

The structure of this book reveals its unique methods. Part I, “The Problem,” begins from specific challenges confronting Western identity. Through the French headscarf debate and the Swiss minaret referendum, Chabal dives into fraught debates around multiculturalism, but rather than endorse either a British or a French model for integration, he instead anchors their shared incomprehension toward Islam in a peculiarly individualized secularism. This then opens outward onto Part II, “Identities,” which grapples with what the *West* *thinks* it is: autonomous, rational, scientific, governed by individual rights and the market. But crucially, this self-perception draws its content from opposition to a non-West that ostensibly lacks these attributes, through a historical linearity in which “they” represent “our” own past.

Part III, “Ideas,” interrogates central components of Western self-identity—individual, society, freedom, faith, the market, and change—pressing away from this linear historical vision in which the secular individual embodies the apex of human progress. Part IV, “Interpretations,” then shifts from this Western self-conception to its underpinnings: a problematic relationship between science and theory in which—à la behavioralism—the presumption of scientific validity conceals ideology, bestows legitimacy, and placates our own self-conceived superiority. Against this, Chabal reformulates a theory that builds into itself the very relativism, linguistic incommensurability, and contingencies that he emphasizes. The book concludes with a brief “postcolonial” critique of secularism, human rights, and sovereignty, demonstrating that it is rational to question the West’s monopoly over such concepts.

In each section, the uniqueness of Chabal’s approach is on full display, and the strength of his text is a certain naïveté. He can think creatively precisely because he has not enclosed himself within a rigid methodology, instead approaching big questions without the overbearing weight of a preestablished theoretical apparatus. He picks up a question and looks at it from all angles, raising three or four aspects to be interrogated, and moves through the analysis in an unencumbered way. He is at his best, moreover, when defying contemporary pieties to make counterintuitive arguments: defending, for example, Max Weber’s emphasis on culture or Samuel Huntington’s notion of civilization against secular individualism, or finding grounds for optimism in a headscarf debate that “ripped to pieces the French assumption that ethnicity does not exist” (p. 174).

This virtue also becomes a vice, however, and Chabal’s willingness to tackle political questions as a novice, to clumsily open up theoretical vistas, frustrates the reader. Often he circles around a question without ever landing, and at other times he tramps down well-trodden dirt as though it were a novelty. When he speaks of David Beckham’s black aesthetic, or refers briefly to Eminem or *The Wire*, it becomes clear that he is simply out of his

depth and prone to oversimplification. More seriously, the author brings no systematic analysis of race and colonialism, and little appreciation of the historical dynamics of cultural appropriation. White teenagers donning dreadlocks therefore suggest a potentially radical transformation of racial identities, rather than their confirmation (p. 63).

A second worry stemming from Chabal’s method is the absence of thinkers who have long made similar arguments, and in sharper terms. Thus, he quickly dismisses much postcolonial theory as merely the “flip side” of the Western approach to theoretical canons, dedicated to “berating Western arrogance from the safe confines of the Western academy” (p. 117). However, like many such critics, Chabal substitutes a critique of a small part for a critique of postcolonial thought in toto (as does Vivek Chibber, *Postcolonial Theory and the Specter of Capital*, 2013). Obscured in the process are thinkers like the Latin American coloniality school, notably Aníbal Quijano and Enrique Dussel, who have critiqued subaltern studies in similar terms and sought to craft alternatives. Also surprisingly absent in Chabal’s account of the Manichaeism of West and non-West—the colonial vestiges still very much alive in the present, not to mention the politics of the veil—is any reference to the thinker who has explored these questions in more depth than any other: Frantz Fanon.

These absences are not inconsequential, but instead point toward a deeper difficulty in the text. Chabal seeks to confront the prevailing dogmas of the West, but instead comes to overly modest and even tepid conclusions. This is perhaps clearest in the book’s title: Is ending “conceit” a useful or sufficiently ambitious goal? Worse still, is his emphasis on rationality hopelessly idealist? If rationality helps to grasp the French response to the riots, it fares far worse as an explanation of the *banlieues* to begin with, and much less of the historical emergence of West and non-West. To focus on rationality is to take Western concepts at face value, as though secularization, human rights, and sovereign individuality were not historically and conceptually intertwined with the slaughter of heathen non-rights-bearing-subjects.

**The Timeline of Presidential Elections: How Campaigns Do (and Do Not) Matter.** By Robert S. Erikson and Christopher

Wlezien. Chicago: University of Chicago Press, 2012. 201 p. \$75.00 cloth, \$25.00 paper.

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— Lynn Vavreck, *University of California—Los Angeles*

In *Double Down: Game Change 2012*, Mark Halperin and John Heilemann report that Neil Newhouse, Mitt Romney’s pollster, “was certain that no convention could have appreciable impact on voters’ perceptions of whether the country was on the right or the wrong track,” (p. 391).

After the Democratic convention in Charlotte, however, as public opinion dramatically shifted in favor of Barack Obama, Newhouse was forced to re-evaluate his certainty. Conventions, it seems, can matter quite a lot.

The shift toward Obama in September of 2012 would not have been a surprise to Newhouse and his colleagues if they had read an advance copy of Bob Erikson and Chris Wlezien's *The Timeline of Presidential Campaigns* (published in October of 2012). Amassing every publicly available trial-heat poll from election years between 1952 and 2008, Erikson and Wlezien search for patterns in the polls that suggest whether and how presidential campaigns affect election outcomes. In doing so, they bring the tools of time series analysis to bear on the fundamental tension that haunts every scholar, reporter, or consultant trying to understand the effects of campaigns: how much of the final outcome is determined by what the candidates do (or what happens to them) in the weeks leading up to the election and how much is driven by the things out of their control, like the state of the nation's economy, the distribution of party identification in the country, or presidential approval? To describe this tension another way, are elections mainly determined by the *choices* candidates make or the *chance* circumstances in which they find themselves?

The answer, of course, is both—and Erikson and Wlezien add their voices to the small but growing set of scholars showing how the process of persuasion in campaigns is slow—and built on solid foundations set in place before the candidates are even known. Despite the media's obsessions with game-changing campaign moments, very few “games” are changed by a single, transformative event happening in the 300 or so days before presidential elections. Nowhere is this revealed in starker terms than in *Timeline*, which leverages the weight of 15 contests, 30 major-party candidates, and generations of voters spanning 60 years. This design lends gravitas to the argument and is a welcome antidote to trendy work on campaign effects that relies on survey-experiments in isolation of competition and is increasingly disconnected from campaign realities.

The book makes three important points. The first is simply to illustrate for readers the impressive aggregate-level stability that defines most election-year poll results and how closely those polls are tied to eventual outcomes. Beginning in April of each year and going through to November, Erikson and Wlezien show that in each of the 15 years they investigate, the early polls (even several hundred days out) do a pretty good job of characterizing the eventual outcome, but more importantly, as the polls change over the months of the election year and Election Day approaches, the change in poll results is systematic and slow. You would be hard pressed to find a game-changer in any of these elections since 1952. Political pundits, take note.

This is not to say that things don't move around; they do. And Erikson and Wlezien do us a great service by separating the types of changes found in aggregate poll results over the years into two types: bounces (in which poll results go up but eventually back down) and bumps (in which the shift in poll numbers is permanent). Bounces are largely ignorable, unless they happen in the final days of the campaign. But bumps—bumps can be important and interesting, but they don't happen very often, and when they do, it's typically because of something predictable like a convention.

In an unexpected but delightful chapter, the authors abandon their aggregate data for individual-level panel data from the American National Election Studies (and Gallup, too). Using these data, they offer even more evidence that change happens, but not to the degree that pundits and political reporters suppose. Using these data, Erikson and Wlezien put an upper bound on the number of voters who change their minds. Just how many voters switch their party vote at least once *between election years*? The largest shift, given the years for which they have data, comes between 1972 and 1976, as 21 percent of Nixon voters in 1972 report voting for Carter in 1976; and 3 percent of McGovern voters switch to Ford. If roughly a fifth of the electorate is switching *between* elections, the authors argue, it seems unlikely that any more than that are switching *within* a single election year. Similar numbers are found when they investigate party defection within a campaign year—about a fifth of the electorate, on average, abandons their party candidate early in the election year (April), but slowly and steadily, those wandering voters come home—and by November, more than half of them have returned to their original party choice.

Finally, *Timeline* delivers on the promise in its title—it reveals the systematic, predictable ways in which voters are affected by campaigns in the 300 days leading up to election day—and how three important moments in the timeline are crucial for understanding election outcomes. The first is the early part of the election year, about 300 days before the election, when the nominating process is in full swing and the candidates are just being introduced to most voters. Erikson and Wlezien find a lot of shifts during this period, as people's vote choice initially takes shape and the process through which information is translated into decisions may be a little noisier for most people (they don't yet know what considerations to give the most weight to because the campaigns are not yet in full swing).

After this initial period, the next important time is the conventions. During this period a massive amount of information is revealed to voters and the fact that the election is right around the corner becomes difficult to ignore. It is during and after the conventions that the final outcome starts to solidify in the trial heat polls—partisans come home to their party candidates and

undecided voters who eventually will drop out begin to do so. Finally, the last few days before the election are a campaign's final chance to nudge the outcome their way because bounces can be consequential if the election is very close.

If you study presidential politics or time series analyses, there is a lot to like in *Timeline*. The connection between the method and the substance is close and tight, which makes this book a great example of how the right method can help illustrate important nuances in the substance of a problem. For example, the thoughtful discussion of whether the polling time series is stationary or integrated helps illustrate the important differences between bounces and bumps. But by far, the most important contribution the book makes is to illustrate that presidential campaigns matter in *predictable* ways, and voters' intentions evolve *incrementally* over the course of the election year.

Election outcomes don't always reflect where the polls start in April, but they always begin to reflect the outcome early in the year. There are no outcome-changing gaffes, only underlying fundamentals and campaigns that help voters make sense of the state of the world around them. To be clear: for Erikson and Wlezien, presidential campaigns matter—despite the regularity of attitude change in elections, the campaigns are an (if not *the*) important catalyst in the process. Without them, the noisiness of decision-making might grow instead of shrink. The regularity comes from something—and it's unlikely that thing is just the passage of time or the proximity of Election Day.

### **Compromise: A Political and Philosophical History.**

By Alin Fumurescu. New York: Cambridge University Press, 2013. 305p. \$90.00.

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— Ryan Patrick Hanley, *Marquette University*

Daily life as a twenty-first-century American citizen is a continual testament to our profound need for guidance on the subject of this insightful and meticulously researched book. Its primary aim is to provide what its subtitle calls “a political and philosophical history” of the idea of compromise—or, as Alin Fumurescu elsewhere has it, a “conceptual genealogy” (p. 7) broadly conceived as an exercise in *Begriffsgeschichte*. As such, while hardly insensitive to contemporary normative concerns, it presents itself first and foremost as an effort to remedy a “lack of historical contextualization” (p. 14) and a “rediscovery of the forgotten genealogy of compromise” (p. 18), particularly as it emerged in Britain and France between the sixteenth and eighteenth centuries.

The main argument of the book is that compromise is today understood in two quite different senses, one positive or “commendable” and the other negative or “condemnable” (p. 19), and that the origin of this “dazzling

discrepancy” (p. 5) or indeed “radical split” (p. 8) can be “almost pinpointed” (p. 6) to the way this concept evolved in the two very different contexts of Britain and France between the late Renaissance and Enlightenment periods. The positive sense refers to the view of compromise as a political virtue that enables two distinct entities to resolve disagreements without resorting to force and violence, and it was in this sense that the concept emerged in Britain. The negative sense refers to the view of compromise as the violation of the essential integrity of one's inner self or self-conception, and it was in this sense that the concept evolved in France.

Why this difference? The author's argument is that “two different kinds of individualism” developed in France and Britain (p. 64; cf. p. 158), which he labels “centripetal individualism” and “centrifugal individualism.” What defines these are two different approaches to “the dialectic of the individual between *forum internum* and *forum externum*” (pp. 19–20). The significance of this dialectic for the project as a whole cannot be overemphasized; indeed, perhaps the work's chief aim and its chief scholarly contribution is its recovery of this “now forgotten dialectic” (p. 10; cf. pp. 24, 46, 95, 100, 116, 131, 267, 269). In brief, *forum internum* is “the forum of conscience, authenticity, and freedom, subject to no one and punishable by no one except God,” whereas *forum externum* is that “in which the individual identified himself and was identified through belonging to one of several communities” and was “liable to judgment and punishment by the community” (p. 10). With that claim in place, the book argues that the French context that privileged *forum internum* promoted the development of a negative view of compromise, whereas the British context that privileged *forum externum* promoted the development of a positive view (e.g. pp. 11, 193, 269).

Compromise is thus “a concept at the crossroads between representation and self-representation” (p. 91; cf. pp. 4, 195), and collectively the nine chapters that make up the work provide a useful road map to these interconnections. Especially valuable are its illuminations of the ways in which the concept of compromise was decisively shaped by medieval conceptions of the dialectic of the individual, as well as by the differing theories and practices of representation and contractualism in France and Britain in the early modern period.

Aside from these substantive insights, three additional strengths of the book deserve explicit mention. The first is the author's impressive erudition. This book covers a remarkable amount of ground, examining concepts from representation to individualism to sovereignty to contractualism, as well as thinkers from Aristotle to Avishai Margalit. It also makes good on its promise to provide “dozens of examples from each side” (p. 19) of the concepts of compromise it aims to illuminate. Coupled with the book's detailed and thorough coverage of the secondary literature is the amount of labor that must have gone