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Institutions *really* don't matter for development? A response to Chang

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Abstract: Ha-Joon Chang, in his article 'Institutions and Economic Development: Theory, Policy and History', argues that the institutions and development literature is theoretically and empirically flawed, trapped by a failure to account for the contextual factors that influence the institutions that drive development. These judgments emerge from a lack of appreciation of the methodological standards of that literature and, especially, from a description of the institutions and development literature that few contributors to it would recognize. At the same time, despite the emphasis he gives to contextual factors, his critique ignores the role of politicians and the progress made in the institutions literature in understanding their incentives to pursue development.

Two central conclusions emerge from the institutions and development literature: private investment and economic growth are substantially less, on average, in countries where private investors face greater threats of opportunistic behavior by third parties (other private actors or government); and politicians often have weak incentives to prevent predatory behavior and to pursue the economic well-being of all citizens. Misinterpretation of these results is pervasive. The governance agenda advanced by donors for development assistance, though putatively rooted in these findings, privileges the fight against corruption over raising the number of actors who can invest securely. It typically delves superficially or not at all into political incentives to pursue development. Meanwhile, some scholars reject *both* the donor agenda and the conclusions of the institutions and development literature. They are skeptical of the evidence and wooed by the extraordinary success of some countries that look nothing like the ideal type portrayed in donors' 'governance consensus'; they are confident in alternative models of development that usually exhibit a large role for government; but also, like donors, they extract messages from the institutions and development literature that are not really there.

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The article by Ha-Joon Chang (2011) follows squarely in this tradition. He argues that development is more likely the cause than the effect of 'good institutions'; the development effects of economic freedom are vastly overrated; robust private property rights are not necessary for vigorous economic activity; the evidence and methodologies that underlie cross-country estimations of the development effects of growth are irredeemably flawed; and the two, mutually incompatible theories of institutional change in this literature, one arguing that politicians choose the institutions that maximize their interests and the other arguing that climate and culture are destiny, are cramped, narrow and contrary to reality. He proposes a research agenda that focuses on contextual factors, taking 'both structural constraints and real human agencies seriously', and that avoids relying on historical 'fairy tales'.

This article examines three of the discrepancies between the claims of Chang's paper and those of the literature. One is methodological: the standard of proof in the contextual literature differs from the standard that Chang (2011) applies to the institutions and development literature. The second is definitional and analytical: many of the criticisms in this paper flow from a distorted picture of the institutions and development literature. The third is political: although his article emphasizes contextual factors, it pays scant attention to the incentives of politicians to refrain from predatory behavior.

In contrast to the methodological approach recommended in Chang (2011), the institutions and development literature (whether North and Weingast, 1989, or Acemoglu and Robinson, 2006, or Knack and Keefer, 1995) focuses on the formulation and empirical assessment of testable hypotheses. Can one construct a convincing counter-factual to support the claim that, but for the presence of these institutions (or anything else), countries would grow more slowly? The opponents of this approach, with their focus on context and a seeming aversion to the use of Occam's razor, judge these efforts to be unpersuasive. The factors that matter for institutions and development are so many and so difficult to observe across contexts (geographic or temporal) that it is not possible to validate any 'but for' statement. They reject, as Chang rejects, cross-country empirical evidence for its reliance on flawed measures of institutions and insufficient attention to omitted country characteristics. But they also dismiss case-study evidence for being 'potted' and paying insufficient attention to factors that they believe are important.

The critique fails to note that institutions and development literature has two significant advantages. First, it employs theories that are clear about the path of causality and which are the 'exogenous' and which the 'endogenous' variables. While data limitations are often severe, at least the gap between theory and data is transparent. Second, the standards for rejecting a theory are clear. Even if the explanations are sometimes narrow, the standard of proof is both transparent and more rigorous than the selective use of particular cases relied on in Chang (2011).

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The second discrepancy is definitional and analytic. Chang's (2011) critique cuts indiscriminately across subjects ranging from legal systems, state-owned enterprises, financial regulation and corporate governance to corruption and political systems. However, the most important contributions to the study of institutions and development have nothing to do with any of these topics. Douglass North and Barry Weingast or Daron Acemoglu and James Robinson, for example, talk about the security of private property, not about whether private ownership is better than public, nor about standards of corporate governance, nor about any other item on this lengthy list.

The strategy of grouping diverse topics under a single rubric sows confusion because they concern largely unrelated questions. The property rights literature focuses on the risk of opportunistic behavior; the purpose of institutions is to curb that risk. The literature on state-owned versus private enterprise asks whether manager incentives to reduce costs and increase productivity are lower in the first and, if so, whether because of state ownership, *per se*, or because of the monopolistic markets that often accompany state ownership. In the financial regulation literature, the key concern is moral hazard: can regulators insure against bank runs without encouraging banks to take on excessive risk?

The reluctance in Chang (2011) to grapple with the underlying analytical issues in the literature on institutions and development leads to some puzzling digressions. He uses selected examples to question the conclusion that unregulated markets grow faster, but the institutions and development literature does not concern itself with this topic. On the other hand, the very large and sophisticated literature that does deal with it is not cited. Another section begins by criticizing the 'currently dominant discourse' for failing to acknowledge that state ownership could be more efficient. A large, nuanced and uncited literature looks at this issue, but it has nothing to do with the *security* of property rights, where the question is not who owns the property, but, given the distribution of rights over property, can individuals assume that their investments are secure?

Chang (2011) cites work on common property and township and village enterprises (TVEs) to tout the potential virtues of communal ownership. While *communal* ownership is, again, unrelated to the *security* of property rights, these examples actually support the claim that secure property and contract rights are essential. Any reading of Elinor Ostrom's path-breaking work reveals that the collective management of common resources is hard because of the propensity of human beings to free ride on the actions of others. Her work documents the tremendous variety of local institutional responses that communities have employed to try to prevent such opportunistic behavior by group members. Gehlbach and Keefer (2009) examine TVEs in China and ask why party cadres were so confident that the fruits of their efforts would not be expropriated by upper-level party officials, particularly so soon after the difficult years under Mao. Their answer: institutional reforms inside the Communist Party made it more difficult for party leaders to act arbitrarily towards subordinate cadres.

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Politics and the incentives of politicians are a persistent theme. Methodologically, they are a key contextual factor that affects causal interpretations. Analytically, they are central to the question: when do politicians support reforms, such as the institutionalization of the ruling party – or the centralization of rents – that allows for credible commitments to investors? The third discrepancy between Chang (2011) and the institutions and development literature is simply that the latter takes politics and the incentives of politicians seriously, while those incentives rarely fall within the set of contextual factors of the research agenda that Chang (2011) prefers. In this sense, the difference between these two research agendas is not that one pays attention to context and the other does not, as Chang (2011) maintains. It is rather that the institutions and development literature focuses more on one set of contextual factors (political incentives and agency); the other emphasizes another set (historical and sociological factors).

These are surely complementary agendas, but one could not tell this by reading Chang (2011), who dismisses without discussion efforts to explore history in the institutions and development literature as 'fairy tales', and ignores altogether the analyses of political incentives that underpin this literature. Those incentives are crucial, though. Khan (2000) and Kelsall and Booth (2010) argue that centralized rents managed for the long term can drive development, even in high-corruption settings. What persuades claimants on rents to cede power to central authorities? Under what conditions do authorities have long enough horizons to manage for the long term? What ensures that the commitments they make are credible (e.g., to limit subsidies only to infant industries that begin to export)? The institutions and development literature has begun to address these questions.

For example, North and Weingast (1989) identify the conditions under which parliamentarians in post-Glorious Revolution England were able to act collectively to impose their will on a powerful monarch, allowing for centralized and long-term management of rents. Acemoglu and Robinson (2006) emphasize the importance of collectively organized citizens – those who can threaten revolution, for example – as a constraint on leader expropriation ('short-termism'). Keefer and Gehlbach (2009) underline institutional changes ('ruling -party institutionalization') within the Communist Party that facilitated collective action by cadres against leaders who renege on their commitments – maximizing for short-term, rather than long-term gain. Keefer (2007) emphasizes that in young democracies, political competitors have more difficulty making broadly credible commitments to citizens, leading them to prefer narrowly clientelist overly broadly developmental policies. Keefer (2008) attributes the problems of credible commitment to the inability of citizens to act collectively; when they can, for example, when they can turn to programmatic political parties, this ability serves as a bulwark against conflict: it is more difficult for governments to resist the collective demand of citizens for better security and more responsive public policies, which simultaneously deter insurgency. This line of reasoning explains

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why democracies do not always succeed – in many democracies, citizens have no capacity to act collectively. In some autocracies, in contrast, a substantial number of citizens (60 million members of the Chinese Communist Party) do have the right to act collectively, yielding a strong basis for investment and growth.

The study of institutions and development has not answered many of the important questions about the sources of institutions or development, and researchers in this area have much to learn from the work of scholars in cognate areas. However, rather than point to opportunities for mutual learning, Chang (2011) mischaracterizes the arguments in this literature and criticizes it for arguments it does not make. And he attacks its methodological approach, but offers nothing to replace it, beyond the selective use of examples and counter-examples, to answer the questions that we all care about. Are secure property rights essential for development? For whom? And how do they emerge?

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