

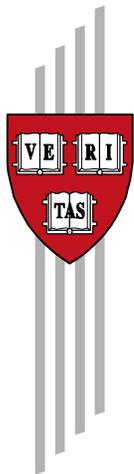
Why Does Hirschmanian Development Remain Mired on the Margins? Because Implementation (and Reform) Really is 'a Long Voyage of Discovery'

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Why Does Hirschmanian Development Remain Mired on the Margins? Because Implementation (and Reform) Really is ‘a Long Voyage of Discovery’¹

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Abstract

A defining task of development is enhancing a state’s capability for policy implementation. In most low-income countries, alas, such capabilities seem to be stagnant or declining, in no small part because dominant reform strategies are ill-suited to addressing complex non-technical aspects. This has been recognized for at least six decades – indeed, it was a centerpiece of Albert Hirschman’s understanding of the development process – yet this critique, and the significance of its implications, remain on the margins of scholarship and policy. Why? I consider three options, concluding that, paradoxically, followers of Hirschman’s approach inadequately appreciated that gaining more operational traction for their approach was itself a type of problem requiring their ideas to embark on ‘a long voyage of discovery’, a task best accomplished, in this instance, by building – and tapping into the distinctive insights of – a diverse community of development practitioners.

¹ An earlier version of this paper was presented to the Second Albert O. Hirschman Conference, held in October 2018 in Washington, DC. All errors of fact and interpretation remain my own; the views expressed herein are solely those of the author, and should not be attributed to the World Bank, its executive directors or the countries they represent. My thanks to conference attendees for thoughtful questions, suggestions and feedback. Email address for correspondence: mwoolcock@worldbank.org

The term ‘implementation’ understates the complexity of the task of carrying out projects that are affected by a high degree of initial ignorance and uncertainty. Here ‘project implementation’ may often mean in fact *a long voyage of discovery* in the most varied domains, from technology to politics.

Albert Hirschman²

In work undertaken over the past decade with my colleagues Matt Andrews and Lant Pritchett, we have sought to address three big-picture issues pertaining to policy implementation, especially in developing countries. First, how well are the world’s (national) governments able to implement their core policy agendas, and how many of these countries are showing discernable improvement? Second, to what extent are the dominant approaches taken by international development agencies to enhancing capability for policy implementation helping or hindering this process? And third, if these dominant approaches are frequently failing, why is this, how have such consistently unhelpful approaches endured for so long, and – more constructively – what might an alternative approach seek to ‘do differently’?

For the most part, we like to think we have reached reasonably solid answers to these questions (see Andrews, Pritchett and Woolcock 2017).³ Our analysis shows that the state of state capability for policy implementation around the world is, alas, not good, and that in only a dozen or so of today’s developing countries is the current trajectory of improvement on pace to reach minimum OECD standards by the end of *this century*. Such a situation prevails in no small part because the dominant approaches to institutional reform promoted by international agencies – especially those centered on adopting universal ‘best practices’ as determined by external ‘experts’ – are too often part of the problem (rather than the solution), changing what systems look like rather than what they can actually do. Accordingly, an alternative approach should thus try to focus on creating and protecting space for local actors to nominate and prioritize the problems they themselves face, then iteratively craft plausible solutions to them, sharing emergent successes through communities of practice. If dominant approaches are predisposed to providing technical solutions to technical problems (which is what they were designed to do, and for the most part do quite well) and measure success as compliance and outputs, and if building state capability for policy implementation comprises, as we suggest, key aspects that are in fact adaptive in nature, then a complementary administrative apparatus is needed to find and fit adaptive solutions to them, one that incorporates alternative metrics of success (such as, locally nominated problems actually solved). This is the argument in its starkest form; the fuller rendering in published work of course provides the necessary qualifiers, subtleties, and empirical support.

For present purposes, however, I want to briefly explore a related question that has long intrigued me. Readers of the preceding paragraph with a solid-enough grounding in the history of development practice will see clear echoes in our approach of ideas and strategies that have been articulated roughly each decade since the very ‘founding’ of the international development enterprise in the 1940s and 50s. Indeed, we explicitly acknowledge that our work ‘stands on the shoulders of giants’ – our book opens with several epigraphs showing that observers have worried for *thousands of years* about ‘reforms’ that merely change appearances to comport with others’ expectations rather than generate more effective capabilities to deliver a coherent policy agenda. As such, we pitch our approach as a ‘perpetual second word’ on institutional reform: if those making truly original claims articulate the

² Hirschman (1967: 35); emphasis added

³ Related empirical support is provided in Buntaine, Parks and Buch (2017) and Honig (2018).

‘first word’ and those who devise definitive proofs own the ‘final word’, we hope instead to help reformers and funders alike recognize that institutional change, especially public sector change, is a very particular kind of challenge requiring a corresponding fit-for-purpose response. It is a challenge for which there isn’t, and never will be, a singular solution but rather as many solutions as there are specific instantiations of the problem, even as general principles can be readily discerned.

So, in all this, the particular set of questions that intrigues me are the following. If the core themes we articulate in *Building State Capability*⁴ (and continue to refine today) are essentially correct and have in many key respects been thoughtfully voiced numerous times in recent decades, why are they still so marginal to contemporary development theory and practice? Did this earlier work not ‘stick’ – or become ‘mainstreamed’, as we might say today – because it contains some fatal-but-unacknowledged flaw? Was it fundamentally misguided, meaning that it is only a matter of time before our own folly becomes apparent – and more fool us, since we should have known better? Or might this time be different? More broadly, if today’s admirers of Hirschman’s approach to thinking about and ‘doing’ development are to secure more intellectual, policy and operational traction than their predecessors, what should they themselves be doing differently?

In the short space I have here, I will attempt to outline my response to these questions. The bottom line is that I think our forebears were correct in their diagnosis of why so many “projects that are affected by a high degree of initial ignorance and uncertainty” so often yield disappointing outcomes, and I think those of us reaching similar conclusions today are also right – and in fifty years’ time, when our grandchildren conduct a similar analysis and (inevitably) reach a similar conclusion, will still be right. What I think our forebears inadequately appreciated, however, was that trenchant supply-side critique alone was never going to be enough to dislodge orthodoxy. Securing lots of nodding heads in academic seminar rooms is one source of validation, but – as Lant Pritchett likes to say – you don’t beat something with nothing: to dislodge (or at least carve out non-trivial space alongside) a powerful incumbent, you have to be willing to provide a supportable and implementable demand-side alternative, and therein build out a vibrant social movement among authorizers and users of this alternative to demonstrate that, in time and under certain specified conditions, it can indeed yield superior development outcomes. If “this time might be different”, it is because new technologies now enable us to build out this social movement intentionally, rapidly, at low cost and at a large scale. In its own way, this is precisely what our ‘Building State Capability’ work⁵ is trying to do by making both our book and our training programs available for free online to anyone anywhere, and by ensuring that essentially all the work is ultimately done by participants themselves (not us).

But since this is largely an academic conference, let me get to this conclusion by first briefly surveying the lineage of key thinkers on whose shoulders we stand. (Doubtless there are other contributors we have entertained unawares, or inadvertently overlooked, but for present purposes these figures are emblematic.) I begin in 1935 in Indonesia, the decade prior to the birth of the multilateral development organizations, as domestic leaders prepared for the post-colonial moment (in this case, from Dutch rule). One such leader, Ki Hajar Dewantara, was focused on education, and how the newly sovereign Indonesian state would build its own system to prepare the nation’s rising generation of citizens and workers. There was no shortage of external advisers offering

⁴ See Andrews, Pritchett and Woolcock (2017)

⁵ This program of work is summarized and shared at: <https://bsc.cid.harvard.edu/>

their prescriptions, but alas, Dewantara lamented, the options presented to Indonesians “often fits so ill with our own style or is so far removed from it that we can use it at best as a decoration and not as material to build with. ... there has been so little to choose from.”⁶ The wonderful metaphor of reform as ‘decoration’ perfectly captures what we have called ‘isomorphic mimicry’: change that yields only the illusion of change, altering appearances to please others and buy short-term legitimacy rather than crafting systems grounded in, and emerging from, locally legitimate material and decision-making processes.

But whether building large administrative systems from scratch or reforming existing ones, astute observers have long recognized that certain key aspects of such processes are legal/technical in nature, and thus readily amenable to discrete specialized inputs from professionalized experts (e.g., lawyers, accountants, economists, software designers). More broadly, a central task of professional associations is to discern, certify and (as necessary) enforce ‘best practices’ – i.e., standardized (often codified) procedures that, when faithfully deployed, reliably yield the desired outcome (Behn 2017). Adopting ‘best practices’, by definition, spares reformers the need to waste time and money experimenting with alternatives: adoptees need only to be ‘trained’ in the new procedures for efficiency gains to be duly realized. But these same observers have also been quick to stress that while adopting certain ‘best practice’ elements may be very necessary, they are also very insufficient: successful organizational reform, at scale, requires engaging with large numbers of people and their associated (and often highly idiosyncratic) identities, values, motivations, incentives, aspirations, fears, preferences, abilities and obligations. Moreover, where there are people there is politics: hierarchies, power, resources and rules whose salience is only partially capturable – or in Scott’s (1998) delightful phrase, ‘rendered legible’ – by formal administrative instruments such as contracts, forms, budgets, organizational charts and reporting lines (important and necessary as they may be). Apprehending and discerning the significance of the ‘illegible’ aspects requires different research methods that in turn inform different support strategies for navigating the reform process.

This summation broadly captures the key insights formalized in classic works at the nexus of public administration, planning and development that appeared roughly each decade from the 1950s onwards. The scholarly work began with Charles Lindblom (1959, 1979), who famously argued that, inelegantly as it may sound, ‘muddling through’ was likely to be optimal strategy for navigating complex reform processes. Thereafter Hirschman himself (1967⁷) spelled out these challenges in more granular detail, using ‘live’ development projects as spaces wherein astute observers could (a) assess the peculiar dynamics shaping how general administrative principles were actually put into practice in particular places, and (b) infer, on the basis of these experiences, broader principles for development theory, policy and strategy. Hirschman’s insightful observations explaining, for example, why projects *always* cost more money and take more time than anticipated was borne of a corresponding long-term perspective wherein the array of net benefits of these same projects were also unanticipated in the planning stages, as was the fact that implementers proved consistently adept at solving problems along the way. Tweaking Adam Smith, Hirschman called this conjuncture of mechanisms the ‘principle of the hiding hand’.

⁶ Cited in Harper (2011: 193)

⁷ See also, jointly, Hirschman and Lindblom (1962).

Subsequent work by Flyvbjerg and Sunstein (2015) assessing a sample of over 300 major projects has sought to show empirically that such ‘beneficence’ on the part of planners appears in fact to be a relatively rare phenomena; vastly more common – 2.5 times more likely, they find – was a ‘malevolent’ form of the ‘hiding hand’, in which large projects consistently missed performance targets because of predation on budgets and contracts by unscrupulous participants. With a vastly larger data base, Williams (2017) draws on sample of 14,000 development projects in Ghana and finds that a third of them ‘failed’ – a result, he argues, less of ‘corruption’ or ‘clientelism’ than of perennial collective action challenges, manifest in particular in public financial management issues. Whatever the “ratio” of beneficence to malevolence in the planning/management of development projects generally (or specifically), for present purposes the two enduring points are that effective implementation matters, and that doing so includes forging a robust organizational capability to resolve (to professional standards) unanticipated – and indeed unanticipate-able⁸ – problems. The important recent work by Honig (2018), conducted on an even larger database of projects from around the world – reaches a similar conclusion.

Concerns with the limits of modern planning systems continued in the 1970s, voiced in a seminal paper by Rittel and Webber (1973). Here again we find a deep frustration with the abiding mismatch between what prevailing administrative systems are designed to do (i.e., manage narrow, codifiable tasks), and the broad array of (idiosyncratic, non-codifiable) tasks they are routinely asked to do. “[W]e are all beginning to realize”, Rittel and Webber (1973: 159) lamented

that one of the most intractable problems is that of defining problems (of knowing what distinguishes an observed condition from a desired condition) and of locating problems (finding where in the complex causal network the trouble really lies). In turn, and equally intractable, is the problem of identifying the actions that might effectively narrow the gap between what-is and what-ought-to-be.

Squeezing such challenges into a single administrative apparatus is doomed to disappointment, they argued, because “the problems of governmental planning—and especially those of social or policy planning—are ill-defined; and they rely upon elusive political judgment for resolution. (Not ‘solution.’ Social problems are never solved. At best they are only re-solved—over and over again.)” (p. 160)

In the 1980s, such enduring discontent prompted Rondinelli (1983) to argue that “international assistance programmes for developing countries are in urgent need of revision” precisely because of the inherent “uncertainty and complexity of the development process”, the levels and forms of which could not be adequately accommodated by the dominant planning systems. Instead, he maintained, development projects should be regarded as “policy experiments” – that is, as specific instantiations of ideas “that facilitate innovation, responsiveness and experimentation”, thereby promoting “decision-making processes that join learning with action.” In the early 1990s, Uphoff (1993) provided a detailed concrete example of such an experiment, showing how a dedicated team had eschewed “Newtonian” social science⁹ to re-build one of Sri Lanka’s largest and most conflict-ridden irrigation systems. More

⁸ Kauffman (2016: 2) defines such problems as ‘unprestatable’ (which is equally inelegant, but accurate nonetheless): that is, challenges so complex that no manner of experience, evidence, intelligence, preparation or wisdom could have anticipated the problems associated with trying to solve them. The more popular expression is that deeply complex challenges are replete with unknown unknowns.

⁹ That is, social science aping (a version of) the physical sciences, in which the world is viewed as a ‘machine’ whose underlying causal mechanisms are optimally apprehended by isolating and analyzing its constituent ‘parts’.

broadly, Scott (1998) showed that the widespread deployment of “high-modernist” logic in the post-colonial period – manifest most conspicuously in deference to foreign expertise (especially in agriculture, finance and land management¹⁰) and the introduction of new managerial systems of public administration, all in the name of promoting national ‘development’ – could only ever partially “render legible” the deep cultural and institutional diversity on which such sectoral activities rests. As such, these reforms, and the development projects to which they gave rise, mostly only helped fledgling governments to “see like a state” rather than build local legitimacy and actual functionality. In being able to “see” but not “act”, however, they ended up “looking like a state” (Pritchett, Woolcock and Andrews 2013) while too often failing to function like one.

Anchored, as it were, by Hirschman, we can thus see an oft-repeated claim spanning the planning and public administration literature in the second half of the twentieth century (and counting), namely that purposively modernizing economies, societies, and polities via development policies and projects is a highly complex undertaking – so complex, in fact, that a single administrative system (and underlying logic) can only get you so far. As useful as it may be for certain technical tasks, there are real limits to the extent to which logframes etc can be expected to engage with adaptive challenges, the presence of which become both more ubiquitous and more consequential as development itself takes place. Such challenges require a different approach, hints of which can be seen in specific cases (such as Uphoff’s Gal Oya irrigation project, and much of the work on common pool resource management that netted Elinor Ostrom a Nobel Prize), especially where success has occurred in unlikely places. But, alas, all this work over all these decades remains marginal to contemporary mainstream development theory, policy, practice and research. But why? Why has such a clear, long-standing and compelling account of a central development challenge, complemented by (the broad outlines of) a coherent and supportable alternative, largely failed to dislodge orthodoxy?

Three broad answers logically suggest themselves. First, the work of Hirschman and his disciples, while perhaps compelling on the surface, may nonetheless contain fatal flaws that, in time, have rendered it intellectually and/or operationally suspect. If so, perhaps it has just inexorably collapsed under its own weight, unable to deliver on its promises. A second possibility is that Hirschman et al’s work has inadequately engaged dominant approaches and disciplinary practices (especially those of economics) on terms demanding a more serious hearing. From this perspective, Hirschmanian development has remained marginal, whether by design or default, not because it is fundamentally unsound but because it has failed to convey its central analytical and empirical claims using the methods and models demanded of everyone else. A third answer could be that, despite robust evidence and adequate communication, thinkers from Lindblom onwards have spoken primarily to – and sought their legitimacy from – a niche academic audience, winning admiring followers on campus on the ‘supply side’ of ideas production across successive generations, but never seeking to build a sizeable and politically influential ‘demand side’ constituency in the corridors of power where key decisions affecting development policy and practice are made. Put differently, perhaps Hirschman et al have been too concerned with ‘preaching to the choir’ rather than having the confidence of their convictions and seeking to forge a large base of active support among those actually doing development.

Which of these three responses provides the best answer? The first option, while plausible, has little basis in the literature – researchers may quibble with or outright challenge some of Hirschman’s

¹⁰ See Hodge (2007).

key ideas, but no-one denies his originality and deeply insightful ways of engaging with development issues. Indeed, Hirschman's work remains one of the best in social science to "think with": of the thousands of books or articles ever published with the words 'economic development' in the title, Hirschman's (1958) *The Strategy of Economic Development* is ranked fourth, with over 13,000 citations since its publication in 1958.¹¹ And if operationalizing his approach continues to be an enduring challenge, I suggest this says more about the entrenched nature of incumbent approaches than it does about the intellectual veracity of potential rivals. The very durability of Hirschmanian development theory (albeit at a relatively modest scale) implies that it is highly unlikely it will ever be empirically "refuted" (at least as this winnowing process transpires in 'normal science').

The second option, however, has more traction. Perhaps the most stinging critiques of Hirschman and his followers was offered by Paul Krugman (1994) in a (in)famous article called 'The Fall and Rise of Development Economics'. For Krugman, the fatal flaw in Hirschman's approach was not his ideas per se – which Krugman both admired in principle and argued had, over time, been largely vindicated. Rather, it was Hirschman's unwillingness and (seeming) inability to formalize his key ideas into clean mathematic models, the hallmark and lingua franca of serious economic theory. In one particularly graphic passage, Krugman asserted that Hirschman (and other producers of what Krugman called 'high development theory', such as Gunnar Myrdal) had "rejected ... a willingness to do violence to the richness and complexity of the real world in order to produce controlled, silly models that illustrate key concepts." Such a stance, Krugman argued, had led Hirschman into a self-imposed "intellectual exile", a product of having "proudly gathered up his followers and led them into the wilderness himself. Unfortunately, they perished there." (p. 40)

What to make of this critique, 25 years on? If economics dominates development research (as it does¹²), and if formal models define 'serious' economic work (as it does), then the reluctance/refusal of Hirschmanian social science to play by these rules is a mark of either weakness or constrained strength. It is weakness if such work can and should be 'modelled' in relatively conventional terms, but in eschewing this approach cedes the vastly greater influence it might otherwise have; it is constrained strength if collapsing such work into the strictures of formal models really would "do violence to the richness and complexity of the real world", thereby diluting its substantive force and distinctiveness. Reasonable observers can support either view (or perhaps elements of both), but together they have left Hirschmanian social science playing only a marginal role in shaping mainstream development theory, research, policy and practice.

My own view, as noted above, aligns mostly with the third option, namely that the enduring marginality of work inspired by Hirschman is a function of failing to prioritize building out a complementary social movement among development practitioners, drawing on *their* collective experience and expertise to demonstrate its operational utility. Intellectual coherence and empirical support are very necessary but very insufficient bases on which to bring about political and administrative change; it also requires active and growing support from those who will do most of the day-to-day work of authorizing (financially, politically, legally, administratively) and implementing whatever the alternative(s) turn out to be. The scholarly merit of Hirschman-inspired work has stood the test of time – indeed, as noted above, one could say its importance only continues to rise – and has

¹¹ According to Google Scholar citation counts, as processed by Harzing's 'Publish or Perish'.

¹² On this point, and its associated consequences, see Rao and Woolcock (2007).

done so by retaining its structure and communicative style (rather than forcing itself into the vernacular of mainstream economics). The task ahead is to take advantage of the vastly lower costs of global outreach (made possible by the internet and social media) to harness the energy and insights of those best placed to build a 21st century administrative infrastructure using 21st century tools for responding to 21st century development challenges – namely, practitioners. After six decades, the critiques of orthodoxy are well-established, as are the core principles that should guide what comes after it: the missing link in the change process is harnessing the well-spring of largely untapped energy, ideas, skills and experiences from ‘operators’ – as opposed to ‘providers’ – of development projects, so that they can own and construct whatever comes next. Paradoxically, perhaps, followers of Hirschman’s approach inadequately appreciated that gaining more operational traction for their approach was itself a type of problem requiring their ideas to embark on, and be refined by, ‘a long voyage of discovery.’

I conclude, as I started, with a quote from Hirschman – but this one is my favorite (and perhaps it is less well known, since it comes from an interview question posed many years ago) because it so deftly captures my own sense of what social science should seek to do, especially in the name of ‘development’. The central objective of such a social science is less one of devising or identifying ever more ‘rigorous’ (and thus narrow) prescriptions for enhancing human welfare, but graciously accepting that much of what makes us human is not knowable, and that we are in fact collectively diminished if we presume otherwise. Our task, instead, is to

treat human beings as something fairly precious and not just something you can play upon. You see, if you ever could figure everything out, if you could have a social science that really is a science, then we would be the first ones to be disappointed. We would be dismayed because if man becomes like that, he could be figured out. And that means that he is not worth as much as we think.... Were we ever to succeed, then mankind would have failed!¹³

It is precisely because of our learned recognition of the inherent limits of academic social science, in other words, that those of us who earn a living in this way should more readily cede to others much of the important work needed to reform development theory and practice. Ideally, this type of work should be undertaken through an ongoing conversation between researchers and practitioners, and, course, between researchers who themselves are willing and able to sensibly ‘trespass’ (Hirschman 1981) into different disciplinary domains – an aspiration that, lamentably, perhaps too few of Hirschman’s followers have fully embraced.

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¹³ Interview with Richard Swedberg, in Swedberg (1990: 164). This quote is also the note on which I also conclude a class I have taught for many years at Harvard Kennedy School on social institutions and economic development.

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