

Commentary

Rethinking modernity and capitalism:
Add context and stir
by Jan Nederveen Pieterse

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There is much to be appreciated about Professor Pieterse's essay on modernities and capitalisms. It very ably takes macro theory apart. The criticisms of macro theory generally make sense, and do so even when applied to an extremely large and diverse set of theoretical approaches. The critiques of convergence are also on the mark. The world is layered, multiplicity is the norm, and context matters. Modernities and capitalisms are plural. The intervention thus appropriately widens the scope of analysis. Most importantly, it is disruptive, calling for social science that is outward looking, evidence-based (though not in any narrow way), and self-aware – particularly in the face of confirmation bias.

My comments here stem from three interrelated and ultimately practical questions arising out of his critique. What functions can social science serve in a world of extreme context-specificity? How can social scientists serve these functions if medium-range theories and specific hypotheses derive meaning from macro theories that are often inapplicable? And is it indeed the case that medium-range theories and specific hypotheses only derive meaning from larger, macro narratives, which are generally totalizing?

I will attempt to sketch some answers, drawing on trends in development economics. This is a field of study that has from the start emphasized context-specificity and pushed back hard on totalizing narratives. Yet it also currently risks, as Professor Pieterse warns, becoming lost in the thicket of small-bore questions and tinkering on the margins. Perhaps most importantly, large strands of development economics are dedicated to answering his closing question regarding which varieties of capitalism are most suitable under which circumstances. The examples I provide will mostly confirm his critique, but will also

raise some questions, and underscore the need for further practical guidance.

Social science obviously serves many functions. Some, such as the inquisition and reshaping of grand narratives, are much more difficult to achieve in a world of variety and context. This appears, implicitly, to be the function with which Professor Pieterse is concerned in his essay. If this were the only function of social science, I would be in total agreement and have little to offer. Yet, this is not the primary function pursued by development economists, nor indeed by many other social scientists. Rather, many of us treat social science principally as a means of making better social choices. Policy relevance is the key criterion. Unlike grand narrative-making, a task whose urgency may diminish with our odds of succeeding at it, informing social choice is a function of social science that is arguably rendered *more* urgent and relevant by context-specificity. I focus on this function for the remainder of my comment.

Development economists have long understood that context has lasting effects on social outcomes. For example, unequal asset ownership has been clearly understood to influence resource allocations and output (e.g. Dasgupta and Ray, 1986), efficiency (Greenwald and Stiglitz, 1986) and class structures (Sadoulet et al., 1998). Institutional economists, who were readily welcomed into the fold, argued that socially suboptimal institutions could be persistent (Akerlof, 1984a, 1984b). Perhaps most influential in terms of policy, theories of learning-by-doing (Arrow, 1962) and big-push industrialization (Rosenstein-Rodan, 1943) emphasized multiple equilibria and path dependence, so that choices today shift contexts tomorrow, which in turn alter the choices then available and their relative merits. None of this sat well

with simplistic Walrasian thinking, according to which the simultaneous clearing of multiple markets and incentives to push towards efficient outcomes erased any imprint of initial conditions or context on aggregate social outcomes.

What makes these types of contributions relevant to the critique at hand is that they almost all involved only medium-range theory. Neoclassical economics served as the macro paradigm that gave them meaning, at least in terms of methods, logics and general concepts (prices, costs, stylized production technology, and rational, marginalist decision-making). And yet the paradigm was assuredly not totalizing. The work just described transcended many of the Cold War political biases that marked much neoclassical economics in the second half of the 20th century, and not only opened up new ways for social scientists to think about history and institutions, but also for policy makers to think about their mandates. These ideas influenced social choices, providing an intellectual bulwark (clearly not always successful) against the worst excesses of *laissez faire* economics in the 1990s, and substantially influenced economics itself (Bardhan, 1993).

How shall we categorize this type of social science? It clearly is not tinkering on the margins. One possibility is that despite giving this body of work its symbols and meanings, neoclassical economics is not in fact a macro theory, but a method. If so, this would imply that substantive, meaningful social science was conducted without the benefit of a macro theory. Alternately, we may have here an example of a macro theory that *is* capable of organic change. I conclude that either the argument that macro theory is necessarily totalizing is too strong, or (my preferred option) that the conditions under which medium-range theory is useful and meaningful are less stringent than the essay requires. The latter possibility will return when considering ways forward. It is also directly underscored by the next chapter in the history of development economics.

A common feature of the theories described previously is that predicting the effects of policy changes becomes extremely difficult when several dimensions of context interact. This multiplicity of theoretical possibilities motivated several authors to begin seeking natural experiments in the 1990s, and shortly after to begin running randomized field trials in order to obtain estimates of the causal effects of 'policy shifts'. Much of this work has been fascinating and useful (Duflo, 2006). Unfortunately, it has also come to exert an outsize influence on the field, despite suffering two major problems. First, only a fraction of interesting policy changes are amenable to experimentation and plausible identification of causal effects (a subsidy here, an educational inter-

vention there, etc.). The policy differences that are large enough to catalyze structural or socio-political changes and that demarcate different varieties of capitalism cannot be studied in this way, and are therefore eased out of the mainstream literature and consciousness. Acknowledging context-specificity is healthy, but it can become a straightjacket.

Second, many PhD students studying conditions in low-income countries today have become expert in the identification of causal effects, but are remarkably unfamiliar with the theoretical and institutional considerations that led to the adoption of the experimental approach in development in the first place. Thus, having run an experiment and obtained predictions of the effects of policy in one tightly controlled environment, they are at a loss regarding how it might be applied in a different environment. The problem is not, as it is commonly argued, that the results of the experiments are not 'externally valid' (i.e. useful in other contexts). Very few plausibly are. The real issue is that the student of development who is not steeped in history, multiple contexts and medium-range theory has little basis for judging the likely effects of the policies they study if they are applied in a different setting. A key purpose of social science training in a world of context-specificity – improving the practitioner's judgment when confronted with a new context – is lost.

These examples underscore the validity of Professor Pieterse's broader argument about the centrality of plurality and context, but suggest that what we lack is facility with medium-range theory and knowledge of underlying conditions, not applicable macro theory. In fact, it is *because* context is king that policy-relevant analysis is often small- or medium-bore.

This level of analysis also reflects the actual distribution of power to make social choices. Few in the policy realm really have power to reorient the main thrust of policy, institutions and social organization (i.e. to choose between capitalisms, writ large). Even those who have had this power have often proceeded by solving important problems incrementally at the margins. For example, drawing, as Professor Pieterse does, on the Chinese experience: agrarian policy under Deng Xiaoping proceeded step by step with reforms of agricultural procurement policy and land rights with a view to increasing output while controlling inflation (Liu et al., 1998; McMillan et al., 1989). The subsequent development of Chinese labor policy in township and village enterprise displayed a similar attention to administrative detail, incentives and context (Meng, 2000). Both reforms revealed a penchant for proceeding through trial and error, informed by a practical understanding of the role of context. It is this careful understanding of the

role of context that I call medium-range theory. It is not a coincidence that Deng, like the author, forswore macro theory (black cat/white cat), and embraced the potential multiplicity of capitalisms, both across territories ('One China, two systems') and between rural and urban China (entrenching *Hukou*). Whether one is sympathetic to any particular goals of these policies or not, it should be clear that medium-range theory and small-bore analysis were central and meaningful in pursuing those goals while upending the macro theories (the various incarnations of Maoist thought) that had come before.

All of this actually strengthens Professor Pieterse's argument in one respect: macro theory is not only insufficient for useful analysis in a context-specific world; it may not even be necessary for serving one key function of social science research.

Finally, the article regards macro theories as misleading, medium-range theories as lacking meaning without macro theory, and nomothetic approaches to developing theory as doomed to failure. So, what size and type of theory, and what approach to developing new theory are the right ones in a world of context and multiplicity? The article does not say, offering instead interdisciplinary comparative studies (good) but no theoretical path forward. Without even mid-range theory, such comparative studies are always backwards looking, uninformative of future choices; critical of existing arrangements, but too frequently mute on what to replace them with. Thus, while the final question is indeed the right one, 'which roles and which combination of government and market forces are best under which circumstances?', the article is not clear on how it is to be answered. I have argued that if the objective of social science is to inform social choice, and macro theory is misleading, then we should celebrate small- and medium-bore analysis on the near margins. It may be all we have, and it can be very powerful.

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