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# Morris Fiorina's Foundational Contributions to the Study of Partisanship and Mass Polarization

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## Introduction

Morris Fiorina is a giant of American political science. According to Google Scholar, as of June 2016, Fiorina has more than 23,000 citations, which almost certainly makes him one of the most well cited political scientists in the entire discipline. Few scholars in the past few decades have amassed such an impressive record of scholarly influence on such a wide variety of topics. Fiorina has written seminal works on congressional influence on the bureaucracy (Fiorina 1977), partisanship and retrospective voting (Fiorina 1981), the personal vote (Cain, Ferejohn, and Fiorina 1987), divided government (Fiorina 1992), voter turnout (Ferejohn and Fiorina 1974), political polarization (Fiorina, Abrams, and Pope 2005) and many other topics. One could write a detailed essay on how Fiorina's work has impacted the field in each of these areas; here, I focus on the two areas closest to my own work: his contributions to the study of (1) partisanship and retrospective voting, and (2) political polarization, particularly mass polarization. Even with just a focus on these particular questions, my essay will be a broad overview more so than a literature review of either area. This too is a testament to Fiorina's influence on the field: this work has been so important and influential that I cannot review it all in one place (for excellent literature reviews on retrospection, I refer the reader to Ashworth 2012; Healy and Malhotra 2013; for excellent literature reviews on polarization, I refer the reader to Layman, Carsey, and Horowitz 2006; Hetherington 2009).

## Partisanship: The Unmoved Mover or Retrospective Judgment?

Fiorina's *Retrospective Voting in American National Elections* is perhaps his most important publication. While many of Fiorina's works are extremely well

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cited, this piece is in a league of its own, with more than 4400 citations. There are only a handful of other political science books with more citations, and they all are ground-breaking works in the field, like Downs (1957), Campbell et al. (1960), Mayhew (1974) and the like. This testifies to Fiorina's place in the cannon.

But citations alone do not tell the story. This book makes two fundamental contributions to the literature. First, it provides a firmer basis for Key's (1966) arguments about retrospection. While the original idea of retrospection came from Key, Fiorina deserves credit for making it a fully articulated theory. Even if voters are not well-informed, they can use important clues from the world around them – whether unemployment is rising or falling, whether business is booming or busting, and whether we are embroiled in foreign wars – to know whether to reward or punish incumbents for their performance in office.

Second, it provides a political basis for partisanship, rather than the purely psychological one offered by Campbell et al. (1960). Through this same process of retrospection, voters not only update their view of politicians, but also their underlying partisan identification. So when Democrats in the 1960s blamed the party for Vietnam and urban unrest, they moved away from the party. Likewise, in the wake of 9/11, Americans affected by the disaster moved toward the Republican Party (Hirsch 2013). Partisanship itself is not immune to politics: it is not “something learned at mommy's knee and never questioned thereafter” (Fiorina 1981, p. 102). Rather, partisanship responds to political changes, albeit with a heavy dose of inertia.

Both contributions from the book spawned a lively literature in the years to come, particularly on the nature of partisanship. Was partisanship the “unmoved mover” of the Michigan school, or did it change in response to changing political circumstances, as Fiorina (and others like Jackson 1975) proposed? A great deal of ink was spilled throughout the 1980s and 1990s, much of it ably summarized in Green, Palmquist, and Schickler (2002), who mount the most rigorous challenge to the retrospective view. At the end of the day, the evidence shows that both views of partisanship are correct: “partisanship functions both as a stable *psychological* construct involving affective group attachment and as a temporary *political* summary judgment of group performance” (Lavine, Johnson, and Steenbergen 2012, p. 10, emphasis in original). The long-term stable, psychological construct is what Green and his colleagues highlight in their studies. This is why partisanship is so stable and resists change, and why it so fundamentally colors the way in which we perceive the political world (e.g. Bartels 2002). Yet on the other hand, partisanship is not immune to real-world changes. As political circumstances change – the Lewinsky Scandal, 9/11, the Iraq War, the great recession – so does partisanship. It takes a great deal to make partisanship change,

and there is a great deal of stability over time. But given the right circumstances, partisanship does move.

Similarly, the debate over voter's ability to vote retrospectively and hold incumbents accountable has also generated a vast and lively literature. This question is truly one of the central political science questions, because if retrospection does not work – if voters do not hold incumbents accountable for their performance in office – then democratic accountability is significantly weakened. After several decades of debate and discussion, what can we say – is there evidence for retrospection? The answer is a qualified yes: Voters reward and punish governments for the performance in office, particularly their handling of the economy (Besley and Case 1995; Bechtel and Hainmueller 2011; Kayser and Peress 2012). That said, voters also make mistakes, focusing too much on short-term economic conditions (Huber, Hill, and Lenz 2012; Healy and Lenz 2014), and punishing incumbents for irrelevant events such as shark attacks (Achen and Bartels 2016) or college football games (Healy, Malhotra, and Mo 2010). Further, because of the power of party ID as a long-standing psychological attachment, they also display partisan bias in holding incumbents accountable for the duties of their office (Malhotra and Kuo 2008). So while retrospection exists, there are also important limits to it.

This debate raises a key question: does retrospective voting lead to better outcomes? The literature does not provide a firm answer. For example, Ashworth and Bueno de Mesquita (2014) show that in some circumstances, voter “irrationality” (by, say, holding incumbents accountable for shark attacks) can actually improve democratic outcomes by changing incumbent behavior (see also Ashworth, Bueno de Mesquita, and Freidenberg Forthcoming). Of course, the flip side is that they show that at other times, it can make it worse as well. This point by Ashworth and Bueno de Mesquita, then, is a particularly crucial one: when does retrospection lead to normatively better outcomes? As Healy and Malhotra (2013) note, while we have considerable evidence of both retrospective voting and failures of retrospection, we know little about when retrospection increases aggregate social welfare (a reasonable criterion by which to judge “better” outcomes). Answering this question will be an important task for future research in this area.

## Political Polarization: How Divided Is the American Public?

Much of the interest in political polarization stems from the 2000 election, and the belief – widely touted in the media – that the election had ushered in a new

era of mass polarization between liberal coastal “blue” states won by Al Gore, and the heartland “red” states won by George W. Bush. While such claims were extremely common in the media’s post-election commentary, there were more often supported by anecdote and assertion than evidence. Interestingly, had journalists bothered to check, they would have found that sociologists had been researching this issue for some years, and had found little evidence of polarization (DiMaggio, Evans, and Bryson 1996; Baker 2000). Fiorina’s 2005 landmark book, *Culture War?: The Myth of a Polarized America*, was one of the first political science efforts to critically examine the red state/blue state divide, and more generally, the question of mass polarization (Fiorina, Abrams, and Pope 2005). Fiorina and his co-authors showed that red states and blue states were, in fact, quite similar (see also Levendusky and Pope 2011), and that ordinary Americans, far from being engaged in a broad culture war, agreed on many of the key issues of the day. In short, the mass public – unlike political elites – was not polarized. This book not only influenced the popular debate, but it launched an enormous political science literature. Here, I discuss one under-appreciated insight from this work, and several important findings that have helped to shape the field and spur on additional research.

Before launching into the ways in which *Culture War* has influenced the debate in political science, I want to draw attention to one of its most under-appreciated selling points. I’ve taught the book now for many years to a large number of students, and one thing that really sticks with them is Fiorina’s point that closely divided does not imply deeply divided. We cannot conclude from a close election that Americans are deeply polarized. The reason is that there’s a difference between polarized attitudes and polarized choices. Even if I’m a moderate voter, I still have to choose between two polarized candidates – Romney and Obama, Bush and Kerry, Clinton and Trump. Voters may well vote for someone who holds a number of positions they dislike (Pope 2012), so comparing choices can tell us little about voters’ underlying preferences. This insight has had somewhat less impact on political science, perhaps since many political scientists implicitly knew this already (even if they did not put it into those terms, see also Jacobson 2007). But yet in the mass media, many still use this sort of erroneous logic – see, for example, the discussion of Bernie Sanders’ 2016 campaign for the Democratic presidential nomination. But when students grasp Fiorina’s point, their eyes light up, and hopefully this helps them resist these sorts of media narratives. The book is full of insights like this that can stick with students and give them (and their professors!) deeper insight into American politics.

But not only does the book help to educate students, it has also reshaped the study of American politics in a number of ways. First, and perhaps most recognizable to political scientists, there is the question of whether Fiorina’s claim

of mass moderation is in fact correct. Alan Abramowitz is most closely associated with the argument that the mass public is actually polarized, contra Fiorina (see, e.g. Abramowitz and Saunders 2008; Abramowitz 2010; Abramowitz 2013). Unsurprisingly, Fiorina and his co-authors sharply dispute these charges (see, in addition to the works cited above, Fiorina, Abrams, and Pope 2008; Fiorina and Abrams 2008; Fiorina and Abrams 2009). But after carefully reading the evidence, one can make a compelling case that Fiorina and Abramowitz are both correct. Abramowitz is correct that some voters are indeed polarized – the most active and politically involved segment of the mass public. But Fiorina is also correct that most Americans remain quite moderate (see also Lelkes 2016). While Americans have not become more ideologically extreme, they have become better sorted: that is, partisans increasingly take their party's positions on a wide variety of issues. This is driven by a process of elite-cue taking, whereby ordinary citizens learn where they should stand on the issues of the day by looking at the more polarized positions taken by elites (Levendusky 2009).

Some readers may critique the distinction between sorting and polarization, and say that it is a difference of degree, rather than a difference of kind. But that argument is incorrect. To say the electorate is better sorted is to say that people know their party's views on the issues, though they need not be much more extreme. While political polarization necessarily implies sorting (if Democrats and Republicans are not on opposite sides, then the issue does not divide the parties), the electorate can be sorted without being deeply polarized (a point nicely captured in the analyses by Mason 2015; see also Levendusky 2009; Hill and Tausanovitch 2015).

That said, of course, one should be careful not to push the sorting argument too far: while sorting is real and consequential, not all voters are sorted, and many still take positions quite at odds with their parties, even on salient issues like abortion. And while elite polarization can help voters become somewhat more consistent (Levendusky 2010) – that is, their issue positions are more likely to agree with each other – there is not necessarily much more constraint, or understanding of why issue positions fit together, except perhaps among the most sophisticated segment of the electorate (Jewitt and Goren 2016). Indeed, many voters continue to hold issue positions at odds with one another: quite liberal on one issue, while deeply conservative on another (Zaller 2004; Broockman 2016).

While there's been a great deal of progress here, there is still an unresolved question posed by the different findings of Fiorina and Abramowitz. Abramowitz is correct that the most polarized are the most active, but what is the direction of that causality? The implicit premise lurking behind the Abramowitz findings, at least as I read them, is that extremism causes activism: individuals have strong beliefs that drive them into the political sphere, and the participation of extreme voters polarizes politics. Yet this is at odds with more qualitative research on

issue activists, which tends to find that people come into activism for apolitical reasons (e.g. they were asked by a friend), and as a result of that activism, their beliefs become more extreme (Munson 2009). In short, the causal arrow may run from activism to polarization rather than the reverse. Unpacking the direction of causality matters, because it changes how we think about efforts to ameliorate elite polarization. One commonly proposed solution to elite polarization is to inspire more moderates to become politically active, on the assumption that moderate voters will demand moderate candidates. If greater levels of political activity lead voters to become more extreme, greater political activity may, paradoxically, increase polarization (see also Mutz 2006).<sup>1</sup> A better understanding of the linkage between extremism and activism an important step to understanding whether more citizen participation would actually reduce elite polarization.

Second, Fiorina's *Culture War* and later works have shaped the literature by posing a central puzzle: how can we have polarized elites representing a moderate mass public? Numerous studies have confirmed the basic pattern documented by Fiorina and his co-authors: while elites are sharply divided, the mass public is much less so (Bafumi and Herron 2010; Jessee 2012; Hill and Tausanovitch 2015). This is a puzzling result for a discipline grounded in the logic of Downsian spatial competition (Downs 1957): if the mass public is centrist, why haven't elites converged to the middle?

So far, there is no clear answer. Most would likely agree that the solution lies in understanding the mechanisms through which electoral institutions translate shifts in voter opinion into outcomes (see the discussion in Fiorina and Levendusky 2006), but it is less clear which institutions and mechanisms actually are central. For example, it seems obvious at first glance that primary elections lead to elite polarization. Because only a handful of highly active and polarized voters turn out for primary contests, candidates work to appeal to their party's base, rather than to the center, and we end up with more polarized candidates. While intuitively appealing, there is very little evidence for this claim, either historically (Hirano et al. 2010), or in more recent periods (Bullock and Clinton 2012; McGhee et al. 2014; Ahler, Citrin, and Lenz 2016). To the extent that primaries matter, it seems to be in the incentives they give to party elites and interest groups (Masket 2007). Likewise, redistricting – another institution that would seem an obvious factor driving polarization – does little to increase polarization (McCarty, Poole, and Rosenthal 2009). This suggests that many of the processes may well be extremely subtle and difficult to uncover.

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<sup>1</sup> Further, as Ahler and Broockman (2016) note, it is not necessarily true that moderate voters will prefer centrist politicians.

There are two strands of literature that hold a good deal of promise for helping to resolve the mass-elite disconnect puzzle. First, there is the “reputational premium” of partisanship. Through a series of clever experiments, Sniderman and Stiglitz (2012) show that partisanship frequently trumps issue proximity. That is, a Democratic voter will vote for a Democratic candidate even when the Republican is unambiguously closer to her on the issues (see also Jessee 2012). In essence, a centrist voter may prefer an extremist from their side of the aisle to a centrist from the other party. Further, Sniderman and Stiglitz show that this tendency is even stronger among those who are sorted (that is, those who agree with their party on the issues), a finding also echoed in work by Mason (2015). So part of the solution to Fiorina’s disconnect comes from voters themselves: because voters ignore spatial proximity in favor of partisanship, there is little incentive for candidates to converge to the median voter.

Second, recent work also suggests that political donors are themselves quite extreme, even relative to politically active non-donors (Hill and Huber 2017). And even more troubling, legislators are particularly responsive to the positions of political donors (Barber 2016). Indeed, this might help to explain the disproportionate influence of the wealthy on legislator behavior, since the wealthy are the overwhelming source of campaign donations (Bartels 2008, p. 279–281). So extreme legislators may reflect extreme campaign donors, though more remains to be done to unpack and carefully test this proposition.

Both of these theories are not only plausible, they are probable. It is almost certainly the case that contemporary voters have a strong aversion to voting for candidates from the other side in an era of elite polarization, and nearly all political scientists (and most ordinary Americans) would be shocked if the extreme voices of campaign donors did not carry disproportionate weight in the minds of politicians. But I also suspect that the disconnect is deeper than that, and there are other factors yet to be explored that may help to explain it. While perhaps an unsatisfying conclusion for an essay like this one, it is more exciting for future scholarship, as it suggests there is much more important work to be done.

Third, while *Culture War* itself only focuses on polarization in terms of issue positions, it also helped to spur the study of a related phenomenon: affective polarization, or the tendency of partisans to dislike and distrust those from the other party (Iyengar, Sood, and Lelkes 2012). This means that many voters now dislike the elites from the other side, and evaluate them negatively (Hetherington, Long, and Rudolph 2016). But even more notably, individuals now dislike *ordinary voters* from the other side. For example, voters increasingly report discomfort with one of their children marrying someone from the other party or being close friends with someone from the other side of the political aisle (Iyengar, Sood, and Lelkes 2012; Abramowitz and Webster 2016).

This affective polarization is a consequence of partisan social identities. Whenever individuals identify with a social group – such a political party – it divides the world into in-groups (one’s own party) and out-groups (the other party), and they therefore show a marked favoritism toward those from their own group (on the role of group identities in politics more generally, see Huddy 2001).

While this group-centric thinking occurs naturally, several features of contemporary American politics strengthen this general tendency. Such effects are especially pronounced when identities are salient (Mackie 1986), or when the groups are in clear competition with one another (Huddy, Mason, and Aarøe 2015), both of which certainly describe the contemporary American political context. For example, with more polarized candidates, who offer voters starker choices, partisanship becomes more salient. Likewise, media coverage emphasizing polarization, especially on cable news outlets and the Internet, paints a picture that the parties are locked in a struggle for control of the government, and with it, policies (Levendusky and Malhotra 2016a; Lelkes, Sood, and Iyengar 2017). In essence, the parties have become “teams,” and voters know that their team is the good one, and the opposing one is the bad one, and they behave accordingly (Huddy, Mason, and Aarøe 2015; Mason 2015).

Such affective polarization might be a mere interesting footnote if it did not affect behavior. Unfortunately, it seems to have relatively profound implications for behavior. Prejudice against members of the out-party now exceeds racial prejudice (Iyengar and Westwood 2015). Further, affective polarization also leads to gridlock. Political trust is now highly segmented by party: we trust the government when our own party is in power, and we do not when the other party is in control. While this has long been true (Keele 2005), it is especially true today in an era of affective polarization (Hetherington and Rudolph 2015). As a result, there is less support for reaching across the aisle to find compromise and consensus (Hetherington and Rudolph 2015), especially among those who are the most partisan (Harbridge and Malhotra 2011) or are sorted (Mason 2015). This sort of affective polarization, then, can have some troubling consequences.

This work raises two important considerations for future research. First, is this phenomenon more about in-group favoritism or out-group derogation? A long line of work in social psychology suggests that these types of patterns are typically about liking one’s own party more than disliking the opposition (Brewer 1999). Lelkes and Westwood (2017) find similar evidence here. While affective polarization leads individuals to favor members of their in-group, and embrace negative stereotypes of the opposition, even the most affectively polarized stop short of doing harm to the other side (i.e. supporting police action or heavy fines against members of the opposing party protesting a government action). McConnell et al. (2016), using several different field experiments, find

the same pattern. This is important because it suggests that there are limits to partisan prejudice, and these patterns, while important and troubling, are not as normatively deleterious as they might seem at first glance.

But that said, affective polarization is still a normative bad. Are there ways of overcoming or ameliorating partisan prejudice and animus? For example, Ahler and Sood (2016) document a fascinating tendency for people to hold wildly inaccurate beliefs about members of the other party (see also Levendusky and Malhotra 2016b). For example, while only about 6% of Democrats are LGBT, Republicans assume that figure is almost 40% (See Figure 2A in Ahler and Sood 2016). When given the correct information, people feel more warmly toward those from the other party – incorrect stereotypes of the opposition partially fuels this animus. Further, some of my own work highlights how emphasizing an identity shared by both Democrats and Republicans – such as an American identity – can reduce affective polarization. When individuals are reminded that those from the other party are also Americans, they become part of a shared in-group, rather than an opposing out-group, and attitudes toward them improve (Levendusky Forthcoming). What other strategies might be used – and how they could be implemented – is an important topic for future research.

As this brief essay makes clear, Fiorina's work has shaped the debate in political science for decades, and will do so for many years to come. I close with three queries for Professor Fiorina himself. First, if he had to re-write any of these works seeing where the debate would go, what, if anything, would he change? Is there some point he would put differently, or a place where he would change his thinking if given the opportunity? Second, I've reviewed two key areas of his work. Does he see any areas of synergy between them? For example, does sorting affect retrospective voting (Healy and Malhotra 2013)? Does sorting give new insight into the debate over the stability of party ID? Finally, in this essay, I've outlined some challenges as I see them for future work. What questions does Fiorina himself see as being the most important ones to be answered?

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