichtman in the *Journal of American History*, they assert that the whole notion of realignment should be abandoned. “Elections and their underlying causes are not usefully sortable into generation-long spans . . . a scandal, a fancy, a blunder, a depression, or a world war may come along and swerve voters.” But for Mayhew, disputes about periodization go
only so far. Writing at the apex of the stock market boom and enthusiasm for the New Democrats’ “third way,” he presses a broader claim: that the “master narrative” at the heart of realignment theory “does not come close to working”—specifically, its insistence that “American political history had been a continuing, zero-sum contest between, on the one hand, an acquisitive and domineering business class and, on the other, a chiefly lower-bracket coalition of farmers and laborers bent on curbing those mercantile or capitalist propensities.”

As a consequence, Mayhew wants the very idea of realignment “struck from everyone’s mental agenda,” along with all mention of “cycles, periodicity, party systems, and the System of 1896.” Instead he proposes that historians and political scientists would do better to acknowledge that “American electoral history is a common carrier” of many “stories” in which parties “converge” on policies demanded by the electorate. His “off-the-top” list of possible “stories” he suggests scholars should explore includes several of the utmost political correctness, involving race or war, but also one about political business cycles that he claims posits a “positive sum” “political economy.”

With differentials between the compensation of corporate chief executives and hourly production workers soaring over the last 40 years from 25 to 419 to 1, and every presidential election setting new records for total spending, one might wonder if the dismissal (there is no argument) of realignment theory’s master narrative is not perhaps overhasty. Even those who still confidently profess to be able tell the left and right bank of the Seine apart might also consider whether a postmodern proliferation of “stories” would not be a cure worse than almost any disease.

This paper, however, concentrates on more narrowly factual issues, to defend and restate the realignment approach to American history in an entirely contemporary fashion. Realignment theory emerged from a series of independent works by different authors, who relied on diverse forms of evidence that evolved and, we think
it is fair to say, improved, over time. Critics have thus had an easy
time compiling Sears catalogs of verbal inconsistencies, factual or
interpretative disagreements between authors, and potential anoma-
lies and counterexamples. But the weight of all these objections is
curiously insubstantial, because they skirt a crucial point. The clas-
sical works of realignment theory largely predate the modern de-
velopment of both spatial statistics and methods for analyzing data
in time (“time series”). While regional studies and geography have
taken up the first and economics the second, historians and polit-
ical scientists only infrequently make use of these powerful tools.
With few exceptions, the realignment debate has been conducted
without reference to them.11 Mayhew, for example, considers only
two statistical studies in any detail, and hurries past their obvious
limitations.12

Before pronouncing any last judgments on realignment theory,
it is essential to scrutinize the evidence over time in the light of
the best available statistical practice and consider whether what
look like anomalies or even falsifying instances might really amount
only to outliers. This is the task of the present paper. Using the
data kindly supplied by Walter Dean Burnham, the first part
of our discussion employs the best known, most widely under-
stood modern approach to time series (so-called “Box–Jenkins”
methods) to analyze national-level election data going back to
1816. (So that this paper is as accessible as possible, our statis-
tical results appear in Appendix 1, while the main text concen-
trates on the implications of our findings for American political
history.)13

Unlike Mayhew, as a matter of principle, we would be most re-
luctant to suggest that our results should still any debate that has
gone on as long as this one. But we believe that our evidence, ob-
tained by applying the equivalent in econometrics of the Boeing 767
to Burnham’s unique data, painstakingly compiled from primary
sources, throws important new light on the welter of claims and
counterclaims in the realignment debate.
Our analysis strongly supports the traditional approach of Burnham and other analysts. It yields the classical dates he and other political historians identified. It also rules out plausible rivals put forward by other scholars, even as it highlights some quite novel aspects of certain elections they discuss, notably 1876. And, yes, Virginia, the model certainly demonstrates that there really was a “System of 1896.” We show that the stupendous fall in voting turnout that Burnham identified is no statistical artifact, but a dramatic “level shift” that defines the modern American political system in a startlingly direct way that both history and political science have only begun to acknowledge.\(^{14}\)

The second part of our paper takes up the other major challenge posed by contemporary critics of the notion of “critical elections”: the question of how certain elections can have persisting effects on other elections, without relying on historically implausible notions of party identification or a political cognate of the supernova that created the Crab Nebula. We argue that the logic of Burnham’s famous analysis of the “System of 1896” contains a striking answer to this question which is altogether distinct from appeals to conversion or mobilization experiences derived from hypothetical “big bangs” operating at the level of mass politics.

Essentially, he argued that the System of 1896 evolved as it did to protect business elites from mass political pressure. Employing spatial regression to assess the issues in the famous controversy between him and his critics, we first show that outside the South the fall in voting turnout in individual states in the wake of the 1896 election was strongly related to changes in manufacturing value added per capita in each state between 1889 and 1929. Or, in plain English, that Burnham was right to claim that industrial capitalism was associated with the decline of democratic control of the state.

We then argue that what Burnham really demonstrated here is that the behavior of major investors is a crucial variable in elections and, thus, realignments. This point, which is the central plank of Ferguson’s investment approach to party competition, easily
explains how particular elections appear to have durable effects. What made the 1896 election formative, for example, was not really the election itself, but the fact that it helped secure the power of the giant firms in the leading sectors that were then coming to dominate the economy via the sweeping merger movement of the 1890s. By creating “big business” in a strikingly modern sense almost overnight—the whole episode lasted less than a decade—the “trust movement” and related developments (including the parallel restructuring of American finance, which entailed a vast increase in the size of the largest banks and swept investment bankers to the pinnacle of the new “Money Trust,” plus a massive restructuring of the nation’s railroads by the most famous investment banker of all, J. P. Morgan) dramatically altered the relative costs of political action facing different parts of the community.

These costs fell sharply for a tiny proportion of the population—the “Lords of Creation,” in Frederick Allen’s apt phrase—as big money on a colossal scale suddenly became even easier to raise (there had been plenty of it before, though never so concentrated) and redirect to the most advantageous places in the political system. Lobbying, advertising (with its consequent impact on media at many levels), law, education, and, indeed, the whole structure of careers in America, changed in ways that were equally far reaching, one sided, and durable. So also did the practice of “philanthropy,” which eventually created something akin to a “second party system” completely outside of mainstream politics to subsidize discussions of public policy questions that in other countries were typically the preserve of transparently party-connected think tanks.

“Structural” shifts on this scale, of course, are comparatively rare. They normally reflect long-run changes in the world economy and domestic social organization; they do not derive principally from political factors. But they can be identified by data that are typically far less elusive than historical party identifications. Indeed, as we show, very simple quantitative tests sometimes suffice, for at bottom the reorganization of politics follows—though not “naturally”—
from the reorganization of the economy that is accomplished by waves of great merger movements.

Because it highlights what “existential” accounts of elections miss and emphasizes the importance of history and economics to election analysis, this approach to realignment has major implications for the study of current American politics. Given space constraints, though, we can barely glance at these at the very end of the analysis (which was essentially completed prior to the 2004 election), when we consider the political implications of the gigantic wave of transnational mergers that has convulsed the American economy in recent years.

The scholars who developed the realignment approach were acutely aware of history’s complexity. Both Burnham and Sundquist, for example, repeatedly drew attention to the developmental character of realignments. Burnham, in particular, has also (rightly in our view) emphasized their systemic nature and the complicated ways individual events and political developments feed into each other in time and space to generate novel outcomes and evolve patterns within as well as between party systems. If consistently pursued, this emphasis on “organic wholes” leads inevitably to approaches closer in spirit to biology than to physics or, a fortiori, celestial mechanics.

Quantitative approaches to realignment such as ours, accordingly, have to tread carefully. They can easily find themselves in deep waters, bumping up against the limits of conventional interpretations of statistical methods or even fundamental issues in the methodology and philosophy of science. But these possibilities raise yellow flags, not red lights. Biology has in fact witnessed many successful applications of statistics.

In refining our strategy for approaching realignments, we tried to take account of possibilities that we suspected might be important, but which are not always easily revealed by techniques and software commonly employed by historians and other social scientists. Appendix 1 sets out our full, formal time series model. It is perhaps most conveniently presented here in three parts: Firstly, an explanation of why we proceeded as we did and the data we relied upon;
then our main results; with finally, an effort to achieve “closure” by examining what our model has to say about other dates proposed in the literature.

In the spirit of the admonition that science is “the art of the soluble,” we began by incorporating two facts about American political history that are truly obvious, but which, we note with surprise, recent critics of the realignment perspective have contested. Firstly, that continuous, national, organized party competition clearly developed out of the Jacksonian Revolution. Secondly, that only once in American history did a third party emerge to capture control of the national government and establish itself as a major national contender for power in every election thereafter—in 1860, when, as everyone knows, Lincoln won, triggering the chain reaction that led to the Civil War.

Despite Mayhew’s claim that he has “not seen a serious defense of the idea” that Jackson’s election in 1828 involved realignment, we think that there is no point in pursuing discussions that do not acknowledge, right at the start, that both that era and the Civil War mark clear realignments.18 Yes, the Federalists and the Jeffersonian Democrats also competed in the early years of the Republic. But that episode ran into the sand.19 With the Federalists discredited by the end of the War of 1812, the Democrats basked for some years in the happy position of Mexican elites following their revolution: a tiny oligarchy presiding over a one-party state at the national level, ruling in the name of revolutionary democracy. The sudden emergence of a rival party on a broad, nationwide basis spelled realignment or the term has no meaning, though the precise date of this development (which was really a process, rather than an event) can be debated. (We finally settled on 1824, recognizing that perfectly reasonable people might argue for 1828 or perhaps some other year.)20

From the standpoint of realignment theory, the case of the Republicans’ meteoric ascent is similar. The idea that the GOP was really only the Whigs in disguise or even a born-again Free Soil
Party is simply false, whether one looks at the voting evidence or the investors who put up the money.21 When a new party comes out of nowhere, takes control of the government, decisively alters public policy, wins a war of genuinely mythic proportions, and establishes itself forever after as a contender for power, it is better to acknowledge that the achievement passes realignment theory’s equivalent of the fabled “duck” test and move on.

This brings us to a critical issue, indeed, perhaps the most important one: the question of the data series to start with. Realignment theorists and their critics have analyzed how votes or seats have split at many different levels of the political system—the nation as a whole, across states, counties, regions, etc. They have also looked at many different types of elections—for President, for Congress, or at lower levels. Our view is that, in the end, realignments are important because they mark fundamental shifts in the coalitions that dominate national politics. We thus focus on data for the United States as a whole (in contrast to, say, some region, such as the non-South) for elections that determined control of the national government.

In part, the data series we chose reflects our interest in basic issues of democratic theory and, especially, the enduring questions raised by Burnham’s analysis of the System of 1896. But our major consideration was to avoid as far as possible the “adjustments” to the data sometimes required by local variations and occasional bizarre twists of American electoral history while still analyzing as long a data series as sensibly possible. Thus, we used the Democratic share of the total potential vote in elections for President and, in off-year elections, for the House of Representatives. This allowed us to bypass many arguments about how to count votes in particular elections, such as 1912, and reduced debates about continuity to a bare minimum, since the Democratic Party’s roots go back to the earliest years of the Republic. But it is very important to recognize that this focus on the total potential vote is very different from analyzing the two- or three- or $n$-party split of the actual total vote, for our figure
also reflects the party’s success in attracting (or repelling) nonvoters who were legally eligible to vote. Yet, as Burnham first indicated, and our study confirms, it is uniquely illuminating.

Of course this figure cannot be gleaned from published vote totals; reliable estimates of the total potential electorate are critical. We consider Burnham’s work, based on primary sources, including not only national, but state censuses, to be definitive in this area and we are most grateful to him for making his unpublished data available to us.

Since we were interested in the details of how the Jacksonian Realignment progressed, it made sense for statistical reasons to start counting ahead of that period. We chose 1816, after the War of 1812 was over and the Era of Good Feeling had begun. National party competition was then at its nadir. Presidential elections were essentially uncontested and only some states selected their members of the Electoral College by voting at all. But while the Democrats had sole status as a national party, their share of the national vote in House elections did not (as it did in the Electoral College that met to reelect Monroe in 1820) reach a hundred percent. Since we were trying to represent the outcome of the contest for control of the national government, we used the Democratic percentage of total potential electorate for House elections in place of the party’s percentage of the national presidential vote in the two uncontested presidential races of 1816 and 1820.22

In the exploratory phase we employed what our research suggested was one of the best of the recently developed “automatic” time series programs.23 While these are not foolproof—in the end, we improved substantially on the model ours proposed—they are very helpful in avoiding pitfalls and bringing to light possibilities that can be checked by more conventional methods.

It was these efforts, for example, that first made us wonder whether a one-time, permanent shift in the seasonality of elections (i.e., the see–saw pattern of the vote alternating between on-year and off-year elections) might have occurred around 1876, an election
that has long attracted attention from realignment theorists. It was here, too, that we picked up the signs of a major once-and-for-all downward shift in the level of the Democratic percentage of the total potential vote in 1902.

Since a statistically significant level shift of this kind is exactly what one would predict from Burnham’s discussion of the System of 1896, we were very intrigued. But we also wondered about the six-year gap. Eventually, we realized that 1902 was the first off-year election after Bryan’s second failed bid for the White House and that 1896 was, after all, a relative peak in voter turnout. Its near record turnout meant that the decline in the Democratic voting share that followed did not immediately crash through the limits of the “normal” range of fluctuations characteristic of the premodern American political system. Only with the turn of the century did it become clear that the turnout decline was no sudden downward lurch in a random walk, but a fundamental change in level.

After verifying the 1876 and 1902 results, we started refining the model via traditional time series methods. We were intrigued by evidence that the Democratic share of the total vote was negatively correlated with the vote five elections (ten years) before. Reflecting on Burnham’s remarks about “mid-system” crises and mindful of Skowronek’s suggestions about the fragmentation of presidential coalitions, we examined our results at length, before concluding that the effect merits cautious notice.

The most important question we tackled, however, concerned the New Deal. Historians have proposed all sorts of dates for when it began; and as mentioned earlier, agreement on when it ended has proven elusive. Recent developments in American politics have only heightened our interest in the latter question, since they raise an intriguing possibility. Extrapolating some of Burnham’s remarks about the pivotal role of the System of 1896 in defining the “modern” American political system, we conjectured that perhaps the New Deal could be understood as an interruption in the development of a distinct, business-oriented political system. Now that the New
Deal was history, perhaps American politics is essentially reverting to trend.

In the end, our statistical results were unambiguous: The New Deal is best understood as beginning in 1932, with 1966 marking the last New Deal election.\textsuperscript{24} Though our results tell nothing about who shifted or why it happened, they are consistent with the argument about a late sixties realignment ("dealignment" might almost be better) advanced by Aldrich and Niemi and lately endorsed by Burnham.\textsuperscript{25}

Our results also throw light on the period just prior to the New Deal. The Democratic share of the total vote suffered a sharp "negative shock" between 1918 and 1926. On its face, this short but steep decline in the party's fortunes suggests not realignment, but another blow to a party that had been in long-term decline since the turn of the century. We will return to this subject below, when we consider the System of 1896 in more detail, but one interesting implication can be mentioned here. Together with our findings about the New Deal, it suggests that the famous 1928 bounce in the Democratic vote should perhaps be viewed as more of a rebound from the post-World War I shock than a true harbinger of the earthquakes of the thirties. As is obvious to anyone conversant with the historical facts of Al Smith's relations with the DuPonts and John J. Raskob, the Happy Warrior was no New Dealer, no matter how many superficial electoral analyses suggest otherwise.\textsuperscript{26}

Since, as explained earlier, we rely on other criteria to date the Jacksonian and the Civil War realignments, there was no point in running elaborate tests on them. Instead, we tested for evidence of temporary shocks to the Democratic percentage of the total potential vote. These were easy to identify as occurring in 1824 and 1860, when the Party twice committed binary fission.\textsuperscript{27}

Save perhaps for the late sixties end of the New Deal, our model was now quite conventional. Along with the Jacksonian and Civil War realignments, we had evidence of a major break occurring around 1900 that has a perfectly clear connection to 1896 and of
a substantial upsurge obviously connected with the New Deal that ended in 1968. But these results, of course, begged the key question: What about other possible dates? From a quantitative perspective, there is no real alternative other than to check out individual possibilities one by one. In theory, this could lead to tedious and potentially troublesome definitional quarrels about how much of an improvement in model fit is required to qualify an election or elections as a realignment.

Though only time will tell, in fact this does not seem to be a real danger. We tried every plausible date that the realignment literature suggested to us. We looked hardest at 1876; we also considered different years in the late 1880s and 1890s, along with various dates in the 1840s. It may be that other data sets or other tests would yield different results. But we did not succeed in identifying any dates that promised any real improvement at all. The classical dates for realignment proposed by Burnham and others, we think, hold up well.

But exactly what happened on or around these dates remains mysterious in crucial respects. If, as suggested earlier, party identifications in some epochs are often shallow and subject to revision on a rolling basis and if they also reflect the myriad influences of continuously changing campaigns and accidental events, then it is not clear how one or two elections can bring about sweeping changes in party systems that last for a generation or more. To meet the challenges posed by its critics, realignment theory cannot beg the question of process; it needs a clear account of how realignments occur that is consistent with contemporary research on elections and weaker notions of partisanship.

Not that it needs to accept every fashionable claim about the “contingent” nature of elections, or the open-ended effects of scandals and other surprise events. Every human endeavor, including politics, is affected by accidents, and to our knowledge, no realignment theorist has ever denied this. Realignment theory’s substantial claim is not that accidents and other nonsystematic influences do
not happen but that the ways they affect party systems are conditioned by the historically specific structures of power that realignments establish. Or, as Sundquist remarked about scandals, they normally lead to “deviations” rather than basic changes in trends. While Mayhew discounts his claim, it seems obviously right.29

More detailed historical studies of campaigns, sensitive to finer shadings and “buzz words,” will surely bring to light finer structures of change, but this whole area of research is in its infancy.30 In the meantime, our conclusion is that current discussions of campaign themes are frequently over-subtle and exaggerate changes in appeals as actually perceived by voters. They overlook strong elements of continuity and overestimate the importance of what are usually incremental adjustments in themes (we would like to see a truly compassionate conservative try to run in the current Republican Party).31 We think that American politics witnesses not only realignments, but genuine alignments. As our time series evidence suggests, marginal experiments with “hot” themes from election to election do not preclude major shifts in voting outcomes and do not touch any essential contention of realignment theory.32

We enter no such claim in the case of the newer conception of party identification. If sizeable parts of the electorate flexibly update shallow party identifications on a rolling basis, or if large numbers of voters operate for very long without any or with only very feeble senses of party loyalty, then realignment theory has a problem. Some thing or force has to be identified that is both powerful and durable enough to affect a whole sequence of elections—to provide the party system with the requisite continuity, in other words, while also bringing about fairly rapid transformations at certain times—or the relationships between putative critical elections and party systems risk ending up like the caricature of nineteenth-century American religious revivals, in which a year or so after the preacher has left town, most people are back celebrating demon rum and worshipping the golden calf.
One could respond by rejecting the newer view of partisanship altogether or in major part. Or one might perhaps riposte that big bangs on the scale of the New Deal or perhaps the Jacksonian Revolution, which involved persisting conflicts over major redistributive issues apparent to vast numbers of voters, could make a big enough impact to do the job, especially if assisted by party machinery, periodic campaign oratory, and a great deal of targeted money. Perhaps—indeed, even almost certainly—but the general case in favor of recognizing that “shallow” party identifications have been relatively common, if not necessarily ubiquitous, in American history surely remains very strong. Years ago, for example, one of us observed that the System of 1896 was all but impossible to assimilate to the “big bang” approach, since it required voters to identify enthusiastically with the GOP at the very moment they were dropping out of the political system by the millions. The realignment of the late 1960s makes similar difficulties from the other direction. It is probably best characterized as a “little bang” in which the Democratic share of the total potential vote shifted downward, but changes in traditional party identification were modest indeed—that is why, until Aldrich and Niemi, it was typically assessed by realignment theorists in terms of “dealignment” or some altogether new type of realignment. And while Bartels’s concentration on a variant of the two-party split of the vote instead of an indicator that registers the colossal fall in turnout after 1896 makes it impossible for us to follow his discussion of electoral continuity, it seems obvious that he and Mayhew are right to draw attention to the fairly large numbers of voters in past elections who scattered votes among different parties at different levels of the government in the same election—behavior that is hard to square with “strong” partisan identifications.

Such giddiness also affected party elites—even those celebrated for their almost religious cultivation of the myth of “party,” such as the Bourbon aristocracies of the Old South. They were often surprisingly willing to jump ship when the issue was big enough, as,
for example, when tariff struggles sent Louisiana sugar producers into Teddy Roosevelt’s Progressive Party; or in 1928, when many recoiled from a Democratic nominee who was not only a Catholic and a wet, but the only high-tariff candidate in the party’s history.\footnote{37}

Fortunately, it is not difficult to identify forces that are powerful enough to periodically remake party systems and then to sustain them through one election after another for a generation or more. But to see them it is necessary to cast a fresh eye on the most famous and controversial of all realignment analyses, Burnham’s seminal discussion of the System of 1896.

Both Burnham and his critics, after all, agreed that his argument implies that the final cause of the stunning turnout decline that followed the 1896 election was the desire of American business elites to insulate themselves from democratic pressures. All the sound and fury that this strikingly original suggestion triggered, however, helped obscure the plain fact that the desires and capabilities of business elites have no intrinsic connection with questions of party identification. They are something altogether different, however convenient a screen deeply held party identifications might be for investor blocs at certain times. Including the business community within Burnham’s heavily quantitative argument amounted to the discovery of a new kind of conditioning variable, one that Burnham argued was fundamental to the functioning of the system as a whole.

This is the point that should have emerged from the discussion and should then have been systematically tested. Needless to say, it did not happen. Instead, when they were not deriding him as a conspiracy theorist, Burnham’s early critics focused on the possibility that either ballot fraud or changes in the form of ballots and the conduct of elections could explain away the turnout decline.\footnote{38}

Later critics, such as Mayhew, attack on a wider front. For Mayhew, the 1896 election is a paradigm of why elections have to be approached as unique events, in which issues are continuously re-framed and revalidated. No “critical election,” he insists, could have played the role Burnham ascribes to the election of 1896. \footnote{519}
the problem of contingency. In any reasonably open polity operating in an event-packed world, no election result can program the future like that.”

In contrast to earlier critics, Mayhew acknowledges the steep fall in voting turnout and the withering of party competition all through the South and in much of the rest of the United States. But he scoffs at any notion that either was related to the spread of industrial capitalism. Indeed, he denies that any real argument on this point has ever been offered. At most, he claims, Burnham has offered “intimations” of a causal link between ballot changes and industrial power. “No one has crafted a plausible or coherent argument that those reforms had any causal roots in the 1896 realignment.”

His most striking new claim, though, is that business elites had no reason to worry. “Why should we suppose that the alleged ‘insulation’ came to be needed after 1896?” he asks rhetorically. After reeling off statistics testifying to high rates of real growth between 1871 and 1913 and rising wages in manufacturing between 1889 and 1914, he concludes with a flourish characteristic of the late 1990s that “as of the early twentieth century, the chief mass pressures of relevance to the United States were probably those of Europeans trying to immigrate to enjoy the country’s economy.”

Mayhew’s claims about both the 1896 election and the System of 1896 as a whole are extremely impressionistic, providing a vivid case in point of the dangers inherent in suggestions that proliferating “stories” could possibly be a serious strategy for progress in the social sciences. He is evidently unaware, for example, that between 1880 and 1922, rates of outmigration of workers heading back across the Atlantic to Europe often ran well over 35% and in some years over 50%. Nor does he discuss or even mention the notable historical studies of Populism by Goodwyn and Schwartz. Even a casual reading of these works shows that big business needed all the insulation it could get from angry southern and western farmers during the 1896 election itself. Nor can there be any real doubt that the period after 1896—or the “age of industrial violence,” as
one labor historian suggestively titled a work on the period—saw major challenges to the American capitalist order, making control of the state by big business crucial. Montgomery, Noble, and other scholars, for example, have exhaustively documented how in this period employers sought to introduce cost accounting systems; install formal managerial hierarchies; carry out time and motion studies; institute closer supervision; create formalized job ladders based on standardized evaluation of (often written) credentials; introduce piece work; and destroy the position of craft unions by contracting out, redesigning jobs, or introducing machinery that semi-skilled workers could run.44 Talk about “positive sum” outcomes in such circumstances is mostly cant; and, in any case, the question of how to divide up non-zero sum payoffs could often be as contentious and bitter as any zero sum battle, particularly when high rates of economic growth exhaust the pool of unemployed and wages begin to rise sharply.45

From the late 1870s, when a near-nationwide strike wave led to the first systematic efforts to gather reliable statistics, industrial strife grew fiercer with virtually every swing in the business cycle.46 Industrial conflict exploded during and after World War I, when organized labor’s ranks briefly surged, before being almost destroyed outside of a few crafts and a handful of industries by the combined effects of sudden depression, the Great Red Scare, and the “American Plan” drives for the open shop mounted by the National Association of Manufacturers and allied employer organizations.47 Had Mayhew looked beyond the Rees study’s 1914 cutoff point, he would have seen that most wages fell stupendously just after World War I, generally failing to recover until the New Deal ushered in another party system in which labor itself became a significant investor for the first time in American history.48

The broad shape of elite responses to the challenges from below is as uncomplicated as the evidence about the behavior of strikers. Counts of articles on employees, laboring classes, and related entries in magazines indexed in the Readers Guide for the era rather
strikingly resemble normal curves, with a towering peak centered on 1919–20, when American elites suffered something like a nervous breakdown. In the face of unprecedented labor militancy, capital and parts of the middle classes reacted strongly. A mix of carrots and sticks, including fines, prosecutions, bribes, bargaining concessions, favorable publicity, acceptance of the “principle” of collective bargaining, and in at least one case in Pittsburgh, membership in the exclusive Duquesne Club, was held out to leaders of the handful of unions too well entrenched to be destroyed. A few of the most grievous workplace abuses were palliated, notably the absence of compensation for injuries sustained on the job. Meanwhile public policy tilted massively in favor of employers.49

This was somewhat disguised by the widely repeated bromide that the state normally did not inject itself into disputes between private parties. What this meant in practice, however, was that public policy normally left employers free to attack their workforces with virtually any weapon money could buy. Almost all large firms maintained private armies. Some of these—the Pennsylvania Coal and Iron Police, General Motors’ “Black Legion,” or the notorious goon squads that the Ford Motor Co. recruited—far outmanned local police forces.50 Firms too small to afford their own Freikorps, as well as larger units requiring reinforcements, regularly contracted out to any number of consultants, including the notorious Pinkerton Detective Agency. They also banded together in specially organized trade associations, such as the National Metal Trades Association, which maintained flying squadrons ready to jump on trains to assist members in need. (By the mid-1930s, a Senate Committee later established, thousands of management spies honeycombed the workplaces of America that Mayhew celebrates.51)

When private resources proved insufficient, control of the state became crucial—and who exercised it glaringly obvious. Between 1880 and the New Deal, National Guard intervention in labor disputes was epidemic and virtually always in favor of employers. Between 1880 and 1934, federal troops intervened more than three
dozen times to break strikes and became involved in more than 100 other major incidents, while the federal government seized workplaces (virtually always on behalf of employers) more than a hundred times. Taken as a whole, it is a record that in some respects brooks comparison with American military interventions in the Caribbean during the same period.52

As numerous as they were, such cases marked the exception, rather than the rule. Most labor disputes involved local authorities. Here, the role of courts was crucial, for once they ruled against strikers, the local gendarmerie could act to defend not employers, but the majesty of the law. Not surprisingly, between 1880 and 1930 the wrecking of strikes and union organizing campaigns by means of court injunctions developed into a fine art. The statistical series presented in an old study of *The Government and Labor Disputes* shows a steep rise in injunctions until 1920, when the values thereafter could be mistaken for a power series.53

Where injunctions proved insufficient or unhelpful, an increasingly imperial judiciary provided other weapons. Perhaps the most revealing quantitative assessment of this development appeared in an article in the *Journal of Social Science* a year after the 1896 election, when Mayhew professes to perceive no forces at work from which business might plausibly seek to insulate itself. Its author, F. J. Stimson of Boston (cousin of the more famous Henry), brought statistical evidence to bear on a question that was then agitating his wealthy friends and acquaintances, *viz*., “the charge that our laboring population are [sic] beginning to make that our courts are unfavorable to their interests.”

“Upon their surface,” Stimson admitted, the facts appeared to sustain the charge, for his classification of some 1,550 pro-labor laws enacted by state legislatures in the preceding ten years revealed that some 60% of them fell into categories that had been invalidated by one or another court decision. Nevertheless, Stimson concluded, the charge of the laboring classes “is unsustained by a more careful study. It is our legislatures that are at fault—our legislatures

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are playing politics... Labor leaders distrust experience, socialists detest lucidity. Between the two and the desire of our temporary law-makers to appear ‘friendly to labor,’ everything ‘goes.’”

Add to this the disorders attending every downward swing of the business cycle in this pre-Keynesian age (which commonly triggered demands for relief and thus either new taxes or new debt at the bottom of the business cycle) and it should not be surprising that elites sought to insulate themselves from political pressures, or why, as Burnham stressed, insulating themselves meant insulating courts, too. Since Rusk and Converse’s famous exchange with Burnham, a substantial number of works have appeared documenting how both major parties collaborated to change ballot laws that permitted “fusion” candidacies that aided Populism or rewrote registration laws, added literacy tests, and instituted registration requirements or even poll taxes to push down voter turnout.

The sheer amount of detail in these works, however, has led to something of a loss in focus, while within political science their findings remain largely unknown. Burnham’s famous hypothesis is therefore overdue for a global quantitative test.

This is not in fact very difficult. We decided to reopen the whole question, this time using spatial regression techniques to control for statistical problems peculiar to the analysis of geography. Because no one now denies that southern elites (essentially planters and some textile interests) deliberately restricted voting turnouts in the Old South through poll taxes, literacy tests, and a wide range of legal and extra-legal measures, the argument between Burnham and his critics turns on what happened in the rest of the United States. We, accordingly, examined the thirty-four non-Southern states that were in the union in 1896. We measured turnout decline by subtracting the average of each state’s Presidential turnouts in 1920 and 1924 from its corresponding average for 1896 and 1900.

Given the history of debate over the System of 1896, it was obvious that our regression would have to include a variety of controls, along with the basic “Burnham” variable just mentioned—the
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change in manufacturing value added per capita in each state from 1889 to 1929. Since many political scientists and some historians have emphasized ethnocultural issues instead of industrial conflict, we wanted to control for the percentage of the population in each state that was foreign born.\(^5\) For the same reasons, we wanted to control for urbanization. The Burnham–Rusk–Converse controversy also mandated checking whether a state’s choice of office block or party column voting formats affected turnout.\(^5\) Common sense and time-honored political science reasoning also suggested that the degree of party competition within states might make a difference.\(^5\) Though we knew that average educational attainment rose almost everywhere after 1900, we included a measure of state educational levels, along with other variables often invoked in contemporary discussions of nonvoting, just to drive home the point that history matters in quantitative studies of American politics.

Appendix 2 shows our results. It is obvious that Burnham’s “System of 1896” hypothesis is well supported. The change in manufacturing value added per capita across states between 1889 and 1929 is strongly related to the decline in turnout between 1896 and 1924. Since values in many states change by hundreds of dollars over this period, this variable’s impact is substantial, while other economic variables we tried did not work at all.\(^5\) Other factors also contribute: Turnout decline was steeper in more populous states, where, plausibly, social conflicts were generally more intense.\(^6\) Contrary to what many political historians might have expected, a higher percentage of foreign-born residents raised turnout somewhat. Party competition did work against turnout decline, though the coefficient indicates that it did so only weakly, while the use of the office block column ballot was associated with a fall in turnout.\(^6\) Urbanization is not significant, nor did any of our other controls make a statistically significant difference.\(^6\)

But if Burnham was right in arguing that the fall in American voting turnout outside the South after 1896 was powerfully affected by industrial capitalism, then three questions become inevitable: Firstly,
as Mayhew himself comes close to asking at one point, what explains the timing of this triumph? Surely many businesses would have loved to insulate themselves from democratic pressures even earlier in the Gilded Age. Secondly, why did not the forces of two-party competition work more powerfully to reverse the decline in democracy? And thirdly, how can one hope to generalize from this case to other realignments, since manufacturers presumably can come to dominate America only once?

These are large questions. Each could easily occasion a lengthy discussion. But all were discussed in considerable detail by Ferguson in the course of introducing his investment approach to party competition. His account allows us to concentrate on the handful of points most relevant to understanding the questions raised here, and leave details about specific historical episodes for another essay.

In accounting for the timing of the corporate takeover, the crucial point concerns the timing of merger movements. These emerge readily from any graph of American merger movements. There is a first giant wave of mergers in the mid and late 1890s, followed by smaller, but still substantial waves in the late 1920s and late 1960s. It is obvious that “big business” in a strikingly modern sense emerged in a few short years between 1893 and 1901, when the first great merger movement abruptly created scores of corporate giants that operated on a nationwide (and in some important cases, worldwide) scale, including General Electric, International Harvester, and U.S. Steel. Most of these firms originated in mergers organized by a handful of investment bankers, of which J. P. Morgan was the most famous.

The “trust movement” (as contemporaries often referred to it) coincided with three other sweeping institutional transformations of the business structure. All involved finance. The first was a vast increase in the size of America’s largest banks, whose scale increased pari passu with that of their clients. The second was the dramatic reorganization of the structure of American finance that brought the investment bankers who organized the merger movement to the top
of a hierarchical structure embracing most of the largest commercial banks and insurance companies (the so-called “Money Trust”). The third was the reorganization of American railroads, again by Morgan and a handful of other investment bankers. The combined result of all of these was a much closer integration of industry and finance, which pulled large numbers of previously “Gold Democrat” bankers and railroad men permanently out of the Democratic Party.

What was happening is perfectly obvious, but rarely noted. The financiers were investing more and more in American industry. They were beginning to acquire some of the same interests in tariffs, aggressive foreign policies, and export drives against British competitors that the industrialists shared. In addition, their own rising sense of importance tempted them to claim a bigger role in world finance. The climax of this process was the breathtaking merger movement of 1897–1901... this merger wave placed bankers on the boards of hundreds of companies. Centralizing the economy as never before, the great merger wave created a series of gigantic new corporations in which the bankers had major influence.65

This process had sweeping effects on American life, but its political consequences were particularly dramatic and far-reaching. It drastically reorganized existing markets for political parties and candidates. One could almost say that this process created a true national market for them. Up until then, most political money, even for presidential elections, had to be raised locally, or it could not be raised at all. A national market for politicians existed mostly in a vestigial sense. Astor made interest-free loans to President Monroe while he sat in the White House; Abbott Lawrence did the same for William Henry Harrison (who, alas, died before he could deliver on tariff and banking issues as Lawrence expected); and Jay Cooke, the Seligmans, and many others lavished gifts on U.S. Grant, but literally no one had the means to finance national campaigns independent
of the dozens, even hundreds of local political machines that organized local politics. Nor could presidential aspirants, senators, or congressmen realistically hope to bypass these local power centers, since the effort would have involved approaches to thousands of potential contributors whose enterprises were mostly small and locally oriented anyway. In addition—and this is no mere detail—the local organization and orientation of virtually the entire press meant that national campaigns had to be waged through provincial media, in many senses.

During the Gilded Age, New York (along with its satellites, such as Newport, Rhode Island) rapidly emerged as the center of corporate America. Its unprecedented concentration of wealth rapidly turned the city into the closest thing in American politics to a one-stop shop for politicians seeking friends and money. It became increasingly common for presidents, important congressmen (in this period, all, of course, were men), and would-be presidents to scuttle up to Gotham to be dined (or in some cases, such as Grant’s, to be feted) and showered with money. Not surprisingly, as presidents found they could tap resources on their own, sometimes in open competition with political machines, they began taking on the airs—and even occasionally the reality—of actors independent of the machines. (Exactly this, we think, may be the final cause of the shift in the seasonality of elections that our time series equation picked up. After 1876, the see–saw pattern of voting between on- and off-year elections grew more jagged—evidence, we think, of the increasing importance of the Presidency.)

The great merger movement took these developments to an entirely different level and nationalized them to a significant degree. By centralizing wealth on an unprecedented scale, the trust movement and other developments just discussed drastically cut campaign transaction costs for candidates acceptable to the leaders of big business. Political brokers like John Wannamaker and Mark Hanna (both major business figures in their own right) could, and did, cultivate ties to the new monster national corporations like Standard Oil,
James J. Hill’s Great Northern Railway, or J. P. Morgan to assemble campaign war chests of unprecedented size.

Giant corporations operating in national markets also created almost overnight a vast national market for advertising. This swiftly opened up a whole new arena for business to influence politics. National magazines, such as McClure’s and the Saturday Evening Post, along with a handful of older magazines, such as The Atlantic, competed for corporate advertisers by seeking to attract audiences. Most of these concerns quickly became part of big business themselves and investment bankers began joining their boards, or in the case of a few famous “muckraking” concerns, purchasing them. National newspaper chains also emerged; their “yellow journalism” quickly became the era’s counterparts to today’s Fox Network.

Presidential candidates could now raise enormous funds from relatively small numbers of magnates, who owned plants and retained law firms and lobbyists (in practice, virtual synonyms) all over the United States, while politicians in states and localities found their abilities to tap funds increasingly called into question by the spread of civil service reform and (somewhat) more vigorous law enforcement. Presidents could also go over the heads of other political actors to appeal directly to the people through the corporate-dominated national media and big-city dailies. Though we lack the space to develop the point any more in this paper, this is the long-sought thread that links the 1896 election to the rise of the strong presidency and other turn-of-the-century changes in American politics. A proper analysis, however, needs to place these changes in the context of other developments of the period that also had major political consequences, such as the rise of large foundations that claimed to operate outside of politics while discharging functions that in other countries were (or eventually became) the province of party-related research institutions; the corporate-oriented restructuring of higher education; and the rise of “professionalism” encouraged and sometimes directly subsidized by large-scale business and foundations.
If the investment approach accounts nicely for the timing of the corporate takeover of American political life, it also has a uniquely clear answer to why party competition operated so feebly in defense of mass democracy after 1896. As noted, the parallel reorganizations of industry, finance, and the railroads pulled most “Gold Democrat” financiers and railroad magnates out of their party for good, creating a huge bloc of investors with interests in both industry and finance almost irresistibly attracted to the Republican political formula of high tariffs, “dollar diplomacy” and imperialism, the gold standard, laissez faire, and a strong antilabor policy. The loss of these investors lamed the Democrats for a generation.

But a substantial bloc of businesses remained in the party with an interest in the Democrats’ traditional policies of free trade and a pro-British foreign policy. These included investment and commercial bankers who simply missed the merger movement (notably in Boston, which gradually receded in status compared to New York); importers and many mercantile interests, such as port city retailers like the Filenes of Boston; some large copper companies whose refineries could operate profitably only if tariffs did not block out copper ores from abroad; firms like International Harvester that dominated foreign markets with their products and wanted lower American tariffs so that their wares would be allowed into other markets; certain foreign multinationals, such as the German chemical companies, who battled to keep American tariff rates down; some railroads, and a variety of other interests, including many independent oilmen and transit companies close to big-city Democratic machines. In 1912, the issue of the Federal Reserve temporarily brought an even broader range of financiers over to the Democrats.

Such interests were as frightened of labor turbulence and the mass electorate as any Republican manufacturers. They were possibly even more conservative on certain other issues. To win elections by making appeals that would have increased voter turnout would have been self-defeating for them; they would literally have lost by
winning, since they would then have confronted an aroused mass public. In some documented cases, these investors joined efforts to depress turnout; it is in any case obvious that whenever there was conflict, there was no interest. Investors simply closed their wallets to candidates they perceived as too radical, so that in 1904, for example, the Democratic candidate for President, Judge Alton Parker of New York, ran well to the right of the great Wizard of Oz himself, Theodore Roosevelt.

Our final query, about whether and how this account can be generalized, is equally easy to answer in principle. First of all, consider the general form of the answer in more detail. The investment account of realignments points to a massive change in political coalitions in the investor blocs that dominated the parties. The change happens for profound reasons of political economy and involves a relatively sudden and, in this case, overwhelming, transformation in the scale of the relevant actors. The scale change—indeed the abrupt appearance of whole ranges of actors that literally did not exist even a few years before (giant manufacturing corporations, allied with investment and commercial bankers who also dominated the railroads)—fundamentally shifts power. In place of hundreds of ordinary monkeys, suddenly King Kong appears. These shifts in scale and concentration make it much easier for these actors to get what they want and much harder for everybody else. The shift in incentives and “transactions costs” is long lasting; it does not go away when one or another politician disappears. In fact, no detail of the 1896 election affects it in any fundamental way, nor is it tied essentially to partisan identification. (Indeed, if party trends work against a political coalition with this kind of resources, it can redirect its efforts to some other part of the country to compensate.) But it also develops intrinsically, over the long run, as growth cycles and depressions in the world economy slowly build up new forces (including, most obviously, new industries) and create the conditions for changes in the underlying balance of power between social classes that the system incorporates.
The question is therefore whether similarly massive, deep-seated changes in investor blocs occurred during or just ahead of the other realignments our time series has identified. ("Just ahead of," because many analysts have pointed to cases in which economic downturns appear to have precipitated political conflicts that had simmered during a preceding boom.) Here, we can build directly on prior work by one of us. The case of the Jacksonian Realignment seems obvious. The breakup of the "Era of Good Feeling" clearly fits an account in terms of sudden changes in scale and the rise of investor blocs that literally had not previously existed. With the end of the War of 1812, the factory system spread rapidly through parts of the Northeast and, more slowly, into the Middle West. An “industrial structure” in a true sense rapidly developed, bringing with it for the first time a broad range of interests whose demands for tariffs could be satisfied only at the national level. Innumerable companies also formed to promote internal improvements in waterborne transportation and roads, while a flock of new bankers who detested the Second Bank of the United States grew rapidly. With agricultural prices rising, demand for western lands also soared. These changes in the mode and scale of production brought a towering concentration of wealth that is now well documented.

With the headlong pace of American development posing one issue after another that could be resolved only by control of the national government (including the rate of removal of Native Americans from the West and South and territorial acquisitions involving possible wars, not only with Mexico, but Great Britain, then the center of the world economy), older eastern mercantile capitalists and the planters who dominated the southern cotton economy now suddenly faced true rivals. Reasonable data on mergers exist only from the 1890s, and systematic comparative evidence about size differentials among large firms goes back only to the turn of the twentieth century. But a wide variety of sources provides reasonably good, if imperfect, evidence about the rough relative sizes of
railroads, banks, canal companies, and industrial firms before then, including the various U.S. censuses of manufacturing. They indicate that scale changes during the Age of Jackson were both sudden and substantial.70

Statistical evidence for the period just ahead of and during the Civil War is also incomplete, though railway manuals and other sources suggest that the boom of the 1850s brought forth notable increases in the scale of firms and corporate concentration in certain sectors. But no comprehensive review of the statistical evidence is known to us. Nevertheless we can offer compelling evidence of a link between momentous scale changes in the economy and political power in this period—evidence that should also still doubts about how an economic sector like manufacturing that had been thriving in parts of the United States as far back as the War of 1812 could properly be described as coming to power only at the end of the century.

Power in America is often exercised in Byzantine ways, but the investment theory of party competition is usually straightforward about how to find it: Just look for the candidates and advisors with direct ties to major investors. Until the rise of White House staff and Executive Office advisors complicated the task, a perfectly serviceable way to make a rough and ready assessment is simply to scrutinize the cabinet and people around the President for obvious ties to major investors. Here the historical record is striking. We have already seen that while small manufacturers dotted the landscape of New England, Pennsylvania, and parts of the Middle West for decades before 1896, giant manufacturing businesses were comparatively rare.

Large-scale businesses, however, did exist—very large-scale businesses indeed. These were railroads. These started small in the late 1830s. During the boom of the 1850s they grew substantially, as local lines began to integrate into what finally became trunk lines. The true giants emerged from the ensuing rush to build nationwide systems after the Civil War. Notoriously, several of the largest of
these profited handsomely from federal government subsidies put through by cooperative (and often handsomely bribed) Republican officials.

The stupendous scale of railroads compared to all other enterprises of the period is difficult to imagine today. America’s first true “big business” dwarfed every other institution in society in the 1850s . . . as late as the early 1880s, Carnegie Steel, a leading manufacturing corporation, was still capitalized at only $5 million, while at least 41 railroads had capital values of $15 million or more.71

While rarely done, it is straightforward to count presidents and cabinet officials with “direct” business ties to various types of corporations and businesses, in the sense that they personally owned major blocs of shares in, were employed by, or represented these interests at the time they entered the cabinet or the campaign began.72 (Had the count been of indirect ties through family members meeting these qualifications, the totals would swell substantially, though the qualitative significance of the results would not change.) Two striking facts jump out from such efforts: Firstly, direct representatives of railroads in the White House or the cabinet were exceedingly few in the run up to the Civil War. Then, under the Republicans, the pattern abruptly switches.73 The GOP standard bearer in 1860 was, of course, a renowned attorney for the Illinois Central, who owed his nomination in no small part to massive efforts by many lines. Thereafter, representatives of railroads swarmed over the federal government, quickly claiming major places in both parties.74 Secondly, for all the hundreds of manufacturing concerns in the United States before the 1890s, only two people with direct ties to such concerns appear to have made it into the cabinet after the Civil War: One, Robert Todd Lincoln, son of the fallen President, headed Pullman, a true giant peculiarly tied to railroads; the other, Daniel Lamont, in the Cleveland administration, is much more obscure and may be misclassified.
Then, suddenly, with the presidency of William McKinley, as though by an invisible hand, what occurred with Lincoln and the railroads happened to the manufacturers. Individuals with strong ties to truly large firms from this sector piled into the cabinet, though with a notable bias (aside from a few exporters strongly interested in lower tariffs) toward the Republicans. Though several generations of historians have talked around the astonishingly brazen facts, the perfect symbol of the new Republican order was the emergency bailout fund raised by some of the most famous manufacturers in the United States to rescue the political career of America’s best-known champion of the high tariff on the very eve of the 1896 campaign, when a note McKinley had cosigned went into default. The list is a veritable fantasy of an investment approach to critical realignment: Andrew Carnegie and Henry Clay Frick headed it, along with Philander Knox (the former general counsel of Carnegie Steel, whom McKinley subsequently appointed Attorney-General), Samuel Mather (a major figure in the Cleveland business community with large interests in mining and iron manufacturing, who almost immediately helped organize Morgan’s giant Federal Steel, which then merged with Carnegie Steel to form the biggest corporation in history, U.S. Steel, superintended by Frick, Knox, and Morgan himself), Mark Hanna (who was not merely a political boss, but a major iron manufacturer whose family eventually helped organize National Steel), and John Hay (who had many longtime business and family ties to Mather and other Cleveland industrialists, was later appointed Secretary of State by McKinley, and eventually became famous as the author of the “Open Door” notes demanding access to foreign markets on an equal basis for American manufacturers).

Almost as extraordinary was J. P. Morgan’s unique contribution to that year’s political business cycle. In a development historians appear to have missed completely, Morgan organized a special syndicate of international banks. Fearful that less steely investors might dump American securities after Bryan’s surprise nomination
and perhaps trigger a panic that would work to his advantage, the Corsair and his allies essentially set up as private central bankers and bought gold from all comers. “Orderly markets” were preserved, allowing the slight upswing in business and farm prices that historians have identified as playing a role in Bryan’s eventual defeat.77

After the 1890s, the relationship between realignments and merger waves can be treated with more precision, as can the closely related issue of turnover among giant firms. But more detailed consideration would take us too far afield. Still, the critical point is crystal clear. Quite like the turn-of-the-century shifts in the American political universe, both the New Deal and the realignment of the 1960s were closely associated with giant merger waves.

Our paper’s principal concern has been with the past, what it meant, and how best to think about it within a political science framework. But it is difficult to conclude without a final remark on the connection between party realignments and merger waves. As one scrutinizes recent data on mergers, the famous dictum of Horace that prefaces our paper comes irresistibly to mind.

Just possibly the story of the System of 1896 is, after all, about us. We have already noted that our time series analysis suggests that the New Deal ended around 1968, near the crest of yet another merger wave. By comparison with previous merger movements, however, this wave had several distinctive properties. Firstly, though very large in absolute terms (sheer numbers of mergers), the sixties wave looks less impressive if assessed in terms of a yardstick that adjusts for the changing of the economy. Even more striking, however, are other outstanding features of this merger wave: An outsized proportion of its mergers were failures; in addition, in the decades of headlong globalization that followed, large firms on average became smaller; and the share of the economy represented by the very largest actually declined by many different measures.78 In effect, King Kong was shrinking and all kinds of monkeys were horning in on his act.

Not surprisingly, the visible political traits of the late 1960s realignment differed markedly from previous ones. It was peculiarly
“formless.” Waiving often striking regional details, from a summary national standpoint, what occurred was a modest fall in the share of the total potential vote for the Democrats. Despite some breathless claims in the early 1970s and again a decade later, however, the Republicans did not emerge as a new national majority party. Not surprisingly, many analysts often interpreted this state of affairs as a “dealignment.”

In the 1990s, however, a worldwide merger boom of historic dimensions bubbled up. American firms, now increasingly taking foreign partners or becoming takeover targets for buyers abroad, led the way. A wave of start-up “dotcom” mergers also swept over the United States. While many of these latter soon crashed, the dawn of the new millennium also brought an explosion of couplings between firms that were clearly not going away—Exxon and Mobil, Time with AOL, Chevron and Texaco, Citicorp with Travelers and then with Smith, Barney, J. P. Morgan with Chase, Bank of America with NCNB, etc.

Once again, the average size of firms began to rise and the share of the total economy represented by large firms began growing again. In some industries, such as mining and oil, turnover also slowed, because only a few giants were left. Even controlling for the changing GNP, the sheer volume of deals was stupefying.79 King Kong and his cognates were back.

It is difficult to still a sense that the United States is again on the brink of epochal political change. In several of the industries dominated by new (or newly swollen) giants, notably oil, mining, and utilities, the tilt in favor of the GOP is so lopsided that it almost defies belief.80 Could it be that many of the often startlingly radical policies adopted by the new Bush administration provide a glimpse of an impending realignment in American politics? Was, for example, the rape of California by Enron and other newly deregulated, hyperaggressive energy giants an isolated event or a portent of things to come? Are the administration’s dramatic break with multilateralism and periodic swipes at “old Europe” somehow bound up with
new positions of U.S. firms after the worldwide merger wave? And did the invasion of Iraq signal how a plenary share of America’s multinationals expect to play the “Great Game” in Asia and other parts of the world in the next decades?

This paper cannot even begin to consider such questions. But one fact about the 2004 election is striking: The value for the variable that we used in this paper, the Democratic percentage of the total potential vote, behaved anomalously. A rise in voting turnout that was little short of sensational swept it up, too, even as the Party’s standard bearer was submerged in a still more powerful Republican tide.81 If this heralds some new, powerful trend in popular mobilization, then we may indeed be witnessing yet another instance in which a sweeping merger movement triggers partisan realignment.

Appendix 1

Let $Y_t$ be the Democratic share of the total potential vote (expressed as a percentage) in elections for President and, in off-year elections, for the House of Representatives, as explained in the text.82

Then, the second- and fifth-order multiplicative autoregressive model can be written as

$$
(1 - \phi_2 B^2)(1 - \phi_5 B^5)(Y_t - \mu)
= \omega_1 I_t^{(1824)} + \omega_2 I_t^{(1860)} + \omega_3 X_{1,t} + \omega_4 X_{2,t}
+ \omega_5 X_{3,t} + \omega_6 X_{4,t} + \omega_7 I_t^{(1964)} + \epsilon_t
$$

where

$$
I_t^{(1824)} = \begin{cases} 1, & \text{if } t = 1824 \\ 0, & \text{if } t \neq 1824 \end{cases}
$$

$$
I_t^{(1860)} = \begin{cases} 1, & \text{if } t = 1860 \\ 0, & \text{if } t \neq 1860 \end{cases}
$$
The estimated model is

\[
(1 + 0.24B^2)(1 - 0.24B^3)(Y_t - 32.04)
= -17.55 I_{t}^{(1824)} - 9.99 I_{t}^{(1860)} + 5.58 X_{1,t} - 11.95 X_{2,t} \\
- 6.27 X_{3,t} + 3.28 X_{4,t} + 9.08 I_{t}^{(1964)} + e_t
\]

| Parameter | Estimate | Error | \( t \) Value | Pr > | \( |t| \) | Lag |
|-----------|----------|-------|--------------|-------|---------|-------|
| \( \mu \)  | 32.03658 | 0.55293 | 57.94 | <.0001 | 0 |
| \( \phi_3 \) | -0.23737 | 0.11003 | -2.16 | 0.0338 | 5 |
| \( \phi_2 \) | 0.24417 | 0.10962 | 2.23 | 0.0286 | 2 |
| \( I_t^{(1824)} \) | -17.54639 | 2.97653 | -5.89 | <.0001 | 0 |
| \( I_t^{(1860)} \) | -9.98779 | 3.01165 | -3.32 | 0.0013 | 0 |
| \( X_{1,t} \) | 5.57652 | 1.12116 | 4.97 | <.0001 | 0 |
| \( X_{2,t} \) | -11.95389 | 0.91220 | -13.10 | <.0001 | 0 |
| \( X_{3,t} \) | -6.26593 | 1.77816 | -3.52 | 0.0007 | 0 |
| \( X_{4,t} \) | 3.27757 | 1.04450 | 3.14 | 0.0023 | 0 |
| \( I_t^{(1964)} \) | 9.07550 | 3.06138 | 2.96 | 0.0039 | 0 |

Notes: An autocorrelation check of the model residuals plus Bartlett’s Kolmogorov–Smirnov statistic both indicate white noise residuals. Tsay’s test indicated changes in variance were not a problem.83
Appendix 2: Results for Spatial Regression on Turnout Decline, 1896–1924

Turnout decline indicates spatial dependence among states and a Moran test confirmed this. Its value of 0.2848 is significant. We, accordingly, estimate the usual Gaussian stationary spatial model as developed, e.g., by Cressie.84

Formally, let $Y_i$ be TUDEC496, the turnout decline in 1896–1924 for the $i$th state, $i = 1, 2, \ldots, 34$, and $X_{ij}, j = 1, 2, \ldots, 5$ be the independent variables that listed in Table A2.1.

$$
Y_i = \beta_0 + \beta_1 X_{i1} + \beta_2 X_{i2} + \beta_3 X_{i3} + \beta_4 X_{i4} + \beta_5 X_{i5} + \delta_i + \epsilon_i
$$

where $\delta_i$ is a latent spatial random effect variable, with $E(\delta_i) = \bar{\delta}_i$, $\text{Var}(\delta_i) = \sigma_\delta^2/n_i$; $n_i$ is the number of neighbors of district $i$, and $\bar{\delta}_i = \frac{1}{n_i} \sum_{j \in \text{neighbors}(i)} \delta_j$. And $\epsilon_i$ is the random error term with $E(\epsilon_i) = 0$, $\text{Var}(\epsilon_i) = \sigma_\epsilon^2$ and $\text{cov}(\epsilon_i, \epsilon_j) = 0$ for $i \neq j$. (Note that a steeper turnout decline registers as a higher positive number, so that a negative sign on a variable implies that it is a retarding influence.)

Equation (A2.1) can be estimated as a Conditional Spatial Autoregression (CAR) using the SLM function of S-Plus. Table A2.1 shows the estimated coefficients and associated $p$-values. Q–Q plots of the residuals indicate that this time the residuals well approximate a normal distribution. When the Moran test is run on this model, the result is a large $p$-value indicating that the residuals are not spatially autocorrelated.

| Name of Variable                  | Value    | Std. Error | $t$ Value | Pr(>|$t$|) |
|----------------------------------|----------|------------|-----------|----------|
| Intercept                        | 47.2812  | 11.1673    | 4.2339    | 0.0002   |
| Office block ballot              | 4.5949   | 2.3739     | 1.9355    | 0.0635   |
| Competition index                | −0.3310  | 0.1167     | −2.8357   | 0.0086   |
| Foreign born % 1910              | −0.7264  | 0.1421     | −5.1126   | 0.0000   |
| Change 1889–29 Manf. val add per capita | 0.0369 | 0.0127     | 2.9112    | 0.0071   |
| Population 1900                  | 0.0294   | 0.0078     | 3.7646    | 0.0008   |

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\[ R^2 = .76; \] substantially more than a nonspatial ordinary least squares model.

NOTES

We are most grateful to Walter Dean Burnham for very valuable assistance that went far beyond the indispensable data he supplied to us. We are also grateful to David Reilly, Frederick Pryor, Robert Yaffee, and the editors of this journal for advice; and to Vivek Chibber for inviting us to present an earlier version at his research colloquium at NYU.


8. Ibid., 154.

9. Ibid., 165, 156, 148, 153.


22. Would our results differ if we had used the presidential totals? Essentially all the qualitatively interesting results discussed below, including the 1876 change in seasonality, hold if one uses the presidential vote totals, though the autoregressive terms (obviously) differ. The upward shift of the popular vote moves back to 1828, while the 1902 break becomes 1904, the first presidential election after Bryan’s second loss. The fifth-order autoregressive term also falls away. None of this matters very much, in our view, and follows naturally from the difference in data.

23. Autobox; we profited greatly from advice from David Reilly, even when we sometimes proceeded differently.


27. The point can be formulated in an alternative, statistically equivalent way: When the decisive political change involves the other major party blowing up or suddenly arriving on the scene, a time series centered on the Democratic share of the total potential vote may not register such events, especially in a high-turnout era. If one does not want to move at once to a multiple time series approach, then dummy variables representing the relevant institutional changes are the obvious solution. Given the historical facts, the dummies belong exactly where we have placed them in our equation.

28. One near-miss merits special mention. We found some indications of a downward level shift in the Democratic vote for 1980 and after. But this failed to attain an acceptable level of significance even initially. After employing Tsay’s procedures for detecting variance changes discussed in Appendix 2, we discovered that including it
in the model led to changes in the model variance. When we introduced a correction for the variance change, the level of significance became simply unacceptable.


32. Our last sentence is inspired by a comment by Peter Schlozman, relayed to us by Kay Schlozman.


34. The point of view represented in *Ibid.* perhaps lends itself readily to such a position, though the authors probably would not instinctively agree.


45. Mayhew’s claim (133) that “probably the best kind of insulation is a high economic growth rate,” requires major qualification. Strikes are often pro-cyclical; as mentioned below, the System of 1896 exploded in 1919–20 precisely because of a very high rate of growth.


47. Mayhew claims (129) that the period after 1920 had no intrinsic connection to earlier periods of the System of 1896; but in fact the struggle to contain labor
led to the “Great Red Scare” and another wave of ballot legislation in New York and other states that further lowered voting turnout, as elite attitudes hardened enormously.


50. See Jerold S. Auerbach, Labor and Liberty: The La Follette Committee and the New Deal (Indianapolis: Bobbs-Merrill, 1966), passim, which is based largely on the La Follette Committee hearings the book describes.

51. Ibid.


56. We used the U.S. Census figure for 1910; this is a percentage; its range runs from about 1.8 to 33, with a mean of 18. We tried other measures, including one for only non-Northwest European immigrants; this, however, excludes the Irish and therefore makes little sense. Several of our variables represent state-level counts of individuals. On the pitfalls of such “cross-level” inferences see, e.g., Christopher Achen and W. Phillips Shively, Cross-Level Inference (Chicago: University of Chicago Press, 1995).

57. We followed A. C. Millsbaugh, “Irregular Voting in the United States,” Political Science Quarterly 33, No. 2 (1918): 230–254; we added Kentucky, which he unaccountably omitted. Most of the rest of our data came from various U.S. Censuses; the data for state manufacturing value added per capita are from Richard Easterlin, “Redistribution of Manufacturing,” in Simon Kuznets, Anna Ratner Miller, and Richard Easterlin, eds., Population Redistribution and Economic Growth, United States, 1870–1950 (Philadelphia: American Philosophical Society, 1960), 131. Recalling Burnham’s cautions about treating ballot changes as “uncaused causes” we employed a method recommended by Paul Allison, Multiple Regression (Thousand Oaks: Pine Forge Press, 1999), 62, to check whether the office block format itself was really an indirect reflection of industrial power. The answer appears to be negative. Its spread may therefore have reflected a slightly different set of direct concerns and pressures (such as simply striking at parties directly).

58. We used the indices in Paul David, Party Strength in the United States, 1872–1970 (Charlottesville: University of Virginia, 1972), for the period 1896–1930. The index has a top of 100. The actual range is from 56 to 98, with an average of about 86.
59. Changes in manufacturing value added per capita among the states run from $12 to $368, with an average of approximately $175. Note that measures of the change of GDP per capita for states near enough to our period have not been published.

60. Population is measured here in units of 10,000. The average value of this measure is approximately 164.

61. This was, obviously, captured via a dummy variable.

62. The Catholic percentage of the population did not appear to matter. While we believe that some Protestant denominations at certain times, notably around 1919, may have played a role in furthering turnout decline, there is no practical way to test for such effects with existing data.


64. For this and most of what follows on the merger wave of the 1890s, cf. Ferguson, *Golden Rule*, 71–79.

65. *Ibid*, 76.


67. See, e.g., Ferguson, *Golden Rule*, 46–47; though the idea is much older.


70. It is likely that the boom that crested in the 1830s intensified patterns evident in the twenties. Neither the corporate form nor public issues of stock were common. Combinations probably occurred mostly by forming new partnerships or takeovers of bankrupt assets. Individual fortunes, however, certainly did grow.

71. Ferguson, *Golden Rule*, 65, whose discussion cited work by Chandler and Burch. If this passage sounds familiar, however, it may be because it is. Kevin Phillips uses the same illustration three different times in Phillips, *Wealth*, 42, 254, 305. Hunts for unattributed borrowing are reaching pathological proportions in American literary life; one of us, accordingly, did not belabor this point when reviewing Phillips's genuinely useful work for the *Washington Post*. Cf. Thomas Ferguson, “Following the Money,” *Washington Post Book World* 32, No. 20 (May 19, 2002): 7. Elsewhere in the book, Phillips is quite clear that he was a careful reader of *Golden Rule*; but he should keep his note cards in better order.

72. We relied upon the discussion in Philip H. Burch, *Elites in American History*, 3 vols. (New York City: Holmes & Meier, 1981), making a few additions or corrections based on our own research. An inevitable element of uncertainty of judgment enters in some cases where a multiple assignment is obviously appropriate or where a lawyer may have served as a local counsel for some railroad or manufacturer.

73. This is not to say they were not indirectly represented; there is no question they were. But it is easy to understand why the pro-Southern Buchanan administration halted all federal aid to railroads on the very eve of the Civil War. Ferguson, *Party Realignment*, 67.

74. From Fillmore, through Pierce, Buchanan, and Lincoln, the sequence runs 0, 2, 2, 7, with the high figure for Lincoln representing larger lines. Rates remain high for a generation, becoming almost absurd under Grant.

75. The numbers jump from 0, 0, 1 for Cleveland’s first term, Harrison’s term, and Cleveland’s second term to 6 under McKinley, 9 under Theodore Roosevelt, 4 under Taft, etc.; with, again, the size of the firms increasing markedly.

76. Many names have been mentioned in connection with this agreement by various authors. Our list is not derived from any secondary source, but from the “Copy of
Receipt For Deed of Trust,” Feb. 23, 1893, now in the Hanna-McCormick Family Papers, Library of Congress, Box 2. Myron T. Herrick, a prominent manufacturer who later helped organize Union Carbide, is also named as a trustee.

In William McKinley (New York: Times Books, 2003), it is obvious that Kevin Phillips, who is certainly not insensitive to the role of wealth in politics, rested his account of the note episode entirely on secondary sources. Else he could hardly have written of the underwriters that “none appear to have sought favors or accepted offices” (68). The story that McKinley did not know the identity of most of his rescuers has been repeated by some biographers. We doubt this very much; all the names are plainly on the trust agreement, a copy of which McKinley surely had to have possessed, if only to protect his wife. Phillips should also put his discussion of McKinley’s personal probity in context. As Horatio Seymour memorably enjoined Samuel B. Tilden, the celebrated New York bank attorney who may well have been robbed of the Presidency in 1876, the problem really was to put “men in office who will not steal but who will not interfere with those who do.” Quoted in Thomas Cochrane and William Miller, The Age of Enterprise (New York: Macmillan, 1942), 159–160.

77. On the upswing, see e.g., Gilbert Fite, “Republican Strategy and the Farm Belt in the Election of 1896,” American Historical Review 65, No. 4 (1960): 787–806. Ferguson found records of the gold pool in a famous London bank, of which more another time.


82. Dependent variables expressed as percentages are sometimes transformed. We experimented with several forms; but since they made no difference, we used the untransformed variable.
