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Critical Junctures and Historical Legacies

Insights and Methods for
Comparative Social Science

Edited by David Collier and Gerardo L. Munck

ROWMAN & LITTLEFIELD
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Introduction

Tradition and Innovation in Critical Juncture Research

David Collier and Gerardo L. Munck

For half a century, the study of critical junctures and their legacies has been an influential and fruitful research tradition within the broader enterprise of comparative social science. Launched by Lipset and Rokkan in 1967, it is traceable to Weber's (1946 [1922–1923]: 280) idea of historical “switchmen” who select the “tracks along which action is pushed.”

This tradition is based on a theoretical framework centered on the idea that critical junctures are major, macro-level episodes of change that yield enduring legacies. It has been closely associated with comparative historical research and the use of qualitative methods. And it has explored fundamental substantive questions about a broad array of topics, including state formation, political regimes and democracy, party systems, public policy, government performance, economic development, and cultural norms.¹ Research on critical junctures has been and remains a vibrant tradition in the social sciences.

At the same time, the field has undergone important changes and several questions have been raised about this tradition. Interest in the critical juncture framework has led to many valuable refinements. However, the growing literature has created confusion about some of its central ideas. For example, scholars hold different views regarding whether critical junctures are only big changes, necessarily involve contingent events, always comprise institutional changes, and consistently lead to divergent outcomes. Further, scrutiny of this framework has led to some critiques. Most importantly, students of incremental change argue that critical juncture research is flawed because it underplays the role of agency, focuses excessively on exogenous shocks to explain change, and neglects gradual change.

1. For a comprehensive list of works in this tradition, see Appendix III.

Questions have also been raised regarding how claims about critical junctures are empirically assessed. Discussions about the use of qualitative methods have introduced both improvements and critiques of these methods. Moreover, a literature using quantitative methods that is relevant to the study of critical junctures—in that it tests hypotheses about long-term, persistent effects—has begun to take shape in recent years. As with discussions about the framework used in this research, new and not always compatible ideas have been introduced about how empirical research should be conducted.

This volume contributes to research on critical junctures by offering a critical review of the key theoretical and methodological ideas used in this field. As we will show, some old ideas are still useful, while others are not and should be discarded. Some recent proposals are genuine advances and should be incorporated into the tradition's repertoire, whereas others should be discarded. Some critiques are valid and should be taken seriously, but others should not. Also, some new ideas that are not part of the recent discussion merit consideration. Thus, based on a careful, balanced assessment of the field, this volume reaffirms the value of many traditional practices and specifies how the field can build on proven ideas, while embracing the need for innovation and opening new frontiers.

We will also place a strong emphasis on substantive research. Debates about theory and methods are necessary and important. However, when delinked from substantive concerns they become sterile. Moreover, research on substantive questions offers the clearest indication of the value of a research tradition. Thus, this volume presents many examples of substantive research. These examples illustrate how researchers draw on the critical juncture framework in developing arguments and rely on various methods in assessing these claims. They also show how this research produces knowledge about how big structures originate, endure, and change, using a macro-historical perspective to elaborate original explanations that are crucial to an understanding of the world's past and current problems.

This chapter introduces the volume. We first situate research on critical junctures within the historically oriented social sciences and identify its key aims and characteristics. We then consider debates and disagreements concerning the framework and methods, and spell out the book's approach to contrasting views about this research. Finally, we present a detailed overview of the book, mapping out its organization and briefly discussing each chapter.

AIMS AND CHARACTERISTICS OF CRITICAL JUNCTURE RESEARCH

Research on critical junctures is part of a larger social science agenda that focuses on temporality and challenges presentism or short-termism, i.e., the assumption that patterns of the current world can be explained by causes in the recent past (Pierson

2004: 45–46, Ch. 3; Rueschemeyer 2009: 147–51; Guldi and Armitage 2014).² This agenda is advanced in broad fields of research that have been identified by the labels of (1) comparative historical analysis, a tradition launched in the 1960s that bridges sociology, political science, and history;³ (2) historical institutionalism, a newer tradition begun in the 1990s and based largely in political science;⁴ and (3) the new institutional economics, an approach pioneered by economists that achieved prominence throughout the social sciences in the 1990s.⁵ Thus, research on critical junctures is not a self-contained field. Many concerns that motivate this research, and many of its ideas and methods, are shared by a larger community of scholars. This research is part of a large intellectual movement.⁶

The critical juncture tradition itself is wide and diverse. However, it can be characterized in terms of three features: (1) a theoretical framework, (2) research methods, and (3) substantive questions.

(1) *Framework*. Research in this tradition relies on a framework that is built around two key ideas. One is that critical junctures are a distinct kind of change,

2. See also Rueschemeyer et al. (1992: 7, 23, 35), Reid (2011), and Akyeampong et al. (2014).

3. For overviews of comparative historical analysis, see Skocpol (1984), Collier (1998), Mahoney and Rueschemeyer (2003), Ritter (2014), Mahoney and Thelen (2015), and Møller (2017). On the temporal concepts used in comparative historical analysis, see Kreuzer (2013, forthcoming).

4. Historical institutionalism overlaps in part with comparative historical analysis and some scholars (e.g., Skocpol) play a key role in both traditions. In addition, many of the works cited as examples of historical institutionalism are also routinely treated as part of comparative historical analysis. Indeed, there is considerable overlap in the works discussed in two texts on comparative historical analysis (Mahoney and Rueschemeyer 2003; Mahoney and Thelen 2015) and two texts on historical institutionalism (Steinmo 2008; Fioretos et al. 2016). For overviews of historical institutionalism, see Steinmo et al. (1992), Thelen and Steinmo (1992), Thelen (1999), Pierson and Skocpol (2002), Steinmo (2008), Fioretos et al. (2016), Rixen et al. (2016), and Fioretos (2017).

5. For overviews of the new institutional economics, see Richter (2005), Bates (2014), Levi and Menaldo (2015), and Alston et al. (2018). The new institutional economics is an encompassing field, much of which has little resonance with critical juncture research. However, the strand of the new institutional economics associated with North is certainly part of the historical turn in the social sciences. On North's contribution, see Galiani and Sened (2014).

6. Three strands of this literature, which might be fit under the three broader traditions just noted, are American Political Development (APD), which adopts a historical perspective to the study of U.S. politics; legacies research, which has focused among other things on postcommunist and postcolonial societies; and historical political economy, which combines economic theories and quantitative historical research. On American Political Development, see Mayhew (2002) and Orren and Skowronek (2004). On legacies research, see Simpser et al. (2018). On historical political economy, see Gailmard (2021).

an idea that has been conveyed in different terms. Some authors conceive of these as “periods of radical change” (Rokkan 1970, in Flora 1999: 37), “crucial founding moments” (Thelen 1999: 387; see also Ikenberry 2016: 541), or “large-scale change” (Katznelson 2003: 282). Others see them as “discontinuous . . . macro transformations” (Collier and Collier 1991: 11), “discontinuous . . . massive changes” (Weingast 2005: 164–65), or “changes that are . . . abrupt [and] discontinuous” (Roberts 2014: 43). Although using different terms, scholars have generally understood critical junctures as large, rapid, discontinuous changes.

The other main idea in the framework is that critical junctures produce a distinct kind of causal effect, usually called a historical legacy. One basic point is that the effect of critical junctures is “frozen over long periods of time” (Rokkan 1970, quoted in Flora 1999: 36) or, more simply, is “durable” (Roberts 2014: 42). A related point is that this effect “reproduces itself without requiring the recurrence of the original cause” (Scully 1992: 12) and does so through distinct “mechanisms . . . that reproduce the legacy” (Collier and Collier 1991: 35, 37). Thus, a critical juncture is a change that produces an effect that persists over a long time, through mechanisms of reproduction, after the initial cause has ceased to operate.

Not all authors conceive of critical junctures and historical legacies in the same way. However, there is considerable consensus in the literature on these basic points.⁷

(2) *Methods*. Research on critical junctures has relied heavily on qualitative methods. All of the twentieth-century exemplars are based fully or largely on qualitative research (e.g., Moore 1966; Lipset and Rokkan 1967; Collier and Collier 1991; Baumgartner and Jones 1993; Putnam 1993). Furthermore, inasmuch as this research has been methodologically self-conscious, it has drawn on general discussions about qualitative methodology (Smelser 1976; King et al. 1994; Brady and Collier 2010) and on reflections regarding the study of historical processes (Collier and Collier 1991: Ch. 1; Grzymała-Busse 2011; Mahoney and Thelen 2015: Parts III and IV).⁸

(3) *Substantive Questions*. Finally, research on critical junctures is characterized by its focus on big, fundamental, questions or what has been described as first-order historically enduring questions (Rule 1997: 45–48; Laitin 2004: 33–36) or, more simply, “big problems” (Lichbach 2009: 29–31). The following examples of actual research give a sense of the centrality and range of the substantive questions addressed in critical juncture research.

7. For an overview and comparison of different conceptions of critical junctures and their historical legacies, see Appendix I.

8. See also Mahoney and Rueschemeyer (2003: Part III), George and Bennett (2005), Capoccia and Kelemen (2007), Bennett and Checkel (2015), and Møller and Skaaning (2021).

- Why do some countries have weak states and others have greater state capacity?⁹
- Why do some countries have democratic regimes and others authoritarian regimes?¹⁰
- Why do some countries have high-quality democracies and others low-quality democracies?¹¹
- Why do countries have different party systems?¹²
- Why do some countries have governments that perform well and others governments that perform poorly?¹³
- Why do policy monopolies—the dominance of public policy by the same group of policymakers and organizations—develop and endure in the U.S. government?¹⁴
- Why do some countries have legally enforced racial segregation and others do not?¹⁵
- Why are some countries prosperous and others are poor?¹⁶
- Why were developmental states established in some Asian countries and not others?¹⁷
- Why did the West become dominant in the world?¹⁸
- Why have international orders varied in character over time?¹⁹

In short, a considerable body of research in this field has been produced since the 1960s. This research has relied on a framework centered on the idea of critical junctures and historical legacies. It has been strongly associated with qualitative research and has addressed many key substantive questions in the social sciences. As a result of the work of a large community of scholars, critical juncture research has become an established tradition in the historically oriented social sciences (Thelen 1999; Pierson 2004: Ch. 2; Capoccia 2015; 2016).²⁰

9. Skocpol (1979), Mann (1986), Tilly (1990), Waldner (1999), Kurtz (2013), Soifer (2015), and Mazzuca (2021).

10. Moore (1966), Collier and Collier (1991), Jalal (1995), Yashar (1997), Gould (1999), López-Alves (2000), Mahoney (2001), and Tudor (2013).

11. della Porta (2016), Fishman (2019), and Mazzuca and Munck (2020).

12. Lipset and Rokkan (1967), Collier and Collier (1991), Scully (1992), Bartolini (2000), Riedl (2014), Roberts (2014), and Caramani (2015).

13. Putnam (1993).

14. Baumgartner and Jones (2009).

15. Marx (1998).

16. Waisman (1987), North and Weingast (1989), Engerman and Sokoloff (2012), and Acemoglu and Robinson (2012).

17. Chibber (2003), Kohli (2004), and Vu (2010).

18. Diamond (1997), Mitterauer (2010), and Acemoglu and Robinson (2019).

19. Ikenberry (2001).

20. See also Fioretos (2017: Ch. 1) and Berntzen (2020).

DEBATES AND DISAGREEMENTS ABOUT THE FRAMEWORK AND METHODS

The growing literature on critical junctures has also given rise to some new discussions. Since around 2000, many scholars have advanced several ideas about the framework and methods used in this research. Some have made proposals for changes that neatly supplement and clearly add to the repertoire of ideas associated with this tradition. However, others have made suggestions that are not compatible with earlier ideas or are themselves mutually incompatible. This new literature reflects strong interest in the study of critical junctures. It also has created some confusion, controversy and, ultimately, uncertainty about the framework and methods used in critical juncture research.

Weighing in on these discussions is crucial in a volume that seeks to suggest how this research should move forward. Here we preview the key issues this volume will address. We lay out contrasting views presented in the literature and briefly anticipate the specific positions supported by the authors in the following chapters. We likewise offer a more general statement about how these issues and debates are approached in the book.

Theoretical Framework

Some key disagreements concern the framework used in substantive research. Until the 1990s, this research relied on a set of ideas introduced by Lipset and Rokkan (1967) and others, that collectively shaped a distinctive approach to historical causation.²¹ These ideas provided useful guidance for research. However, as the literature on these ideas has expanded, questions have arisen about the core components of the critical juncture framework. The idea of a tradition relying on a common theoretical

21. Lipset and Rokkan (1967: 3, 50–53) presented the concepts of critical juncture and cleavage structure, and a somewhat vague notion of the “freezing” and “entrenchment” of outcomes of critical junctures. Stinchcombe (1968: 101–25) specified the distinctive causal structure—a self-replicating causal loop—that reproduces an outcome over time. David (1985) launched the related idea of path dependence, and both David (1985) and Arthur (1989; 1994) supplemented this general idea with the more specific idea of increasing returns that lock in past choices. North (1990) linked the idea of path dependence to the study of institutions and institutional change. And Collier and Collier (1991: Ch. 1) offered a synthesis that integrates the ideas of antecedent conditions, cleavage or crisis, critical juncture, aftermath, and historical legacy.

Some researchers use the cognate idea of punctuated equilibrium, drawn from research in evolutionary biology pioneered by Eldredge and Gould (1972) and developed extensively by Gould (2007). Krasner (1984; 1988) played a central role in introducing these ideas in political science. Other researchers use what Gellner (1964: 42–49) calls a neo-episodic model of change. These various frameworks share some core ideas.

structure has also in some respects been cast in doubt. Even the value of this research has sometimes been questioned.²²

Critical Junctures and Legacies

One source of uncertainty involves the basic idea of a critical juncture and its legacy, the core concepts in this field. Although we find substantial consensus about how critical junctures and legacies should be understood, different views are defended in the literature.

First, scholars disagree on whether critical junctures are large-scale, rapid, discontinuous changes. Most hold that critical junctures are only macro-level events that introduce discontinuous change, that is, some new property or a qualitative novelty (Lipset and Rokkan 1967: 13–15, 47; Collier and Collier 1991: 11; Katznelson 2003: 282).²³ However, an influential view, especially among scholars who draw on the idea of path dependence, is that small rather than big changes can be critical junctures (Mahoney 2000: 526; Pierson 2004: 44, 50–52; Greer 2008: 220–21).²⁴

Second, scholars also diverge on whether critical junctures are wholly contingent events, that is, events in which actors are not constrained by antecedent conditions. Numerous authors hold that critical junctures are often strongly shaped by precisely such antecedent conditions (Collier and Collier 1991: 27; Katznelson 2003: 282–83, 291–92; Pierson 2004: 51–52).²⁵ They also stress that the extent to which critical junctures inherently entail contingency is best treated as a variable factor to be addressed in the context of substantive research.

However, growing attention to how path-dependent processes originate has generated an interest in the decisive role of actors' choices. Several authors claim that critical junctures are a distinct kind of cause—one in which actors' choices cause outcomes but are themselves not caused—and that such fully contingent choices

22. The new literature includes, among others, Thelen (1999; 2003; 2004), Mahoney (2000; 2001), Pierson (2000; 2004), Katznelson (2003), Orren and Skowronek (2004), Streeck and Thelen (2005b), Hogan (2006), March and Olsen (2006), Boas (2007), Capoccia and Kelemen (2007), Djelic and Quack (2007), Greer (2008), Howlett (2009), Sydow et al. (2009), Mahoney and Thelen (2010), Slater and Simmons (2010), Soifer (2012), Bernhard (2015), Capoccia (2015; 2016a; 2016b), Hacker et al. (2015), Rixen and Viola (2015), Sarigil (2015), Wittenberg (2015), Conran and Thelen (2016), Fioretos et al. (2016), Hall (2016), Collier and Munck (2017), Acharya et al. (2018), García-Montoya and Mahoney (2020), and Gerschewski (2021).

23. See also Roberts (2014: 43) and Ikenberry (2016: 540–41).

24. See also David (1985: 332) and Arthur (1989: 117).

25. See also Weingast (2005: 165–66), Martin and Sunley (2006: 402–03), Slater (2010: 55), Slater and Simmons (2010: 887–90), and Soifer (2012: 1574, 1593).

should be treated as a defining characteristic of critical junctures (Mahoney 2000: 507–08, 511, 513; Capoccia and Kelemen 2007: 343, 348; Bernhard 2015: 978).²⁶

A third source of disagreement concerns the causal structure through which a critical juncture generates a legacy. The classic position, articulated by Stinchcombe (1968: 101–25), holds that the historical legacy of a critical juncture is, in causal terms, a persistent or enduring effect that is created by a self-replicating causal loop (Howlett 2009: 252–53; Sydow et al. 2009: 697–98; Rixen and Viola 2015: 317). In contrast, a more recently formulated position is that a legacy can also be produced by a distinct kind of path dependence, one involving “reactive sequences” that generate a long-term effect but not one that reinforces a prior outcome and involves persistence (Mahoney 2000: 509, 526–35; Arestis and Sawyer 2009: 11; Beyer 2010: 4–5; Martin and Sunley 2010: 84–85).

A well-vetted and precise understanding of critical junctures and their legacies is a basic requirement for this field of research. These controversies are addressed in this volume, by D. Collier (Chapter 1), Munck (Chapters 5 and 19), Riedl and Roberts (Chapter 6), and Waldner (Chapter 7). On the first source of disagreement, D. Collier (Chapter 1) and Munck (Chapter 5) defend the traditional conception of critical junctures as rapid, discontinuous, macro-level changes. On the second point, D. Collier (Chapter 1), Riedl and Roberts (Chapter 6), Waldner (Chapter 7), and Munck (Chapter 19) make a case for not treating contingent choices as a defining feature of critical junctures. Finally, on the third point, Munck (Chapter 5) defends the standard conception of critical junctures as changes that create a persistent effect through a self-replicating causal loop. This volume thus argues that many earlier ways of thinking about this field definitely remain useful.

However, as stressed by Munck (Chapter 5), the current discussion shows that we need a better theoretical foundation for critical juncture research. Indeed, the diversity of views regarding essential concepts is a sign that basic issues have not been properly addressed and that the framework should be given stronger foundations. Thus, Munck provides a thorough discussion of key issues and shows why some views about critical junctures and legacies should be adopted while other (earlier and newer) ideas should be discarded. Regarding erroneous ideas that should be dismissed, see also Chapter 19.

Agency, Exogenous Shocks, and Incremental Change

Additional issues are raised by analysts of incremental change, who express serious reservations both about some building blocks, and also the overall value, of critical

26. See also Greer (2008: 219), Calder and Ye (2010: 42), Capoccia (2015: 148, 150–51, 156–60, 165), and García-Montoya and Mahoney (2020).

junction research (Thelen 1999, 2003, 2004; Streeck and Thelen 2005a, 2005b; March and Olsen 2006).²⁷

According to this view, critical juncture research does not explain change properly. This limitation is seen as flowing from Mahoney's (2000: 507) view that "path dependence characterizes specifically those historical sequences in which contingent events set into motion institutional patterns or event chains that have deterministic properties." Indeed, this deterministic conception of path dependence is seen as underpinning a static approach to change—one that disregards the ever-present role of agency and contentious actions that challenge established orders and necessarily invokes exogenous shocks as a source of change (Crouch and Farrell 2004; Thelen 2004: xii–xiii, 8, 25–31; Streeck and Thelen 2005a; 2005b: 6–9).²⁸

These critics also maintain that critical juncture research offers an inadequate overall portrayal of how societies change. In their view, this research depicts history in terms of sudden big jumps followed by long periods of continuity and ignores the pervasive and impactful role of gradual change. Further, this research is seen as overlooking a key insight: that much change is incremental, and that such change can actually lead, cumulatively, to major social change (Thelen 2004: 28; Streeck and Thelen 2005a; 2005b: 6, 8–9; March and Olsen 2006: 12; Mahoney and Thelen 2010: 2–3).

Responding to these strong critiques, D. Collier (Chapter 1), Munck (Chapter 5), and Riedl and Roberts (Chapter 6) show that they are largely unwarranted. With some exceptions, critical juncture research offers an explanation of change that draws on endogenous and exogenous factors, as well as the interplay between structural factors and actors. Moreover, rather than denying the importance of incremental change, researchers acknowledge the need to consider critical junctures as one among various possible kinds of change and to explore ways in which the study of big, discontinuous changes and incremental changes might be combined. Many substantive chapters (see especially Chapters 2, 4, 12, and 14) provide further evidence that critical juncture research avoids the various pitfalls discussed by these critics.

At the same time, this debate again points to the need for an explicit discussion of the theoretical foundations of critical juncture research, a challenge taken up by Munck (Chapter 5). He discusses two basic issues in the study of social change: the problem of deciding how the analysis of endogenous and exogenous sources of change should be combined, and the problem of integrating structure and agency. He also argues that insights about these problems developed more broadly in social theory, yet largely ignored by critical juncture research, should be used to reconstruct the critical juncture framework.

27. See also Crouch and Farrell (2004), Orren and Skowronek (2004), Deeg (2005), Djelic and Quack (2007), Mahoney and Thelen (2010), Hacker et al. (2015), and Conran and Thelen (2016).

28. See also Arthur (1994: 27, 118), Thelen (1999: 387; 2004: 8, 31, 34), Pierson (2004: 51–52), Martin and Sunley (2006: 402), Greer (2008: 219), and Fioretos (2017: 12).

The critique made by analysts of incremental change also highlights the need to bridge approaches focused on rapid, discontinuous change and on gradual adjustments, a task addressed by Riedl and Roberts (Chapter 6). They make a case for treating critical junctures and incremental changes as two potentially complementary kinds of change, and they specify the conditions under which one or the other will occur. More broadly, Riedl and Roberts show that a focus on the relationship between critical junctures and other types of transformation opens up new, barely explored avenues for future research.

Research Methods

The methods used to assess claims about critical junctures and their legacies have also become the focus of discussion. Until the 1990s, critical juncture research relied almost entirely on qualitative methods, and these methods continue to predominate. But some changes are evident. A concern with causal inference has long been part of the qualitative tradition, yet given the heightened interest in inferring causation—especially in the disciplines of economics and political science (Gerring 2017)—this has become a stronger focus in qualitative research on critical junctures. Further, since the turn of the century, we see a growing body of literature that could be called quantitative history that places a distinctive focus on quantitative methods specifically designed to identify causal effects (Angrist and Pischke 2008, 2015; Morgan and Winship 2015; see also Goodrich et al. 2012 and Wawro and Katznelson 2014).

In this changing situation, the relative value of qualitative and quantitative research has become a matter of some controversy.²⁹ Many scholars hold that qualitative methods should continue to be used in the study of critical junctures (Collier 1998; Mahoney and Rueschemeyer 2003; Mahoney and Thelen 2015; Fioretos et al. 2016). Moreover, although these scholars are open to quantitative methods, they have reservations about their use. This position is clearly formulated by Kurtz (2013: 50), who states that

[i]n principle, given data sets of long historical sweep, it might be possible to adequately model theories . . . [about critical junctures and legacies] in large-N quantitative data. But even so, it would imply complicated functional forms and would pose a series of econometric and data challenges.

29. Researchers have largely taken it as a given that, in terms of the choice between experimental and observational research, the latter is the most viable and useful. There is a silent consensus in the field, articulated by Diamond and Robinson (2010: 1), that controlled experiments are not possible in the study of historical causation because “one cannot manipulate the past.” Even though real experiments, that is, randomized controlled trials, on certain aspects of claims about critical junctures, could be designed, experimental research has practically not even been discussed (for an exception, see Steinmo 2016).

Other scholars, who work within the new institutional economics tradition and use quantitative methods to study critical junctures, offer a different perspective. At times these scholars articulate a critique of qualitative research, stating that this research is useful, at most, for the purpose of collecting data (Nunn 2014: 351, 354). They also make a case that quantification is an unambiguous sign of progress (Nunn 2009: 71, 81). Thus, the rise of quantitative history has introduced a methodological divide in historically oriented analysis.³⁰

The controversy about methods used in critical juncture research is more circumscribed than the debate about the theoretical framework employed. However, addressing this discussion is essential. All research traditions need to scrutinize their methodological foundations and be open to incorporating new methods into their repertoire. Moreover, as the social sciences in general place more attention on causal inference, all research traditions need to consider their practices in light of new standards. Consequently, matters of method are dealt with in this volume, by D. Collier (Chapter 1), Waldner (Chapter 7), and Munck (Chapter 8).

Taken together, these chapters argue that qualitative methods continue to have value—a point forcefully demonstrated by the substantive studies in this volume. However, some chapters make a case for innovation. Qualitative research on critical junctures should focus more intently on problems of causal inference. Quantitative analysis should be embraced and improved through greater reliance on qualitative data and methods. Finally, a dialogue that bridges these traditions, and avoids a ranking of methods, is indispensable.

These three chapters address diverse issues and emphasize distinct ideas. D. Collier (Chapter 1) places a heavy emphasis on long-standing concerns in qualitative research. He highlights the need for description that carefully establishes conceptual equivalence across domains. He also endorses the use of process tracing and analysis of mechanisms.

Waldner (Chapter 7) focuses squarely on causal inference in qualitative research. He provides a critical review of methodological issues and underscores the problem of backdoor paths that emerges when there is a common cause of a posited cause and a posited effect. He argues that this problem has not been recognized and addressed in discussions about qualitative methods. Waldner further stresses that qualitative research on critical junctures needs to go beyond its usual focus on the connection between a posited cause and a posited effect—the central concern of process tracing studies—and consider the bias possibly introduced by backdoor paths.

Lastly, Munck (Chapter 8) provides a critical assessment of the newer quantitative literature. He shows that quantitative history is relevant to research on critical junctures—its interest in persistent effects is consistent with the focus on legacies in

30. This divide runs through the new institutional economics itself. Indeed, some researchers identified with this tradition make a strong case in favor of case studies (Alston 2008; Potete et al. 2010) and “analytical narratives” (Bates et al. 1998; Rodrik 2003).

the literature on critical junctures—and he argues that it makes an important contribution due to its attention to causal inference. However, Munck also holds that quantitative history has important limitations as a tool to study historical causation and that it ignores the distinct contribution to causal inference provided by qualitative research on mechanisms.

Addressing Divergent Positions

Contrasting views about the framework and methods used in a research tradition can block progress in knowledge. Inasmuch as the positions defended in debates are problematic, they can lead substantive analysts astray, inducing them to develop and test theories inappropriately. Inasmuch as critiques are unfounded, they can dissuade scholars from following potentially fruitful avenues of research. Thus, when disagreements concern central issues in a research tradition—as is currently the case in critical juncture work—they are best confronted head-on. And that is what this volume does.

It is equally important to confront contrasting views in a way that is mindful of the dangers of change for the sake of change. Change should always be welcome. Indeed, to be successful, every tradition needs to be open to innovation regarding its framework and methods. However, change should be managed carefully. Dispensing with established ideas of proven value is reckless. In turn, new ideas should be carefully introduced, after they have been closely scrutinized and their value is substantiated. That is, progressive change should be selective. It should not discard old but useful ideas. It should also avoid incorporating new but flawed ideas.

Of course, as Kuhn notes, progress in the production of knowledge sometimes comes through more radical change—paradigm change. However, as he argues (1970 [1962]: Chs. 6 and 7), such cognitive revolutions occur and could even be said to be needed when serious anomalies—phenomena that cannot be explained in terms of a theoretical framework—add up and an epistemic crisis ensues. And this is not the current situation with the critical juncture tradition. Indeed, the task at hand is better understood in terms of Otto Neurath's (1944: 47) metaphor of the ship of science, which is summarized as follows:

Imagine sailors, who, far out at sea, transform the shape of their clumsy vessel from a more circular to a more fishlike one. They make use of some drifting timber, besides the timber of the old structure, to modify the skeleton and the hull of their vessel. But they cannot put the ship in dock in order to start from scratch. During their work they stay on the old structure and deal with heavy gales and thundering waves. In transforming their ship they take care that dangerous leakages do not occur.

In brief, innovation is always desirable. Yet, given the current state of the critical juncture tradition, progress is more likely when risky innovations are pursued for good reason and with deliberate care.

OVERVIEW OF THE BOOK

This book is divided into four parts, with a concluding chapter that emphasizes the power and promise of critical juncture research.

Basics: Core Concepts and Big Substantive Questions

The first part starts by introducing the core concepts of the critical juncture framework. David Collier (Chapter 1) proposes a working definition of a critical juncture and a five-step template that synthesizes the basic sequence on which scholars routinely focus. The heart of this framework is the critical juncture-legacy nexus. To these two core concepts, Collier adds three more: antecedent conditions, cleavage or shock, and aftermath.

Antecedent conditions stand prior to a posited critical juncture, and encompass diverse features of economy, society, and politics that set the parameters for subsequent change. The cleavage or shock precedes and precipitates critical junctures. Cleavages grow out of a fundamental societal or political division, and shocks—sometimes international, sometimes domestic—may likewise be the triggering event. Finally, during the aftermath, the transformations introduced during a critical juncture may be contested, and are either rolled back or gain acceptance.

This framework extends Lipset and Rokkan's (1967) approach and updates the framework presented in *Shaping the Political Area* (Collier and Collier 1991: Ch. 1). It also comments on methodological issues. It is intended to anchor research in this tradition, yet is sufficiently flexible to be valuable for scholars who approach this topic in different ways.

The volume turns next to some examples of substantive research. Indeed, before delving into detailed discussions of theory and methods, it presents three chapters that showcase the richness of critical juncture analysis and illustrate how the concepts presented in Chapter 1 are employed in comparative historical studies.

James A. Robinson (Chapter 2) analyzes Europe's colonization of the Americas, which he interprets as a shock of global significance that opened new opportunities for power and wealth, along with the resulting critical juncture, i.e., the complex patterns of institution-formation triggered by this shock. In tracing the long-term economic trajectories produced by different types of colonialism, he tackles two analytical tasks.

Robinson first focuses on antecedent conditions to explain the differential response to this shock. In Europe, these involve institutional differences: constitutional monarchy in Britain versus absolutism in Spain and Portugal. In the Americas, they involve contrasts in the density and organization of indigenous peoples. Adding even further historical depth to this analysis, Robinson traces the origins of the institutional differences among Britain, Spain, and Portugal to a prior critical juncture—the collapse of the Western Roman Empire.

Second, Robinson spells out why the institutions created with colonization yielded contrasting economic trajectories, thereby providing a compelling explanation for the different long-term patterns of economic development followed by North America and Spanish America. His explanation is strengthened by his analysis of how institutions founded in the colonial period are reproduced, generating path dependence. Robinson thus connects successive links in a causal chain that extends over several centuries. Although the analysis makes strong explanatory claims, he concludes by speculating about the possible role of agency in these transformations—an important recurring theme in this literature.

G. John Ikenberry (Chapter 3) applies the critical juncture framework to key questions in the study of international relations. Why do international orders vary in character—i.e., take the form of a balance of power, have a hegemon, or are constitutional in form? Why do international orders vary in terms of stability and durability? He analyzes four potential critical junctures: the order-building attempts by leading states in the wake of four major wars, after 1815, 1919, 1945, and 1989. And he provides a multipart argument.

According to Ikenberry's multistep argument, wars destroy established orders and open opportunities for states to build new orders. However, a key antecedent condition, whether the states that are part of a postwar settlement are democratic or not, determines the type and stability of the international order that emerges. The postwar order is also shaped by the magnitude of the shock of war and whether power disparities after the war are great or small. For example, the international order takes on a "constitutional" form, as it did after World War II, when more of the states are democracies and the power disparities among the leading states emerging from the war are great. Ikenberry also explains the stability and durability of the post-World War II order in terms of the logic of increasing returns. The chapter concludes by drawing implications from this analysis for a possible change in the current international order due to the rise of China.

Finally, Sidney Tarrow (Chapter 4) analyzes the wave of mobilization, protest, and conflict that erupted in and around 1968 in France, Italy, and the United States. Drawing on the critical juncture framework, Tarrow argues that these protests were a shock that triggered policy innovations conditioned by antecedent conditions. However, Tarrow underscores that policy innovations do not necessarily have a lasting effect, and that in France—the case where the shock was the most explosive—a conservative reaction was largely successful at rolling back changes. Indeed, in France, 1968 left few legacies. In Italy and the United States, the movements of the late 1960s helped to transform institutional politics and contributed to the "movementization" of politics. Thus, Tarrow argues that, somewhat surprisingly, the legacy was more extensive and enduring in countries where the shock was less explosive—Italy and the United States. In short, Tarrow shows how the study of social movements and their impact can benefit from the deployment of concepts from the critical juncture framework.

Framework and Methods: Historical Causation and Causal Inference

The second part of this volume discusses the theoretical framework and methods used in research on critical junctures. It addresses the challenges faced in the critical juncture tradition, tackling various debated issues and proposing solutions to controversies. It also introduces ideas relevant to this field that have not been discussed within this tradition. Jointly, the chapters in this part provide a justification for retaining classic ideas and practices, but also for introducing significant innovations.

First, starting with the theoretical foundations of critical juncture research, Gerardo L. Munck (Chapter 5) provides a critical review and proposes various refinements and reformulations of the framework. First, Munck focuses on the way critical juncture analysis should provide a historical explanation of social order. He argues that this research must confront two foundational issues that inevitably emerge in the study of historical causation: the problems of infinite regress and distal (i.e., distant) nonrecurring causes. And he holds that these problems should be addressed by conceptualizing critical junctures as rapid, discontinuous, macro-level changes that generate a persistent effect. In addition to clarifying these core ideas, he makes a case for greater attention to causal mechanisms that stabilize the legacy.

Second, Munck discusses how critical juncture research should explain social change. Again, he highlights two foundational issues: the challenges of integrating endogenous and exogenous sources of change, and also structure and agency. He claims, most basically, that critical juncture research should avoid pitfalls by considering both endogenous and exogenous factors as well as the interplay of structural factors and actors. He also argues for attention to the limits of endogenous drivers of change, a more careful analysis of agency, and for theorizing mechanisms of change. Overall, this chapter suggests how the critical juncture framework can be reconstructed on stronger foundations.

Rachel Beatty Riedl and Kenneth M. Roberts (Chapter 6) examine the extent to which outcomes are explained by antecedent conditions and differentiate between two types of critical junctures: generative, in which contingent choice plays a greater causal role; and activating, in which antecedent conditions play a more important causal role. They treat the weight of antecedent conditions and contingent choices as a matter to be theorized and also treated as an empirical question rather than being settled by definitional fiat.

Riedl and Roberts also tackle a further challenge: to explain whether institutional change is likely to take the form of a critical juncture or some other form (e.g., incremental change, serial replacement). To this end, they focus on two key variables: strength of the institutional environment and the role of contingent choices. The result is a novel synthesis that pinpoints the circumstances under which different kinds of change—critical junctures, incremental changes, and serial replacements—are more likely.

The book then moves to methods. New ideas about methods regularly play a key role in energizing research programs. Two chapters scrutinize the use of qualitative analysis and quantitative methods in critical juncture studies.

David Waldner (Chapter 7) calls for greater attention to causal inference in qualitative research on critical junctures. He draws on ideas from the fields of causal inference and causal graphs, and focuses on bias introduced by backdoor paths—an issue when a posited cause and a posited effect have a common cause. Waldner then diagnoses several limitations of qualitative research. He argues that treating contingency as a defining attribute of critical junctures—a view he finds problematic—does not solve the problem of backdoor paths. More broadly, he shows that some well-known studies on critical junctures have not adequately addressed the possibility of backdoor paths. Finally, he offers some methodological recommendations for dealing with these challenges.

Waldner maintains that qualitative researchers must rely on strict procedures to properly analyze a causal path between a cause and an outcome—the front-door path in the language of graph theory. However, as he notes, the possibility of backdoor paths is a distinctive threat to causal inference that qualitative researchers have largely ignored.

Gerardo L. Munck (Chapter 8) examines a new line of quantitative historical research by economists and political scientists that is relevant to the critical juncture tradition. He notes that this new literature does not generally use the language of critical junctures, but shares an interest in distant causes and persistent effects. Although it primarily seeks to estimate causal effects, it also studies causal mechanisms and even uses some qualitative tools. Munck sees this new literature as a welcome addition to a field that, until recently, relied entirely on qualitative research.

Munck's overall assessment of quantitative history is mixed, however. Although a major strength of this literature is its keen focus on causal inference, Munck argues that quantitative researchers sometimes mistakenly suggest that analysts must choose between qualitative and quantitative methods, and that quantitative research is always preferable. In fact, he holds that quantitative history would benefit from a greater use of qualitative tools and, in particular, from taking advantage of a strength of qualitative research, the careful tracing of processes connecting distal causes with much later outcomes. Thus, he recommends developing mixed-methods strategies that better marshal the strengths of both traditions.

Substantive Applications I: States and Political Regimes

Part III of the book continues with chapters that exemplify the range of substantive questions and the kind of arguments advanced in this literature. These chapters focus on critical junctures associated with state formation, the state's response to religious authorities, labor's political incorporation, communist rule, and democratization. They span Europe, Latin America, and postcommunist countries, and developments from the nineteenth to the early twenty-first centuries. Some chapters focus more on theory generation, others on hypothesis testing. One addresses the distinctive challenge of theoretical integration, based on bridging the perspectives of multiple authors.

Sebastián L. Mazzuca's (Chapter 9) study of nineteenth-century state formation in Latin America is an example of theory generation. He develops a novel argument, connecting political transformations in the nineteenth century with twentieth-century patterns. He begins by examining the outcome to be explained: the inefficient combination of populations and natural resources, together with relatively poor governance and economic performance, that characterizes Latin America in the twentieth century. Thus, while many studies begin with a critical juncture and work forward, he begins with the outcome to be explained and works backward.

Mazzuca then turns to explaining this outcome by leveraging comparisons both across and within regions to analyze the antecedent conditions leading to distinctive patterns of nineteenth-century state formation. Whereas in Europe this process was driven by war, in Latin America it took place in the context of global capitalism and the dynamics and legacies of colonialism. Within Latin America, a combination of geographic factors (proximity to seaports), political factors (coalition building), and the mode of incorporation of peripheries created commonalities and differences in state formation. These factors together created perverse relationships between core and periphery, and hindered the long-run economic development of the two largest states—Brazil and Argentina—and by extension, of their smaller neighbors. Mazzuca concludes by briefly noting the relationship between his analysis and other work that seeks to explain some of these same outcomes.

Andrew C. Gould (Chapter 10) provides an overview of his book, *Origins of Liberal Dominance: State, Church, and Party in Nineteenth-Century Europe* (Gould 1999). He conceptualizes liberal forces as those that favor a secular, representative government that places constitutional checks on the monarchy, while not involving democracy. He also explains the contrasting fate of liberal parties and liberal regimes in nineteenth-century Belgium, France, Germany, and Switzerland in terms of the evolution of church-state relations.

Gould shows that the church's role in liberal reforms and the ultimate success or failure of liberal regimes was shaped by two closely connected critical junctures. In the elite phase, the cleavage between liberalism and the clerical elite led to a critical juncture—the liberal reform of political institutions—in which the church's support for these reforms depended on its potential to gain political authority. In the mass phase, institutions created in the first juncture were the antecedent conditions for the next liberal reform period. Here, the church's role depended on the support derived from a substantial expansion of the suffrage. Gould offers a novel argument for the failure of democracy in Germany. He also offers some observations on how the study of critical junctures has evolved.

Ruth Berins Collier (Chapter 11) addresses a key issue in empirical research: How should a critical juncture hypothesis be empirically assessed against alternative hypotheses involving temporally proximate causes? Focusing on Brazil, Chile, Mexico, and Venezuela, she juxtaposes (1) a thesis about the legacy of the critical juncture of labor incorporation during the first half of the twentieth century with (2) hypotheses rooted in a set of temporally proximate international events and policies that had an immediate impact on domestic politics in Latin America in the 1940s.

R. Collier's analysis suggests that, although international events left a clear imprint, they did not deflect internal dynamics and they yielded some mistaken predictions. However, she argues that the two perspectives can usefully be integrated, because international factors help explain some patterns and account for the timing and intensity of the steps that reflected internal trajectories.

Robert M. Fishman (Chapter 12) tackles a key debate in the study of democratization—whether the mode of transition has a long-term effect—and uses a comparison of Portugal and Spain to support the view that the mode of democratic transition can leave an enduring legacy for the post-transition regime. He also shows that the critical juncture framework need not be restricted to formal institutions. Rather, drawing on ideas from the sociology of culture, he applies it to uncodified, yet exceedingly important, political practices.

Fishman argues that an inclusionary democratic practice emerged in Portugal, where actors in positions of authority acknowledged and interacted with mobilizations and demands that occurred outside of formal institutions. By contrast, a segmented and exclusionary democratic practice arose in Spain, where similar mobilizations and demands were not legitimated and accepted. By applying the critical juncture framework, he cogently demonstrates the steps through which this contrast between Spain and Portugal was generated and subsequently consolidated. Further, by employing a cultural perspective, Fishman teases out a fundamental contrast in post-transition regimes that is rarely acknowledged in studies of political institutions.

Danielle N. Lussier and Jody LaPorte (Chapter 13) reconstruct the literature on legacies in post-communist countries initiated with Jowitt's (1992) work and, using the critical juncture approach and Waldner's (Chapter 7) methodological framework, cast a new light on the study of historical legacies in the postcommunist world. Evaluating three important studies of postcommunism, Lussier and LaPorte map out alternative causal pathways. Legacies may be shaped by the critical juncture itself (Wittenberg 2006), by the critical juncture in combination with antecedent conditions (Pop-Eleches and Tucker 2017), or by a chain of multiple critical junctures (Grzymała-Busse 2002).

Lussier and LaPorte establish a fruitful dialogue between critical juncture research and the influential analysis of postcommunist legacies. They show that the concepts of the critical juncture framework facilitate a comparison of these three studies and help to pinpoint agreements and disagreements. Moreover, they derive an important conclusion: the idea of a universal critical juncture that explains postcommunist outcomes is not valid.

Substantive Applications II: Neoliberalism and Political Parties

Part IV of the volume consists of an exchange about the possibility that the neoliberal reforms in Latin America in the 1990s are a critical juncture that has transformed party systems. The dialogue is centered on Roberts's major book *Changing Course in Latin America* (2014), which treats neoliberalism as a critical juncture in Latin

America. One of the methodological issues raised by such a claim—and addressed by several chapters—is whether this thesis can be supported without the benefit of considerable hindsight.

Kenneth M. Roberts (Chapter 14) discusses the problems he faced in writing his book on the impact of the neoliberal reforms of the 1990s on party politics. He notes that in carrying out this research, the challenge of achieving adequate hindsight was daunting, and he was pushed to recast the argument several times over a number of years. He stresses the value of distinguishing between the crisis or shock of the 1980s—along with even earlier antecedent conditions—and the neoliberal reforms of the 1990s. Moreover, he notes that divergent institutional legacies, i.e., different configurations of party systems, did not begin to be identifiable until the “left turn” in Latin America that occurred in the early twenty-first century.

Only at that point did Roberts formulate his hypothesis that divergent party outcomes resulted from two key differences in the critical juncture proper—that is to say, from the neoliberal reforms: whether conservative actors directed the process of market reform, and whether a major left party was available to channel resistance to market orthodoxy. However, Roberts argues that, by the time he published his book, the consequences for party systems of neoliberal reforms were sufficiently resilient to validate, if still tentatively, the claim that these reforms had indeed produced an enduring legacy and hence were a critical juncture.

Samuel Handlin (Chapter 15) further addresses this problem of hindsight based on a comparison between Roberts’s (2014) book and his own book, *State Crisis in Fragile Democracies* (2017). He argues that, even though scholars should proceed with caution in making claims about recent events, some questions can be addressed with confidence. Moreover, he holds that knowledge is advanced by juxtaposing different arguments about a hypothesized critical juncture, such as Robert’s claim about the impact of neoliberal reforms on party politics and Handlin’s thesis about relative state weakness.

Handlin argues that, based on these alternative perspectives, it is possible to formulate arguments about potential critical junctures and refine them in light of the evidence, even before the dust has settled. Moreover, he holds that explicit comparisons of the duration and a hypothesized legacy of a critical juncture may provide a basis for inferring whether it in fact occurred. It is possible, even with limited hindsight, to formulate arguments about potential critical junctures and assess these arguments in light of evidence.

Timothy R. Scully (Chapter 16) evaluates the hypothesis that dramatic transformations introduced under the Pinochet dictatorship (1973–1990) are a critical juncture that reshaped the Chilean party system. Scully shows that during Chile’s three prior critical junctures, which extend back to the mid-nineteenth century, the Chilean party system had a distinctive feature that is unique in Latin America: a strong political center. Thus, for the post-Pinochet period, a key question is whether the Pinochet dictatorship was a critical juncture whose legacy was the destruction of the political center. As Scully shows, answering this question is complex, given

ambiguities resulting from recent shifts in electoral laws and other short- to medium-term dynamics. However, he argues that Chile continues to have a viable center. Therefore, he concludes that his specific hypothesis about a critical juncture in Chile in the 1970s and 1980s is not supported.

This section concludes with two brief commentaries on the potential of critical juncture research to understand contemporary politics in Latin America. Taylor C. Boas (Chapter 17) explores the problem of hindsight through a dialogue with Roberts's book. He holds that, for critical junctures that have occurred in the recent past, it might be impossible to establish that a legacy is enduring. Notwithstanding this constraint, he identifies certain kinds of limited analytic claims that can be productively assessed. Preliminary evidence can rule out claims that a change was *not* a critical juncture. For example, he shows that Roberts was able to offer two negative findings: that shifts in party alignments were *not* incremental and did *not* occur in the same way in different countries. The latter would have undermined the hypothesis that the critical juncture was a point of differentiation among countries, which is crucial to Roberts's argument. Additionally, Boas suggests that the analysis of sequential critical junctures—as exemplified by Ikenberry's chapter (Chapter 3)—provides additional leverage in thinking about recent transformations. Thus, Boas highlights the value of critical juncture analysis in the study of contemporary politics, while urging care in drawing conclusions.

Finally, Robert R. Kaufman (Chapter 18) closes Part IV of the volume with some brief reflections on the appropriate way to analyze contemporary Latin American politics. He suggests that in the past few decades, the region has experienced so many crises and upheavals that analysts should recognize the challenge of achieving agreement on what transformations have and have not occurred. Correspondingly, he emphasizes that it is difficult to establish whether recent changes constitute critical junctures. Overall, Kaufman underscores the need for caution in using the critical juncture framework to study recent or ongoing developments.

Conclusion

In the conclusion to the volume, Gerardo L. Munck (Chapter 19) clarifies why critical juncture research should be encouraged, and dispels some common misconceptions about this field. This chapter argues that critical juncture analysis excels in addressing big, substantive questions, recognizing qualitative breaks, analyzing the social world with a sense of historical depth, and theorizing with an expansive vision. This chapter also makes a case that several claims about critical juncture research—for example, that it involves a change in an entire system and that it only produces institutional legacies—are erroneous and costly, and shows how the agenda and possibilities of this field are expanded when these misconceptions are rejected.

In short, this chapter encapsulates one of the overarching messages of the volume: critical juncture research has great power and promise, and it is key that it not be curtailed due to flawed views about the theory and methods used in this tradition.

Appendices

Appendix I lists alternative conceptions of a critical juncture and cognate terms, underscoring points of agreement and disagreement. Consistent use of key concepts is an important desideratum in the social sciences, and one of the issues addressed in several chapters is how the core concepts of the critical juncture framework should be understood. This appendix is thus a valuable point of departure for ongoing discussions of concepts and definitions.

Appendix II is a glossary of the broader set of terms used in critical juncture research. Over the years, many concepts have been introduced, and even a researcher who works in this tradition may sometimes have difficulty holding to their precise meaning. The glossary offers brief definitions and pinpoints cases in which terms are given more than one meaning.

Appendix III is a comprehensive bibliography. It includes studies that, while not explicitly framed in terms of critical junctures, adopt a similar approach to comparative historical analysis. The bibliography is organized thematically, and it demonstrates the remarkable range of substantive topics addressed by this body of research.

Appendix IV offers brief discussions of eight books that exemplify critical juncture research. The core arguments of these works are summarized, and their contribution to multiple fields of research is noted. This appendix illustrates, in a synoptic fashion, the kind of work that is done in the critical juncture tradition.

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