

the nature of the self and “learned idioms.” He devotes approximately two and a half times as much space to the political topics noted above as to absolute idealism. With a few exceptions, such as using his conception of the self to justify preferences for one’s compatriots over inhabitants of other countries in distributive justice and immigration, little on the political topics is directly shaped by his idealist approach, while discussion of each topic is generally brief and superficial. For instance, the six international topics discussed in chapter 7 receive a total of twenty-four pages.

Because of the deep suspicion in which absolute idealism is currently held by most scholars, Wulf would have been well served to provide a stronger and more convincing defense of his position. This review is not the place for an in-depth critique of absolute idealism. But a century of analytical philosophers have had little trouble picking apart the position’s major claims, and it would not be difficult to do the same for Wulf’s. For instance, as noted, central to Wulf’s account is a view of the self as a collection of “learned idioms.” Such a view may have direct implications for conduct in a society that is extremely homogeneous. But in a pluralistic modern society, people are constituted by multiple conflicting idioms. Without guidance as to how to prioritize these, i.e., exactly which ones our natures require us to develop, the position offers very little.

Chapter 2 of the book briefly reviews competing “inadequate” theories of obligation. These include views based on consequences, gratitude, consent, fairness, Samaritanism, and membership. Once again, Wulf’s discussion is extremely cursory. For instance, gratitude receives approx-

imately two pages, consequences less than two, and consent approximately three. In addition to objections particular to each theory, Wulf generally contends that they are flawed because of unacknowledged moral assumptions. Thus, for example, consent theory rests on an unexplained requirement to keep one’s promises. In criticizing these theories, in general, Wulf does not seem to be aware of recent arguments and counterarguments (including, for full disclosure, the arguments surrounding my own theory of obligation based on the principle of fairness, which Wulf directly addresses).

But more important than particular lapses is his failure to develop a sustained critique of reflective equilibrium. Is it actually flawed by foundational commitments, or is reliance on considered judgments defensible on other grounds? In *A Theory of Justice* (1971), John Rawls famously writes, “A conception of justice cannot be deduced from self-evident premises or conditions on principles; instead, its justification is a matter of the mutual support of many considerations, of everything fitting together into one coherent view” (p. 21). Once again, this review is not the place for an in-depth discussion of reflective equilibrium. I do not contend that this approach is beyond criticism, but that the criticisms have to be developed properly. Let it suffice to say that there are obvious rejoinders to Wulf’s main claims. Although he is to be commended for striking out in a new direction, his new path is far thornier and beset with more difficulties than he acknowledges. Like it or not, adequate defense of “absolute idealism” should provide a lot more by way of defense than Wulf offers in this book.

AMERICAN POLITICS

Media Concentration and Democracy: Why Ownership Matters. By C. Edwin Baker. New York: Cambridge University Press. 2006. 272p. \$73.00 cloth, \$24.99 paper.

Post-Broadcast Democracy: How Media Choice Increases Inequality in Political Involvement and Polarizes Elections. By Markus Prior. New York: Cambridge University Press. 2007. 336p. \$89.00 cloth, \$27.99 paper.
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— Michael W. Wagner, *University of Nebraska-Lincoln*

While humans need only one heart to pump life-sustaining oxygenated blood to the body, a democracy requires more than a single source to circulate the life’s blood of representative government—information—to its citizenry. It is generally argued that the wider the variety of sources informing the electorate about politics, the better. However, the degree to which issues like the increasingly concentrated ownership of the news media, juxtaposed against the simulta-

neous growing availability of different cable television channels and Websites, actually affect democratic health has received too little systematic, scholarly attention. Two exciting new books address these issues in strikingly different yet intellectually stimulating ways; their dissimilar conclusions about issues surrounding media ownership and the influence on democracy and democratic behaviors of media choice provide worthy grist for the scholarly mill.

C. Edwin Baker’s *Media Concentration and Democracy* makes the case for opposing concentrated media ownership. An accomplished legal scholar who has had much to say about media markets, democracy, and the First Amendment, Baker focuses on three major arguments. First, and most importantly, he puts forward a “democratic distribution principle” that “democracy implies as wide as practical a dispersal of power within public discourse” (p. 7). Flowing from this general principle are two additional arguments against concentrated ownership—that dispersion of ownership both creates democratic safeguards and thrusts media outlets into the arms of owners interested in quality rather than the bottom line.

Baker's democratic prerequisites are more aggressive than Robert Dahl's claim in *On Democracy* (1998) that the *availability*, rather than Baker's preference for the *widest possible dispersion*, of alternate sources of independent information is necessary for a democracy. Baker argues that in order for democracies to achieve fair bargains, there must be a reasonable weighting of as many different people's interests as possible; this requires an extremely wide variety of media owners. Quick to recognize that the "common good" may require media to communicate en masse, he supplies two caveats to his thesis. First, the wide dispersal of ownership could result in a heavily segmented audience, making fair bargaining and consensus difficult. Second, mass media deal with aggregates. Nevertheless, these caveats are brushed aside and do not prevent the democratic distribution principle from supporting the opposition to any merger of any media outlets. For the author, concerns about how media concentration makes the media vulnerable to outside pressure or creates the opportunity for the distortion of the news are of paramount importance.

Baker spends much of his effort answering supporters of media deregulation, countering claims, most notably by Benjamin Compaine (*The Media Monopoly Myth: How New Competition Is Expanding Our Sources of Information and Entertainment*, 2005), that concentrated ownership is not a problem as there are many owners of news media outlets and several sources from which people can acquire information. Baker forcefully argues that the entire media are not the relevant market and that it is a mistake to equate economic criteria for sociopolitical criteria when claiming there is an abundance of owners (p. 59–75). He assails the oft-cited hope that either the market or the Internet will provide a proper diversity of voices in the mediated world, suggesting that the most popular Internet news sites are corporately owned and that few websites reach a large number of people.

Arguing that a proper reading of the First Amendment values democratic processes, Baker embraces a theory of "complex democracy" derived from Jürgen Habermas, which requires discourses striving for uncoerced agreement, on the one hand, and fair bargaining, on the other (p. 146). While recognizing that these goals create tension for the democratic distribution principle, Baker does not grapple with this problem very seriously, nor does he revisit the aforementioned two crucial caveats to the democratic distribution principle. He closes with a series of policy prescriptions surrounding antitrust law and requirements that the government approve media mergers and stop those that increase concentration or allow owners to come from nonmedia firms. These, he admits, are not very likely, leaving the reader to wonder about more pragmatic efforts that might be made to encourage ownership dispersion.

The forceful elegance of Baker's arguments, their grounding in legal theory, and his clear prose are real strengths; the book is appropriate for graduate courses in law, eco-

nomics, political communication, and democratic thought. The author gives the reader good reason to question the wisdom and legitimacy of the concentrated ownership of information providers. However, the arguments proffered fail to engage with political science work seeking to examine similar issues. He asserts that democracy requires people to be able to form opinions and express them, but fails to incorporate key *political* variables into his arguments. Absent are discussions of the capacity of citizens to make democratic decisions in a concentrated environment or with limited information, and the role that political institutions like the media play in creating conditions favorable for democratic decision making (Arthur Lupia and Mathew McCubbins, *The Democratic Dilemma*, 1998).

Moreover, a discussion of factors influencing a democracy's health ought to consider political parties. Paul Sniderman and John Bullock claim that democratic opinion is menu dependent—and that the parties make the menu ("A Consistency Theory of Public Opinion and Political Choice: The Hypothesis of Menu Dependence," in William E. Saris and Paul M. Sniderman, eds., *Studies in Public Opinion*, 2004). Whether concentrated ownership affects the mediated communication of partisan issue positions is not addressed by Baker, nor does he consider that parties might create conditions that increase the concentration of debate, even with wide ownership dispersal, by stamping out perspectives not delivered by Republicans or Democrats.

While Baker argues that media concentration is bad for democracy, Markus Prior explores the thought-provoking possibility that increasing media choice has negative consequences of its own. In his theoretically innovative and methodologically rigorous *Post-Broadcast Democracy*, Prior explores how the media environment affects political behavior. He repeatedly and clearly demonstrates that increased media choice increases political inequality with respect to news consumption, political knowledge, and voter turnout while being a major culprit in the increasing polarization of elections and the electorate itself.

Prior's central argument is simple, elegant, and persuasive: Now that there are more television stations to watch, radio dials to tune in, and Websites to surf, those uninterested in politics can effectively remove themselves from situations where they would receive political information, while news junkies can always get a fix. This results in little to no change in aggregate measures of political knowledge and voter turnout, but misses important, and heretofore unseen, *individual* changes in political inequality. That is, when there were three television stations, if people wanted the television on at 6:30 they had to watch the news, and so even those who were not interested in politics were apt to engage in a process of Downsian by-product learning. With hundreds of channels at the fingertips of one's remote control, those interested in political news are sure to watch politically oriented news programming, while those who do not

wake up wondering how they will hold government accountable are likely to flip to *Desperate Housewives* or *Monday Night Football*. Increasing media choice allows the politically uninterested to avoid learning about politics. This fact has serious consequences for democracy.

Using an innovative survey experiment, Nielsen research data, and sophisticated treatments of American National Elections Study and other survey data, Prior reports evidence consistent with his “Conditional Political Learning” model, which posits that “*the effect of motivation on political learning depends on the media environment*” (author’s emphasis, p. 33). Analyzing data from the 1930s to 2005, Prior shows that broadcast television’s inception increased the political knowledge and propensity to vote of the less politically interested by limiting their media choices, while the advent of cable allowed fans of entertainment to eschew political information, resulting in news fans making elections more partisan.

Prior’s arguments with respect to political polarization are less convincing as they do not adequately deal with some of the major perspectives on partisan change during the time period he studies. In one example, recently rearticulated by James Stimson, political elites were polarized on the abortion issue before public attitudes on abortion became predictable by partisan identification (*Tides of Consent*, 2004). Moreover, Geoffrey Layman and Thomas Carsey’s many accounts of “conflict extension” in the electorate must be incorporated into explanations of polarization involving the media environment.

Given the focus on entertainment fans and news junkies, it is a bit curious that Prior does not consider the political relevance of some entertainment programs. Indeed, it is challenging to “get” the jokes on *The Daily Show* or *The Colbert Report* if one is not familiar with current events. Thus, some by-product learning may occur, especially for younger segments of the population, who have been able to avoid by-product learning as a result of high media choice.

These are minor quibbles. In the main, Prior’s noteworthy accomplishment is sure to be required reading for scholars and students interested in the media, turnout, political knowledge, and polarization. Both books do an excellent job of moving forward the debates about media concentration, media choice, and democracy and should be widely read.

Redistricting and Representation: Why Competitive Elections Are Bad for America. By Thomas L. Brunell. New York: Routledge, 2008. 160p. \$130.00 cloth, \$29.95 paper.
doi:10.1017/S1537592709090422

— Michael H. Crespin, *University of Georgia*

In this provocative and well-written book, Thomas L. Brunell introduces an original thesis: that in order to increase Americans’ satisfaction with Congress, we should draw congressional districts that heavily favor one party or

the other. The reason for this, Brunell argues, is that Americans are more satisfied with their own representatives and Congress as an institution when they are able to vote for the winning candidate. If we wish to maximize voter satisfaction, then the ideal system would work to maximize the percent of the population that has the opportunity to vote for a winner. To achieve this result, we should draw districts with as little ideological diversity as possible.

In the introductory chapter, Brunell begins to discuss why ideologically homogeneous districts produce better representation. If a member of Congress represents an ideologically diverse district, then she can only really be responsive to a portion of that district on any particular vote. For example, imagine a district where half the voters want higher taxes and half want lower taxes. No matter how the member votes, she is going to make half of her district unhappy. Now imagine another district where 80 percent want lower taxes and 20 percent want higher taxes. In this case, the member makes the easy vote for lower taxes and 80 percent of her constituents are happy while only 20 percent are not satisfied with their representation. Brunell thus argues that districts drawn to include only like-minded partisans increase voter satisfaction and make it easier for the representative to gauge the views of her constituents and transfer those views into votes. While these new districts might not be competitive at the general election, the threat of a primary challenge will ensure that members are responsive to their constituents.

In chapter 2, the author uses a Downsian framework to demonstrate that representation will be better (or suffer from less agency loss) in districts with less ideological variance. Heterogeneous, competitive districts, Brunell argues, must have higher variance compared to safe homogeneous seats. Therefore, constituents receive better representation when they reside in homogeneous and uncompetitive districts. This section of the book could be improved by a discussion of sub-constituencies. For example, there may be some issues that are salient for part of the district and other issues that are important to another group in the district. We can draw nice theoretical ideological distributions, but in reality, ideological variance may be more complex.

Next, the author uses survey data to test some of his key assumptions. Here he demonstrates that constituents are actually more satisfied when they vote for the winning candidate and, perhaps more importantly, there is little evidence that satisfaction is linked to competition. Although I find the tests compelling, I question the reliability of the data. According to table 3.1 (p. 36), over 77 percent of the respondents reported voting for the winner. This seems questionably high, even in an era of uncompetitive elections. So, if respondents are not truthful about their vote choices, can we believe what they say about how they rate their representatives? I would also like to see evidence that compares levels of satisfaction experienced