

This PDF includes a chapter from the following book:

When Things Don't Fall Apart

Global Financial Governance and Developmental Finance in an Age of Productive Incoherence

© 2017 Massachusetts Institute of Technology

License Terms:

Made available under a Creative Commons
Attribution-NonCommercial-NoDerivatives 4.0 International Public
License

<https://creativecommons.org/licenses/by-nc-nd/4.0/>

OA Funding Provided By:

The open access edition of this book was made possible by
generous funding from Arcadia—a charitable fund of Lisbet
Rausing and Peter Baldwin.

The title-level DOI for this work is:

[doi:10.7551/mitpress/11073.001.0001](https://doi.org/10.7551/mitpress/11073.001.0001)

2 Productive Incoherence: A Hirschmanian Perspective

How are we to come to terms with the emerging inconsistencies in global financial governance and developmental finance? The seminal work of Albert O. Hirschman provides extraordinary insight into the current conjuncture. Hirschman was a scholar and practitioner who, as much as anyone else, recognized the potential importance of mundane, fragmented, inconsistent initiatives.

Drawing on Hirschman's work, I argue that the present era is characterized by a quality of permissiveness that is far better suited for development than institutional fidelity to an overarching, coherent doctrine that serves as the blueprint for a new economic system. Central to this perspective is the Hirschmanian idea that the absence of a new "ism" to replace neoliberalism might not be a source of despair but instead should be recognized as a historic opportunity that is already expanding the terrain of ideational, policy, and institutional experimentation that is central to social and economic development.¹

A Hirschmanian School of Thought?

The global crisis, especially in its early phase, burnished the legacy of John Maynard Keynes and other Keynesians, such as Charles Kindleberger and Hyman Minsky. Keynes's biographer Robert Skidelsky seized on the crisis to argue for the salience of Keynes in *The Return of the Master* and related work (Skidelsky 2010; 2011). Previous upheavals, particularly the fall of the Berlin Wall and the traumatic transition to market liberalization in Russia, led to the rediscovery of another classic work, Karl Polanyi's *The Great Transformation* (2001[1944]). The salience of Polanyi's book to understanding these events was underscored in Joseph Stiglitz's foreword to the 2001 reissue of the book (e.g., p. xii). Around the same time, Blyth (2002) used Polanyi's work, particularly its central conception of the "double movement," to

make sense of the interaction between significant institutional transformations and changes in economic ideas during the 1930s and 1970s, while Harmes (2001) deployed the same work to make sense of the Mexican financial crisis of the 1990s.

These events have not had the same effect on the fortunes of Hirschman. Certainly, publication of two recent books by his biographer Jeremy Adelman (2013a; 2013b) and work by Michele Alacevich (2014; 2015; 2016) have begun to generate some interest in Hirschman's work. The occasion of Hirschman's death in December 2012 at age ninety-seven also inspired several thoughtful and broad-ranging reflections on the importance of his intellectual legacy.² Today a small number of public intellectuals, economists, and other social scientists are recognizing the salience and originality of his insights, even if their appreciation is necessarily selective in relation to Hirschman's vast and diverse oeuvre.

How wide were Hirschman's professional interests and competencies? Though formally trained as an economist, Hirschman's writings extended over a remarkable range of disciplines and subdisciplines as diverse as economic theory, development economics, Latin American studies, political science, public administration, industrial organization, political philosophy, intellectual history, and rhetoric. On many occasions, he reflected on his tendency to cross paradigmatic and disciplinary borders. The title of one of his best-known essay collections, *Essays in Trespassing*, perhaps best captures his embrace of intellectual wandering.

Nonetheless, while Hirschman is held in high esteem by leading scholars, there is no Hirschman school of thought or paradigm. Dani Rodrik (2007a) notes correctly and with regret that students of development economics no longer read Hirschman, a view also expressed by Taylor (1994). For too many, Hirschman is taken to represent outdated thinking that might be acknowledged respectfully in passing but that does not warrant careful scrutiny.

Why has Hirschman been disposed of so readily? A number of factors bear on his legacy.³ Since the 1960s, appreciation of Hirschman's work has been constrained by factors connected to the sociology and norms of contemporary, specialized, professionalized social science. What Adelman (2013b, 3) terms Hirschman's distinct "brand of social science"—his discursive, literary, nuanced, and often witty style, his interdisciplinarity, and the sheer breadth of his interests and writing—places him in the tradition of the great social theorists of the eighteenth and nineteenth centuries, when intellectuals were broadly educated and their work was widely read by nonacademics. Their work eschewed the "specialization-induced intellectual poverty" that later defined the social sciences (Hirschman, cited in

Fukuyama 2013) and made it difficult for modern social scientists to engage with it. Better, it seemed, to avoid what one was not well trained to digest.

Hirschman flouted what would become central norms in the social sciences. Hirschman rejected simplistic paradigm-based thinking and grand claims. Moreover, he insisted on questioning the prevalent assumptions that enabled the predominant approach to social science. He was at least partially aware of his tendencies toward professional marginalization, referring to his proclivities as a “propensity to self-subversion” (the title of his 1995 book). Yet these were also surely among his greatest strengths as an original thinker (Gladwell 2013). His propensity to self-subvert was, in his view, a hard-won virtue—one that required the humble, inquisitive observer to perpetually subject his own beliefs to scrutiny. “In a time in which people were driven by strong ideological faiths and nothing seemed to work without the pre-defined guidance of a *weltanschauung*, Albert persisted in living outside of and without any *weltanschauung*,” Nadia Urbinati (2015) said of Hirschman.⁴ These commitments, in addition to the sheer breadth of his oeuvre, have sabotaged his standing and influence among contemporary scholars. As Drezner (2013) notes, Hirschman’s rejection of paradigm-based thinking may help to explain why there is no Hirschmanian school of thought. On this point, Hirschman replied to a World Bank director who had sent him a paper referring to the “Hirschman Doctrine” in the following manner: “Unfortunately (or, I rather tend to think, fortunately), there is no Hirschman school of economic development and I cannot point to a large pool of disciples where one might fish out someone to work with you along those lines” (Hirschman, cited in Gladwell 2013).

Hirschman’s profound rejection of the scientific pretensions, hubris, and grand narratives of development economics certainly limited his reach among scholars and development practitioners, who came increasingly to adopt reductionist models and epistemic certainty that facilitated their achievement of authority and influence in policy circles. On more than a few occasions, Hirschman himself reflected on what he—at his most charitable—termed a “difference in cognitive style” that distanced him from others in the field, particularly as social science evolved beginning in the 1960s (Hirschman 2013[1970], 143).

Hirschman’s Legacy and Contemporary Development Economics

In 1994, Paul Krugman took stock of the state of development economics. Krugman argued that the field had only recently been rescued from the likes of Hirschman, Gunnar Myrdal (1957), and other like-minded thinkers.⁵

Krugman (1994, 39) acknowledges that he knows little of Hirschman's work beyond his 1958 book *The Strategy of Economic Development*, but this does not deter him in the least from passing judgment on Hirschman's legacy. Krugman calls the book brilliant, persuasive, but ultimately destructive.⁶ In Krugman's view, the publication of the book marked the end of what he terms "high development theory"—the sweeping development theories of the 1940s and 1950s, as exemplified by the "big push" approach of Paul Rosenstein-Rodan (1943). Krugman (1994, 40) argues that Hirschman resisted and indeed rejected "the pressures to produce button-downed, mathematically consistent analyses and adopt[ed] instead a sort of muscular pragmatism in grappling with the problem of development." Hirschman's book appeared just when development theory was facing a methodological crisis, and it had a profound effect on the work of other development theorists and practitioners. In Krugman's rendering, Hirschman "did not wait for intellectual exile: he proudly gathered up his followers and led them into the wilderness himself. Unfortunately they perished there" (ibid.).⁷ Krugman denigrates Hirschman's stylistic and methodological stance as "vain" and a "dead end" that led to the marginalization of high development theory and development economics until the field emerged from the desert in the late 1980s and 1990s with its new disciples adopting the supposed rigor of formal modeling (ibid., 47, 52).⁸ One cannot help but note that Krugman's essay exemplifies the certainty and cognitive style that Hirschman so strongly rejected.

Is the rescue of development economics from Hirschmanian sensibilities that Krugman celebrates complete? Contemporary research in development economics tends toward one of two methodological poles, both empirical in nature: randomized control trials (RCTs) and "large N studies" by micro- and macro-development economists, respectively. Fukuyama (2013) suggests that the current fascination with RCTs in development economics is moving the field toward small ideas and away from Hirschmanian impulses. In his words, "It is hard to imagine that all the work being done under this [RCT] approach will leave anything behind of a conceptual nature that people will remember fifty years from now" (ibid.). He argues that Hirschman "operated at the opposite end of the spectrum. . . . His legacy is not data collection or micro results, but rather some very big concepts that continue to shape the way we think about not just development but public policy more generally."⁹

Others take a different view of the evolution and state of development economics. In a reflection on the state of the field, Rodrik (2009a) is largely optimistic. While expressing mild worries about RCTs, noting in particular

limits to the generalizability of their findings, Rodrik argues that advocates of this approach are forcing macro-development economists to think more seriously about “evidence.” Rodrik identifies evidence that micro- and macro-development economics are coming together in a “new development economics” that is more pragmatic, less self-certain and universalistic, and more encouraging of policy and institutional experimentation. Rodrik embraces two distinct forms of development trial and error: China’s policy pragmatism on the one hand, which is consistent with Hirschman’s emphasis on experimentation and learning by doing, and the positivist science of RCTs on the other, about which Hirschman might have been much less enthused (discussed later). Notwithstanding this point, Rodrik’s reading of the state of development economics suggests that the field is becoming more “Hirschmanian,” which he sees as an altogether positive step (see also Rodrik 2015a).¹⁰

Irrespective of whether Rodrik is correct about the state of development economics (and the discipline of economics more broadly), one can certainly see a clear Hirschmanian thread running through a great deal of his own work. Indeed, and in recognition of this connection, Rodrik was awarded the inaugural Albert Hirschman Prize by the Social Science Research Council in 2007, and from 2013 to 2015 he was the Albert O. Hirschman Professor in the School of Social Science at the Institute for Advanced Study at Princeton University. In the lecture occasioned by the 2007 prize, Rodrik reflects directly and admiringly on Hirschman’s intellectual legacy (Rodrik 2007a; 2007c; 2007–2008). In that context, he draws a sharp distinction between Hirschman’s work and his own on the one hand and the uniform, self-certain policy prescriptions of the Washington Consensus on the other. Rodrik (2007–2008) credits Hirschman for his embrace of pragmatic learning by doing and for his appreciation of the importance of “opportunism”—which refers to taking advantage of available spaces for innovation rather than insisting on a comprehensive home-grown or imported plan. Rodrik also highlights Hirschman’s search for novelty and uniqueness, his appreciation of sequential and cumulative change, his insistence on the need to have a deep knowledge of local contexts, and his associated suspicion of naïve efforts to transplant policies and institutions from one context to another. Rodrik also celebrates Hirschman’s “bias for hope” and his unrepentant embrace of the possible.¹¹

Hirschman’s insights appear in other contemporary scholarship. More than any other contemporary development economist, David Ellerman (2004; 2005) credits Hirschman for shaping his own thinking, and his work draws broadly and deeply on Hirschman’s oeuvre. Gerald Helleiner

(2010) uses Hirschman's seminal work on exit, voice, and loyalty as a lens through which one can understand the relationship between EMDEs and the BWIs (as we will discuss). One also finds a clear (and acknowledged) debt to Hirschman running through the work of a small number of other contemporary development economists.¹² Several contemporary political scientists also draw productively on some of Hirschman's insights.¹³ On balance, however, Hirschman's work remains terribly underappreciated, with just a few insights attracting the attention of most contemporary scholars.

Hirschmanian Themes

The failure to appreciate Hirschman's work is consequential and damaging. Hirschman's work offers a fresh and extraordinarily useful lens through which to make sense of the discontinuities and productive incoherence of the global financial crisis.

Which Hirschmanian themes provide guidance to observers of and participants in unfolding developments in the field of financial governance and developmental finance? For ease of exposition, I corral these themes under the categories of agent reactions to organizational failure; asymmetric economic relations; epistemic presumptions; failure of grand theoretical narratives and social engineering, and the related centrality of the diminutive, complex, and experimental; linkages and side effects; theoretical and ethical failures of development experts; and associated ideas around possibilism, futilism, and rhetoric. I begin with the best known of his insights and then move directly to themes that are most fertile for the task at hand.

Agent Reactions to Organizational Failure

Hirschman's *Exit, Voice and Loyalty: Responses to Decline in Firms, Organizations and States*, published in 1970, is among his best-known books, and it retains influence among academic economists, political scientists, and policymakers.¹⁴ Hirschman extended its central insights in several later works, including his 1978 essay "Exit, Voice, and the State" and his book *Essays in Trespassing* (1981, 209–284). Exit and voice refer to the two ways in which individuals can respond to their dissatisfaction with the performance of business enterprises, nonbusiness organizations, states, and other institutions of all sorts. The two strategies can curb or even reverse institutional decline. Exit involves "voting with one's feet" (i.e., finding a replacement), relying on the mechanism of competition to ensure adequate performance.

Voice involves “staying put” and mobilizing within an institution to bring about change. The exercise of voice depends on some degree of loyalty and trust. Loyalty constrains the propensity to exit and can thereby create the trust and the space necessary for performance improvements. Excessive loyalty, however, can suppress both voice and exit, and can consequently perpetuate poor performance.¹⁵

Hirschman notes that economists elevate exit over voice, presuming the former to be more efficient. He cites Milton Friedman’s advocacy of vouchers and competition in public schools as a “near perfect example of the economist’s bias in favor of exit and against voice” (Hirschman 1970, 17). Hirschman (1981[1978]) later acknowledged that exit could sometimes occasion states to pursue productive reform. Nevertheless, his work on exit, voice, and loyalty can be read as a warning about the “cult of exit” among economists.¹⁶ Hirschman argued that exit can undermine performance by eliminating the incentive to improve and weakening the effectiveness of voice, particularly since opportunities to exit are not randomly distributed. It is generally those possessing the greatest number of options, those who are most efficacious, mobile, and/or wealthy, who are able to exit poorly performing entities such as states or private institutions. In practice, this means that those with little leverage are left behind. Thus, authoritarian governments or poorly performing managers in public or private enterprises generally prefer to have their harshest critics exit. In typically elegant prose, Hirschman (1970, 59, emphasis in original) described this dynamic as “an oppression of the weak by the incompetent and an exploitation of the poor by the lazy which is the more durable and stifling as it is both *unambitious and escapable*.”

Asymmetric Economic Relations and the Molding of National Interests

Among political scientists working within international relations, Hirschman’s first book, *National Power and the Structure of Foreign Trade* (Hirschman 1980[1945]), is regarded as a classic. Hirschman examines German trade and foreign policy in the interwar period to illustrate the ways in which economic policy can be used as an instrument of statecraft. He shows that asymmetric international economic relations carry the potential for coercion. In a bilateral trade relationship involving countries of disparate size, for example, the larger, more powerful state has influence over the smaller, weaker state since the opportunity costs of terminating the relationship are greater for the weaker state. Hirschman’s argument appears uncontroversial today. But at the time it challenged the traditional liberal idea that greater economic integration promotes fairly dispersed benefits

and hence cooperation among nations (Adelman 2013b, 213). Hirschman also broke with the standard mercantilist view that running a trade deficit necessarily undermines economic power. Instead, he showed that a state can exert coercive power over another with which it runs a trade deficit as its trade partner grows dependent on the foreign market (Drezner 2013). These arguments shed light not only on trade arrangements in the interwar era but also on trade and financial relations in a variety of national and historical contexts, including the contemporary conjuncture. Hirschman's insights are also useful for understanding the coercive power inherent in other types of asymmetric economic relations, such as those involved in North-South trade and investment agreements and the provision of support by large actors (such as the IMF) to states in crisis.

A less widely appreciated, though more innovative and useful, thesis appears in Hirschman's *National Power and the Structure of Foreign Trade* (see Abdelal and Kirshner 1999–2000, 120–121, 156; Kirshner 1997, chap. 4; 2014a). The thesis concerns the power to mold perceptions of national interest. Hirschman demonstrates that the economic incentives available in asymmetric relations can have profound political consequences in smaller, weaker states by shifting the domestic balance of power. Asymmetric relationships can reconfigure foreign policy choices and reshape the perception of national interest by “conditioning” smaller states. Such conditioning is evident in the creation of a “friendly attitude” in the smaller state vis-à-vis the larger state's priorities. Foreign policy decisions in the smaller state may come to reflect the growing influence of domestic interest groups that perceive an alignment between their country's national interests and those of the larger state.

As with Hirschman's argument about coercion, his perspective on the malleability of national interests sheds light on the subtle yet powerful effects of a number of bilateral and multilateral trade and financial arrangements (see cases in Abdelal and Kirshner [1999–2000] and Kirshner [1997, chap. 4]). In addition, and as Kirshner (2014a) argues, Hirschman's notion of preference and interest malleability is relevant to understanding the conditioning effects of American monetary power. For instance, the concept helps to explain the diffusion of capital account liberalization and the U.S. model of light touch financial regulation prior to the global crisis. “Conditioning effects” have since contributed to the emerging recalibration of national interest across EMDEs toward China and other rising financial powers and away from the United States and the IMF. These insights might also be useful for understanding whether and how the expansion of existing financial arrangements and the creation of new ones in EMDEs might

ultimately reshape perceptions of national interest on the part of smaller (and poorer) states (see chapter 6).

Linkages and Side Effects

Hirschman's work on "backwards and forwards linkages" has had some influence in development economics and in discussions of economic development policy. He introduced these concepts in *The Strategy of Economic Development* and elaborated them in later works (e.g., Hirschman 2013[1981a]). The basic idea is straightforward: certain economic activities can create the propitious conditions for new upstream or downstream economic, political, or social capabilities. The idea of linkages grew out of Hirschman's view that growth should be thought of as an unbalanced process and that strategies should focus around targeted rather than comprehensive, grand plans.¹⁷

Hirschman also highlighted the essential role of "side effects," which were far more than the unintended ramifications of a project. Side effects are "*inputs essential to the realization of the project's principal effect and purpose*" (Hirschman 1967a, 149, emphasis in original, cited in Alacevich 2014). For example, a project might establish as an unintended consequence new networks that turn out to be vital to the project's success even though the centrality of the networks was not envisioned at its outset.

Epistemic Issues: Uncertainty, the Limits of Intelligibility, and the Power of the "Hiding Hand"

An important theme in Hirschman's work is the idea that knowledge is incomplete, tacit, partial, and dispersed. Like Keynes and Hayek, Hirschman took knowledge of the future to be fundamentally uncertain (Hirschman 2013[1970]). Following Knight (1971[1921]) and Keynes, he distinguished between probabilistic risk and (inherently) immeasurable uncertainty (Alacevich 2014). Hirschman likened the need for predictability—which entailed a scientific trend that embraced epistemic certainty and parsimony along with the search for general laws and grand paradigms—to neuroses that afflicted the social sciences (Hirschman 1967a, chap. 1; 2013[1970], 138; 2013[1971]; Adelman 2013a, introduction, 137; 2013b, introduction).¹⁸ Hirschman identifies Amartya Sen as a fellow skeptic of overdone reductionism, and he endorses Sen's observation that much is to be gained by making things more complicated (Hirschman 2013[1986a], 249). In Hirschman's words, "let us beware of excessive parsimony!" (ibid., 262).

Hirschman called for appreciation of irreducible complexity (Adelman 2013b, 13). He asked, "Is it not in the interest of social science to embrace complexity, be it at some sacrifice of its claim to predictive power?"

(Hirschman 2013[1986b], 243). The commitment to complexity was both epistemic and practical. For Hirschman, the outcome of any intervention is unknowable in advance since it is always confounded by the “balance of the contending forces that are set in motion” and the totality of circumstances at the time of the intervention, neither of which was accessible to the researcher (Hirschman 2013[1970], 150, emphasis in original). In this and many other respects (discussed later), Hirschman anticipated a paradigm shift in economics, now just under way, toward understanding the economy as a “complex adaptive system” that features constant evolution and abrupt shifts, and, notably, the absence of sufficiently powerful equilibrating mechanisms (such as the Walrasian auctioneer) that can be relied on to bring an economy in a disequilibrium state back into equilibrium (Kirman 2016).

In Hirschman’s view, attempts by social scientists to domesticate what was fundamentally uncertain, disorderly, contingent, and complex had troubling consequences for EMDEs. For Hirschman, as for Hayek (whom he drew on admiringly), there were “limits to ‘intelligibility’ of our complex world” (Adelman 2013b, 238; see Hayek 2014[1944], 181).¹⁹ For Hayek, of course, the limits to intelligibility had determinant policy implications; namely, that it is impossible for any social planning agency to acquire the full range of information necessary to improve outcomes in a complex economy. Thus, for Hayek, it is only through the “‘spontaneous order’ of the competitive market that the diverse and ever changing plans of numerous economic actors, responding to unpredictable and complex shifts in the world, can be reconciled with each other” (Chang 2014, 101). For Hirschman, the failure of development economists and practitioners to appreciate complexity and the limits of knowledge led them to treat poorer countries as essentially simple systems that were analytically tractable. This epistemic error opened up EMDEs as fertile ground for model building, grand paradigms, programming, expert control, and quick fixes (Hirschman 2013[1970], 144).²⁰

Hirschman’s commitments to uncertainty, contingency, and complexity are a constant in his work.²¹ His epistemic position was placed in particularly sharp relief in a project that he conducted for the World Bank during the 1960s. The project culminated in the 1967 book *Development Projects Observed*. The way in which Hirschman operationalized his epistemic views in the project led to an irreconcilable disagreement with the World Bank. The institution’s staff ultimately ignored the work. This episode bears attention since it illustrates so well the nature of Hirschman’s thinking and his break with the prevailing norms of social science in the 1960s and subsequent decades.²²

In the spring of 1963, Hirschman proposed to the World Bank that he enter into what was then unchartered territory for the institution. This involved conducting extensive fieldwork for the institution in Asia, Africa, Latin America, and (after the advice of World Bank staff) southern Italy during 1964 and 1965. Hirschman examined the direct effects and the broad repercussions of various World Bank projects on the economy and society, with the ultimate aim of improving general elements of project appraisal, design, and management. The Bank's staff was expecting something akin to a manual that would provide concrete, generalizable tools for evaluation, such as ways to calculate the economic return on a project. But this is not what Hirschman submitted, either in a detailed, methodologically focused "Interim Observations" memo midway through the project or in the manuscript that was the project's final output. Instead, Hirschman urged a change in epistemic perspective at the institution, and in the interim memo he argued that it should avoid the "air of pat certainty" and work to "visualize the uncertainties," "the element of the unknown," and the "unexpected" that surrounds all projects.²³ Key uncertainties highlighted by Hirschman involve the ability to completely map out a project upon its launch, and the degree to which economic, social, and political change can interfere with project implementation. These uncertainties deflect projects from their anticipated course (Hirschman 1967a; Alacevich 2014, 150–151, 159).²⁴

Hirschman's call for what amounted to a paradigm shift came several decades too soon. The profession was not yet ready to embrace the radical theoretical and practical implications of complexity. Hirschman's report was summarily dismissed (Alacevich 2014, 153–163). Some saw it as impractical; others saw it as simply incorrect—as a misrepresentation of what the institution was already doing. Still others dismissed it because of Hirschman's complete lack of interest in quantitative evaluations. Instead, he focused on developing qualitative analyses, comparing what he termed the "personal profiles" of projects, which included detailed historical reconstructions and consideration of their larger political and social contexts (ibid., 153–159).²⁵

The first chapter of *Development Projects Observed*, "The Principle of the Hiding Hand," appeared as an essay in *Public Interest*. The essay is a particularly rich exemplar of Hirschman's epistemic commitments. Hirschman's clients, however, found it "thin," misspecified, and not useful as a guide to strategy (Alacevich 2014, 156).²⁶

The Hiding Hand concept (the capitalization is Hirschman's) was of course a rhetorical play on Adam Smith's invisible hand, and it reflected Hirschman's recognition that actors always operate in a state of uncertainty

and ignorance. But rather than inhibiting or distorting action, Hirschman saw uncertainty, ignorance, and error as potential drivers of productive action by policy entrepreneurs, just as Keynes emphasized the animal spirits that took hold among private investors and led them to risk adventures that were not warranted by the cold, hard facts. Underestimating problems (e.g., regarding a project's cost or the obstacles that could stymie it) propelled projects forward that would not be initiated in the presence of full information. In Hirschman's words, the Hiding Hand "beneficially hides difficulties from us. . . . People typically take on and plunge into new tasks because of the erroneously presumed *absence* of a challenge, because the task looks easier and more manageable than it will turn out to be (Hirschman 1967a, 12, emphasis in original). Once a project is initiated, project participants are challenged to develop creative solutions to the unforeseen problems that have arisen. The strategies they devise out of necessity can have positive and lasting spillover effects. Thus, predictive and other errors can breed success: actors can and sometimes do respond to crises creatively when they misjudge the nature of a task (ibid., 12). As Hirschman (1967a, 13) put it, "Our more lofty achievements, such as economic, social, or political progress, could have come about by stumbling rather than through careful planning, rational behavior, and the courageous taking up of a clearly perceived challenge."²⁷

Hirschman's distinct vision of development followed from his epistemic commitments. In the words of his biographer Adelman, Hirschman's work is marked by the view that "the study of social change, if it is to be helpful, he felt, should rethink the typical reliance on predictions according to laws of change and consider instead the analysis of possibilities and alternatives for social change" (Adelman 2013a, 137). An essay Hirschman jointly authored with Charles Lindblom makes this point: "It is clearly impossible to specify in advance the optimal doses of . . . various policies under different circumstances. The art of promoting economic development . . . and constructive policymaking . . . consists, then, in acquiring a feeling for these doses" (Hirschman and Lindblom 1971[1962], 83–84).²⁸ Complicating matters further, beliefs can exert causal effects. In the contemporary vernacular, we might say that Hirschman viewed ideas as constitutive and not just reflections of the world. In the words of his biographer, Hirschman thought that "how we understand the world affects how we might change it" (Adelman 2013a, xvii).²⁹ For Hirschman, then, there was no uniform, timeless, context-independent set of factors (such as economic fundamentals) that determine the success or failure of a project. Societies are irreducibly complex, the future is fundamentally unknowable, economies are

constantly in flux, and even efforts to know the world affect outcomes that arise within it (Hirschman and Lindblom 1971[1962], 83–84).³⁰

The Failure of Grand Theoretical Narratives and Uniform Solutions

Hirschman's epistemic commitments underlay his critique of what he saw as "compulsive and mindless" reductionist theorizing in the service of easy causal explanations, quick fixes, and simpleminded solutions to complex challenges (Hirschman 2013[1970], 138).³¹ In his view, ignoring the importance of uncertainty led economists to articulate excessively ambitious paradigms that not only oversimplified but also sought to domesticate the real world of messy, complex social environments in pursuit of predictability and order. Paradigm thinking often induced what Adelman describes as Hirschman's rejection of the "mindless overconfidence in the solvability of all problems or its twin, the fatalism that nothing can be changed willfully at all" (Adelman 2013a, xvi; Hirschman 2013[1971]).³²

Hirschman's view of quick fixes and uniform solutions was also reflected in his rejection of what he termed the "obstacles to development" thesis (Hirschman 1965; 2013[1968b]; Adelman 2013b, 433–435). The obstacle thesis refers to economists' common tendency to search for and find a singular, new, fundamental obstacle to development (such as the shortage of capital) and to rewrite the history of development failures or successes based on the presence, absence, or elimination of this obstacle (Hirschman 1965; 2013[1970]).³³ Hirschman (1965) showed that certain commonly identified obstacles to development could turn out to be assets, that some were not obstacles at all, and that the elimination of certain obstacles could be postponed indefinitely. Attachment to the tradition of identifying obstacles necessarily led to simplistic, homogeneous prescriptions for development. Were Hirschman writing today, he could easily identify obstacle thinking in recent development fads that, for example, focus variously on property rights, social capital, microfinance, and, as of this writing, infrastructure.

Hirschman's impatience with grand narratives, the pursuit of perfection in ambitious utopian projects, and other forms of social engineering applied to plans from all political entrepreneurs—communists of the 1930s, advocates of "big push" and "balanced growth" models of the 1960s, and neoliberals of the 1980s and 1990s (Adelman 2013a, viii).³⁴ In place of social engineering, Hirschman advocated what he termed "immersion in the particular" (Alacevich 2014, 142; Hirschman 1967a, 2) and the need to liberate practice from the straitjacket of reductionist models that provided justification for encompassing, homogenous programs. The universalist approach "hindered rather than helped" since it precluded consideration of alternative or diverse

paths to development (Adelman 2013b, 340; Hirschman 2013[1986a]).³⁵ Hirschman's approach instead was one of improvisation in pursuit of multiple development paths, not implementation of a pristine policy blueprint. He favored complexity, messiness, specificity, and contingency in contrast to what he saw as theoretically sanctioned, paradigm-based uniform solutions (Hirschman 1965; 1967a; 1969[1958]; 1973[1963]; 2013[1970]; 2013[1971]). In reflecting on his own work, Hirschman said, "With this conclusion I can lay claim to at least one element of continuity in my thought: the refusal to define 'one best way'" (Hirschman 1995, 76). This view was consistent with the work of economic historian Alexander Gerschenkron (1962), whose work illustrated the multiplicity and uniqueness of development trajectories in a variety of national contexts (Hirschman 2013[1981b], 58–59). As Alacevich (2014, 141) argues, Hirschman's focus on the particular and the complex was consistent with and helped to inspire the move away from grand theory in development economics in the late 1950s to mid-1960s.³⁶

The Centrality of the Diminutive, the Complex, and the Experimental

Hirschman's epistemic and normative views informed his complex understanding of social change in other ways as well. As suggested, he rejected the common tendency to assess *ex ante* the significance of particular changes or innovations. He wrote of the tendency to dismiss reform, a tendency that reflected both a deep-seated skepticism and the epistemic certainty that dominated social science in his time, and indeed continues to infuse much work today. In this connection, he made an observation that is central to a proper understanding of the contemporary conjuncture, as I will demonstrate throughout this book:

A distinction is often made between "real" and "apparent" or between "fundamental" and "superficial" changes: This device permits one to categorize as superficial a great number of changes that have, in effect, taken place and to assert in consequence that there has not yet been any real change. The decision to assert that *real* change has occurred is made to hinge on one or several tests. . . . But to set up such demanding tests is in itself an indication of a special difficulty and reluctance to concede change except when it simply can no longer be denied. (Hirschman 2013[1968b], 37, emphasis in original)

Hirschman's commitments led him to embrace the diminutive, which he argued could be the building block of meaningful, path-dependent reform and widespread change (Adelman 2013a, vii–viii; Hirschman 2013[1968b]; 2013[1970]; 2013[1971]). Hirschman viewed the task of the development intellectual and practitioner as finding "seams in even the

most impregnable structures” in order to find “openings and prospective alternatives” (Adelman 2013a, vii–viii), a theme that resonates today with certain policy analysts (see Levin et al. 2012, and citations therein). This meant “challenging . . . the euphoric conviction that one could change everything at once—given the ‘necessary conditions’” and led him to articulate an approach that “favored strategic focus over comprehensive breadth” (Adelman 2013a, vii–viii). In this connection, Hirschman said, “To look at unbalanced growth means, in other words, to look at the dynamics of the development process *in the small*” (Hirschman 1969[1958], ix, emphasis in original).

This view of change implied the need to be open to and welcome the unexpected (Hirschman 2013[1970]) and the related need for small-scale experimentation, or what Lindblom (1979; 1959) termed “muddling through.” As Lindblom argues, “‘Muddling through’—or incrementalism . . . is and ought to be the usual method of policymaking. . . . [Indeed] no more than muddling through—is ordinarily possible” (Lindblom 1979, 517). Hirschman and Lindblom (1971[1962], e.g., 77) explored this theme, arguing that policymakers should steer away from “integrated planning,” meaning development models that focus on “big pushes,” balanced growth, and so forth, and instead pursue piecemeal reform in a spirit of opportunism and open-mindedness.³⁷

By now I hope it is clear that Hirschman was deeply suspicious of what I have termed coherence, which is predicated on the notion of the social world as a simple social system where everything fits and where the structure determines what can and cannot work, what is and is not possible. He believed that it was imperative to learn from small-scale, gradual initiatives and from multiple examples, to recognize uniqueness and the specificity of experiences, and to appreciate the possibility of a great many sequences rather than seek universal dictates in a reductive theory (Hirschman 1965; 1969[1958]; 1973[1963]; 2013[1970]; 2013[1971], 22).³⁸ As Fukuyama (2013) rightly notes, Hirschman’s embrace of experimentation did not involve radical or revolutionary change. What Hirschman termed “reform monegering” involved gradualism and small-scale change by democratic governments. In *Development Projects Observed*, Hirschman (1967a, 19) argued that projects “should be developed as much as possible in an experimental spirit, in the style of a pilot project gathering strength and experience gradually, so that they may escape being classed and closed down as failures in their infancy.” He further argued that “projects whose potential difficulties and disappointments are apt to manifest themselves at an early stage should be administered by agencies having a long-term commitment to the

success of the projects” (ibid.). Elsewhere Hirschman wrote of the importance of the “propensity to experiment and to improvise” (Adelman 2013b, 323; Hirschman 1971[1957], 259).³⁹ In *Journeys toward Progress*, Hirschman (1973[1963]) demonstrated through careful case studies that slow reform mongering in Chile, Brazil, and Colombia eventually brought about significant progress, even though some well-intentioned reforms had negative effects and others were plagued by misperceptions and mistakes made by reformers and their foreign advisers (see Fukuyama 2013).

Taking a page directly from Hirschman, Ellerman (2004; 2005; 2014) asserts the importance of adaptive, pragmatic searching; horizontal (south to south) peer group monitoring; and a process of “social or open learning” in development. This involves fostering parallel experiments (which he sees as necessary for learning under uncertainty), pooling the experience of actual projects, seeing what works and comparing results, and promoting cross learning to ratchet up performance of the whole group (Ellerman 2005, 163–165, 234–239).⁴⁰ Ellerman offers the small-scale, gradual, incremental, and pragmatic adjustment of China’s economic policies as an exemplar of a Hirschmanian approach (ibid., 196–197).⁴¹ Rodrik’s work (including his collaborative work on “growth diagnostics”) is likewise characterized by an embrace of targeted as opposed to across-the-board policies, institutional innovation, and monitoring and evaluation as strategies to discover what does and does not work (Rodrik 2007b, especially chap. 2; 2007–2008; 2009a; Hausmann, Rodrik, and Velasco 2008; Hausmann, Pritchett, and Rodrik 2005).⁴² Like Ellerman, Rodrik sees China as engaging in “experimental gradualism.” He also notes that the case of China demonstrates that experimentation need not be limited in scope and can even extend to the national level (Rodrik 2009a, 43–45). Easterly’s and McMillan’s work has also been marked by an embrace of the “piecemeal” and a commitment to “searching” as opposed to grand, comprehensive, utopian plans (Easterly 2001; 2006; 2008; 2014; McMillan 2008; see discussion in DeMartino 2011, 17, fns1, 5, 141–150). These commitments impart a clear Hirschmanian flavor to Easterly’s and McMillan’s work, though their explicit intellectual inspirations are drawn from Popper (1957; 1971) and, in Easterly’s case, Hayek (2014[1944]).

The Development Expert: Ethical Duties and Failures

Hirschman’s epistemic and normative views bear directly on his conception of the ethical responsibilities of the researcher and development practitioner. Hirschman had an ethical commitment to the view that the people of the developing world could and should be the architects of their own unscripted

future(s) (Adelman 2013a, 49; Hirschman 2013[1981b]). Hirschman wrote widely and dismissively of the “development expert” (e.g., Adelman 2013b, 323; Hirschman 1965; 1967a, chap. 1; 1969[1958]; 1971[1957]; 2013[1970]; 2013[1971]). This view reflected his embrace of the principles of autonomy and self-determination, in addition to his rejection of professional authority, epistemic certainty, and pristine blackboard planning.

Hirschman wrote often of the mutual distrust between the subjects and objects of development expertise. But his skepticism regarding the development expert was not limited to the foreign expert. He was equally skeptical of the reification of home-grown experts, particularly the nationals who return home after a period of study abroad with a sure cure for what ails their native country and with the arrogance to dismiss the ideas and experiences of those with longer historical memories (Hirschman 1973[1963], v).

Hirschman’s distrust of experts stemmed from their hubris, reductionist sensibilities, and the associated fiction of expert control, which authorized them to devise sweeping plans and to oversell their benefits and discount the likelihood and costs of their failure (Hirschman 1967a; 2013[1970], 147; 2013[1971]; see Ellerman 2005). Hirschman’s “Hiding Hand” essay masterfully dissects the rhetorical strategies that experts use to sell their reform plans (Hirschman 1967b, 19–23). These include what he terms the “pseudo imitation technique,” pretending that a project is a straightforward application of a well-known technique that has already been used successfully elsewhere, and the “pseudo comprehensive program technique,” dismissing previous reform efforts as piecemeal, and offering instead a comprehensive program without regard to the myriad uncertainties and risks that it entails. The latter strategy provides cover to experts in the event of program failure since it can always be claimed that failure resulted from not having followed the plan in full.⁴³

These themes are echoed in what Hirschman (1973[1963], 247–249) terms his “Digression on the Semantics of Problem-Solving,” where he takes on the practical power associated with labeling reforms as either “piecemeal” or “comprehensive.” Nassim Taleb (2007, chap. 6) advances similar concerns in connection with what he terms the “narrative fallacy,” or the tendency to oversimplify complex social phenomena via accounts that impose logical or causal links on sequences of events or facts. Such causal stories are dangerous to the extent that they are taken literally and induce overconfidence in the intelligibility and tractability of the world, and to the extent that they lead the expert to believe the world can and should be redesigned to conform to the elegant model that resides in his head. Long before Hirschman, Adam Smith and Henry Sidgwick had warned of the same conceits and errors (see

DeMartino 2011; Kirman 2016). Hirschman, of course, attributed the very same tendency to development experts and reformers. Easterly (2001; 2006; 2008; 2014) and Ellerman (2004; 2014) have also emphasized this concern. Like Hirschman, their views are informed by direct experience working in EMDEs and a Hayekian rejection of both the democracy-constraining and practical failings of social engineering.⁴⁴

Hirschman's critical view of development experts also stemmed from his rejection of the idea that EMDEs were somehow simpler and more tractable than AEs (Hirschman 2013[1970], 144; Rothschild and Sen 2013, 367). This view of EMDEs, prevalent throughout Hirschman's career and too often in evidence today, founded the particular zeal and naïve optimism with which economists sought to tackle the "development problem." The idea that EMDEs and the behavior of actors therein are inherently tractable is a constant in development policy. One might consider the contemporary embrace of RCTs and conditional cash transfer programs that seek to modify the behaviors of the poor through small cash payments as indicators of the continued impact of old views about the simplicity of EMDE actors, structures, and practices.⁴⁵ That spirit was also in evidence with a vengeance in the postsocialist context of the 1990s, when leading economists converged on a simplistic, largely uniform reform package for the transition economies despite their distinct institutional frameworks, histories, assets, and cultures (see DeMartino 2011, chap. 9; McMillan 2008; Murrell 1995, 164, 177).

Possibilism, Futilism, and Rhetoric

The ethical responsibility of the development expert entails a commitment to what Hirschman termed "possibilism." Hirschman's possibilism entails the idea that small-scale, messy, disparate innovations reveal what could be, and what reforms might be available. As exemplified in the concept of the Hiding Hand, possibilism is grounded in faith in the demonstrated capacities of individuals, institutions, and societies to develop diverse, creative solutions to unforeseen challenges and development problems. Possibilism encapsulates the enduring bias for hope that infuses so much of Hirschman's oeuvre (see Sanyal 1994; Lepenies 2008).⁴⁶ Central to Hirschman's possibilism is his humility and his related emphasis on uncertainty—on imperfect, deficient knowledge of what is and what could be. He counterposed possibilism with the predominant "futilism" in the social sciences (and especially in development economics)—the view that any initiatives that were not entirely consistent with the precepts of received theory were bound to fail (Hirschman 2013[1971]). Hirschman wryly observed that development economists, particularly in Latin America, had a tendency

to swing counterproductively between the poles of disaster and salvation, with no space for the intermediary state of purgatory (Hirschman 2013[1968a]; 2013[1970], 145–146). In contrast, and as examined earlier, Hirschman believed in the productive nature of ignorance, which often served as a condition of possibility for ultimately beneficial initiatives.⁴⁷

Hirschman saw “possibilism” as an ethical compass that called for a “little more ‘reverence for life,’ a little less straitjacketing of the future, a little more allowance for the unexpected—and a little less wishful thinking” (Adelman 2013a, xii; Hirschman 2013[1970], 147).⁴⁸ Hirschman concluded his essay on “Political Economics and Possibilism” by identifying “the right to a non projected future as one of the truly inalienable rights of every person and nation; . . . [setting] the stage for conceptions of change to which the inventiveness of history and a ‘passion for the possible’ are admitted as vital actors” (Hirschman 2013[1971], 30; see also Hirschman 2013[1970], 147; Adelman 2013a, xii). Hirschman famously said of possibilism that “social scientists often consider it beneath their scientific dignity to deal with possibility until *after* it has become actual and can then at least be redefined as a probability” (Hirschman 1980[1945], xii, emphasis in original). In reflecting on his own work, Hirschman said that “the fundamental bent of my writings has been to widen the limits of what is or is perceived to be possible, be it at the cost of lowering our ability, real or imaginary, to discern the probable” (Hirschman 2013[1971], 22). Elsewhere he asked, “Aren’t we interested in what is (barely) possible, rather than what is probable?” (Hirschman’s diary, cited in Adelman 2013a, xii).

So deeply was Hirschman associated with the idea of possibilism that his *New York Times* obituary had the title “Albert Hirschman, Optimistic Economist, Dead at 97” (Yardley 2012). Some of Hirschman’s contemporaries dismissed what they perceived to be the excessive optimism inherent in his case for possibilism. In a private letter, he responded to a negative review of his book *The Strategy of Economic Development* that focused on precisely this point. He wrote, “Perhaps I have overstated my case, but it seems to me that you in turn overstate the extent of my overstatement” (Adelman 2013b, 351).

Hirschman’s possibilism was not simply a matter of disposition but rather reflected both his ethical commitments and his appreciation of the practical consequences of rhetorical choices. Hirschman’s interest in the real political and economic consequences of what his biographer terms “how social scientists played with words” (Adelman 2013b, 6) was a theme that extended beyond his work on possibilism and futilism. In his view, the epistemic certainty that contributed to paradigm thinking made reality “appear

more solidly entrenched than before” (Hirschman 2013[1970], 148) and diverted actors away from available opportunities and potentially productive courses of action (Hirschman 2013[1971]). He understood theorizing the solidity of obstacles to social improvement as a practice that could increase their practical salience. He famously said that “obstacles to the *perception* of change thus turn into an important obstacle to *change itself*” (Hirschman 2013[1968b], 43, emphasis in original; see also Hirschman 2013[1970]; 2013[1971]).

Hirschman’s appreciation of the practical effects of rhetoric are on full display in his book *The Rhetoric of Reaction: Perversity, Futility, Jeopardy* (1991), his intellectual history of two hundred years of conservative rhetoric against progressive social policy (see also Hirschman 1973[1963], 247–249). Hirschman argues that conservative rhetoric fell into one of three categories, which he termed theses. The perversity thesis holds that well-intentioned actions have unintended negative effects that leave society worse off than before the intervention; the futility thesis holds that interventions will fail to produce any effects at all; and the jeopardy thesis holds that the costs of a proposed change are so great as to threaten other previous hard-won gains.⁴⁹ In developing these theses, Hirschman by no means denied the likelihood of unintended negative effects of interventions (Fukuyama 2013); indeed, he counted on them. Rather, he challenged the tendency to make the discovery of unintended negative effects a reason for opposing all deliberate efforts at reform—the tendency to allow the perfect to impede the good.⁵⁰

Lamentable Evaluative Criteria

This selective Hirschmanian tour provides substantial, useful guidance when considering contemporary developments in financial governance and developmental finance. Indeed, one can tease a method out of Hirschman’s madness—incomplete, to be sure, but good footing nonetheless for engaging irreducibly complex changes in an irreducibly complex world. The methodological prescriptions take the form of *proscriptions*—as injunctions against deep-seated academic habits and sensibilities that today infuse the social sciences, especially political science and economics. Most simply put, along with Hirschman, *we should refuse to know too much, and we should refuse to rush to judgment*.⁵¹ This imperative requires us to reject evaluative criteria that purport to determine *ex ante* or even *ex post* whether particular policy or institutional innovations are coherent, viable, sufficient, scalable, and significant.

Coherence

We should not vet new initiatives by reference to coherence criteria, adjudicating their viability based on the degree to which they “fit” into an overarching system. We should instead presume that any observed institutional innovations within and across countries will conflict to some degree or other with established institutions, just as existing institutions conflict with each other. This is equally true of innovations that are dictated by a coherent theoretical vision and those that are not. Seamless, coherent systems are neither possible nor ideal. Indeed, they are inherently risky. Similarly, we should not be concerned with whether they are redundant or duplicative in some way, in violation of some efficiency norm, or whether they are consistent with a generalized, universal theory, plan, or “ism.” Tensions between seemingly inconsistent endeavors might be more apparent than real, and even real tensions might yield unforeseeable adaptations and innovations that serve to solve important problems.

Viability

We should not presume to know whether proposed or existing innovations can exist and survive over the long term or whether some or all of them are unviable in the context of pressures emanating from the global economy, the power of global financial actors, or fragilities in EMDEs. Even those that fail may impart useful lessons (provided we are open to them) that benefit other initiatives. Hirschman reminds us that learning happens through confrontation with obstacles and failures and not just or primarily through successes. Moreover, new capacities, knowledge, networks, and coalitions may be built in the context of institutional innovations even when particular policy or institutional arrangements fail to survive. These Hirschmanian linkages or side effects may bear fruit in unexpected and unpredictable ways over the medium and long terms. Finally, full exploitation of positive and negative events for learning purposes requires a break with paradigm thinking that constrains what lessons we can take away from the rich experiential record that presents itself. Following Hirschman, we need to aspire to be Isaiah Berlin’s foxes rather than hedgehogs (Tetlock 2005).

Sufficiency

We should not be concerned with whether the observed innovations are adequate in the sense of addressing the full range of needs for developmental finance. They can’t. But then, what can? Those who apply the sufficiency test are almost invariably drawn to paradigmatic accounts that

confer the status of sufficiency only to innovations that fit into their theoretical schemes. It bears emphasis that finding any innovation (or web of interconnected innovations) sufficient requires utopian thinking where all unintended consequences, contradictions, and perversions are eliminated by theoretical fiat. That is not Hirschman's way, and with good reason.

Scalability

We should not judge innovations against the standard of whether they are scalable and even universalizable (rather than contingent or context dependent) or speculate as to whether they are doomed to remain small, barely surviving, and even then only in the specific environments where they have arisen. We should presume instead that scalability is always in part illusory and aspirational—it is a standard that is often imposed by grand narratives that require homogeneity and universality on reiterated yet context-specific, diverse constructions. Replicability but with significant variation is a less ambitious but more achievable goal than scalability—but it may be a valid objective only if we recognize that replication is a story we employ to make sense of what may be internally heterogeneous developments.

Significance of Change

Finally, we should not attempt to discern whether innovations represent fundamental or superficial changes. We must not impose a “test” of fundamental change, such as whether any particular institutional endeavor disrupts the structural power of the IMF or the United States. In addition, we should not dismiss change on the grounds that what appears to be a new development is simply a repeat of past practices in a new guise. We should presume instead that significance is always context dependent—that a reiterated construction always represents novelty owing to the unique circumstances in which it occurs. In addition, we should presume evolution rather than fixed identities and realize then that significance is revealed only over time in the process of institutional adaptation. Moreover, we should recognize the need to parse reforms as significant or insignificant as an urge driven in part by our professional training and the long tradition of futilism and epistemic certainty that marks it—one we would do well to suppress as we engage a world that is so much more complex and richer than we can capture adequately through our various paradigms and predictive models.

Along with Hirschman, we might recognize that each of these criteria reflects the drive of social scientists to repress uncertainty in the pursuit of understanding and, ultimately, control. Each constrains our appreciation of the possible, the nonsensical, the ad hoc, and the unscripted, and blinds us

to the significance and potential of piecemeal, small-scale initiatives that are now proliferating in EMDEs. It would be far better to intervene in ways that acknowledge the possibility that each might evolve with the effect of addressing pressing development problems and deepening capacities, provided they are not strangled by scientific closed-mindedness that deprives them of recognition, legitimacy, and support.

The Hirschmanian Method: Its Nature, Promise, and Risks

How might we make sense of the developments that I will examine in subsequent chapters from a Hirschmanian perspective? A central point is that the developments are not to be taken as universally good or bad, progressive or regressive, rational or irrational. Instead, we are encouraged by Hirschman to take them on their own terms, as context-specific gestures toward problem solving. This requires avoiding the kinds of evaluative criteria that predominate in much social science. In this sense, we must keep in mind the negative nature of Hirschman's intervention—to reject approaches to social science in general and the study of change in particular that presume to know in advance what developments are and are not possible, viable, and beneficial. We must avoid not just prediction but prearrangement of the institutions and practices we encounter. If we learn just this much from Hirschman, that we are not authorized or competent to tell the history of institutional evolution and social change in advance, we will be much better able to appreciate and contribute to projects of economic reform.

Even if Hirschmanian possibilism has much to offer, the approach is hardly watertight or complete. As the foregoing suggests, no particular "method," no set of techniques, falls neatly out of the approach. To the degree that the approach can be reckoned a method, it is one that will not appeal to many social scientists. It may be encapsulated simply as a *rejection of dismissiveness*, as *observation without (premature) judgment* that at once takes account of the apparent limitations and risks associated with incoherence while refusing to dismiss new initiatives as dead ends, internally contradictory, ephemeral, inefficient, or necessarily damaging to development. The method calls for a particular attitude, addressing the researcher's sensibilities as she encounters unfolding events rather than addressing her techniques per se. Hence, the approach is consistent with quantitative and qualitative methods, and with empirical and analytical inquiry. Moreover, the approach is equally open to the study of state and nonstate actors and to investigations of local, national, regional, and global developments.⁵² If Hirschman himself was often more apt to emphasize the salience of the

small and disparate, that might be taken as a corrective to what he recognized as a misplaced disregard among his peers for this level of analysis. As his oeuvre demonstrates, he was equally comfortable engaging matters of state behavior and interstate dynamics. Nothing was off the agenda, but neither was any one domain, level, or factor taken to be the key to development. We find instead a profound rejection of a simplifying reductionism that, in Hirschman's view, could only generate illusions of understanding and control.

It would be irresponsible to end our survey of Hirschmanian themes, however, without attention to certain ambiguities and risks that we will encounter in applying this framework to contemporary events. First, observation without premature judgment can be taken as a thorough retreat away from the difficult but necessary task of assessing initiatives in terms of their contributions to goals that the development economist advocates. Does Hirschman leave us in a debilitating relativist morass? The answer—perhaps more a hope—is that he does not. At issue here is not our ability to apply normative criteria, theoretical insights, and empirical techniques to the initiatives unfolding around us—something we can't escape doing—but the certainty with which we cling to our normative assessments, theoretical conclusions, and empirical findings. Hirschman implores us over and over to recognize the *productivity of doubt*—including doubt in our own deepest convictions, theoretical priors, methods, and findings. We are urged to push back against the internal and external pressures to rush to judgment, giving space and time for initiatives to evolve and for events to play out before we dot all the i's and cross all the t's in our analysis.

But how long is long enough? When can we break into the never-ending sequence of events and begin to render judgment? Is it too soon, for instance, to evaluate the present postcrisis period, since so many of the initiatives that we will examine in later chapters are in their infancy (and some are not yet even born)? The answer, I think, is that Hirschman would have us render tentative judgments at each and every moment, all the while recognizing that what we are assessing is incomplete, unfinished. Since there is no ending to the drama, there is no natural point at which we can pass final judgment. Hence, there is no contradiction in avoiding premature judgment while at the same time grappling tentatively with the events under way around us. Hirschman's possibilism asserts that in this tentative grappling we should foreground existing and emerging potential and opportunities rather than limitations and failures.

This book's central claims can now be reframed in light of Hirschman's extraordinary contributions. The positive claim is that we are today immersed

in a Hirschmanian moment, one marked by productive incoherence in terms of global financial governance and developmental finance. The normative claim is that the emergent Hirschmanian world is a good world to inhabit—one far better attuned to development than a fully coherent system. But it bears repeating a concern broached in chapter 1 and that will be revisited in chapter 8: that there is a grave risk that a global economic policy environment marked by incoherence may come to feature beggar-thy-neighbor initiatives in which efforts to insulate powerful constituencies at home from economic turbulence might offload risk and economic hardship onto others—especially the relatively powerless and vulnerable. This is not just an academic concern. For instance, many observers have called attention to the problems created for EMDEs by loose monetary policy in AEs during the global crisis, and have called for strategies that mitigate harmful spillover effects, including involvement by the IMF and other bodies (see chapter 7).⁵³ At the same time, and as we will also investigate in chapter 7, the IMF has expressed worry about the negative spillovers resulting from the proliferation of the competitive use of capital controls. It can reasonably be argued that these risks can only be managed adequately within an internally coherent global policy regime, one with mechanisms to ensure compliance among national governments. In this view, an incoherent regime is terribly risky and on that account unsustainable.

We will return to this matter in the concluding chapter of the book, once we have explored the range of initiatives now under way and as a consequence can examine the risks of incoherence more concretely than we can here. Some of these initiatives might indeed plausibly threaten economic welfare abroad even as they promote economic security at home. The question that will emerge is whether the imposition of policy and institutional conformity in line with some model or other is the best or even the only viable response to this threat. The global neoliberal vision answered that question in the affirmative, of course. But it must be emphasized that in practice that regime provided ample opportunity for powerful countries and elites to adjust the rules in non-neoliberal directions as circumstances warranted. Fidelity to the model was routinely sacrificed to protect those interests having sufficient influence. The range and extent of departures from the neoliberal ideal—such as state interventions in financial markets through lender of last resort activities and bailouts of private financial institutions—reveals that *coherent* systems are inherently risky and on that account unsustainable (Polanyi 2001[1944]). The Hirschmanian sensibilities that animate this work urge us toward an alternative answer. We are encouraged to recognize the risks of incoherence while avoiding the deep-seated fastidiousness (among

economists and policymakers) that values order and consistency for their own sake. Legitimate concern over risk should drive the pursuit of a new mindset that focuses on managing policy diversity rather than enforcing policy conformity. In a Hirschmanian world, the mandate of global institutions (such as the BWIs) must be to enable arrangements that expand policy space for valued national economic objectives. It is of course not obvious just how this might be done. Hirschman would encourage us to look for the answer through application of the same strategies that he thought were productive for development—inaugurating a perpetual chain of experimentation, trial and error, learning by doing, and persistent policy and institutional adjustment to an ever-changing world.

In chapter 3, we turn to the Asian financial crisis, an epoch-shaping event that set in motion developments that prepared the way for the Hirschmanian impulses that have deepened and spread during the global crisis and that persist today.