**Impoverished Economics?**

**A Critical Assessment of the New Gold Standard**

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**Abstract**

This article situates the 2019 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel in the history of thought on development, tracing how the focus, theory and methods have shifted in the field. The article evaluates theoretical and methodological critiques of the way randomized control trials (RCTs) are employed by the laureates, as well as attempts to overcome these challenges. In terms of theory, the article demonstrates what gets lost when experiments are guided by methodological individualism and assumptions of individual utility-maximizing behavior. In terms of methodology, the article unpacks the limits of RCTs related to their causal model, and their lack of attention to human agency and wider socio-economic context. Finally, the article discusses the experimental approach’s relevance for research and policy-making and cautions against any approach becoming a “gold standard,” due to the importance of pluralism for maintaining an open debate about development.

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This article provides a cautionary counterweight to widespread calls for celebration of the 2019 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, which was awarded to Abhijit Banerjee, Esther Duflo and Michael Kremer. While it is good news that this year’s Prize directs attention to the persistence of global poverty and the need to do something about it, social scientists have long provided thorough critique of the turn towards experimental approaches to fighting poverty that this Prize rewards. Concerns with the approach, which has been likened to a methodological “gold standard,” can be roughly grouped into questions of focus, theory, and methodology.

Critiques of the focus of the laureates’ work deal with the types of questions that are possible to ask with their preferred method of randomized control trials (RCTs). Their focus is on experimenting with interventions that might generate positive results at the micro-level, such as improving incentives in a variety of ways to improve health and educational outcomes. Such a focus directs attention away from the macro-level issues of global organization of production and finance that cause the high degrees of inequality and poverty to begin with. Rewarding a focus on more narrow interventions fits with the recent shift in development economics from the study of development as structural transformation to the study of ways to tackle symptoms of poverty (Chang 2012). In fact, the Sveriges Riksbank Prize committee specifically applauded Banerjee, Duflo and Kremer for addressing “smaller, more manageable questions.” What’s more, due to this narrow focus, the laureates’ work tends to be removed from analyses of power, wider social change, and the international economic structures that influence national development (Chernomas and Hudson 2019).

Some have responded to this critique of the laureates’ focus by pointing to the fact that RCTs have indeed been used for studying issues that are about more than just narrow interventions, such as labor markets and migration. While employing RCTs for the study of such larger questions is not the norm, it is worth engaging with this rebuttal because it appears to misunderstand the critique. An example is Blattman and Dercon’s (2018) study of industrial jobs and entrepreneurship in Ethiopia, which provides important insights into workers’ employment preferences. However, it cannot say anything substantial about strategies and constraints to structural transformation and industrialization. Indeed, such “big” structural questions would be impossible to study with randomized experiments, given that they deal with economies of scale, complex global dynamics, and issues that cannot be confined to a single unit (such as a household, village, or factory).

Perhaps the most striking example of such a big contemporary issue that cannot be studied with RCTs is China’s impressive structural transformation, which is the most successful case of poverty reduction of the past few decades. Given the inability of RCTs to deal with such issues, which are necessary for understanding how the conditions that create poverty can be challenged or reversed, many critics have questioned the extent to which RCTs will actually be able to significantly reduce global poverty (e.g. Ravallion 2012).

Critical theoretical engagement with the laureates’ work is also important, especially because they at times position their approach as atheoretical. For example, Duflo famously likened economists to plumbers. The plumber-analogy is misleading, as it suggests that economists’ work is purely technical, objective and value-neutral. However, all approaches in the social sciences are rooted in particular theoretical frameworks and worldviews, and the laureates’ work is firmly grounded in neoclassical microeconomic theory. This has implications for how experiments are designed and the underlying assumptions about individual and collective behavior. For example, in Duflo’s (2012) analysis of women's empowerment, all behavior is assumed to be explained as manifestations of individual maximizing behavior. Because of this, Duflo fails to understand a series of other important factors related to women’s empowerment, such as the role of sustained struggle by women’s organizations for rights.

The economics discipline generally has been fairly dismissive of the relevance of the work of historians and social anthropologists for understanding individual behavior (Bourdieu 2005). However, alternative methods and theoretical starting points can give useful understandings of human motivations. For example, Kabeer’s (2019) recent study finds that qualitative assessments were far superior to RCTs in explaining the outcome of asset transfer programs targeting women in extreme poverty in West Bengal and Sindh. She concludes that it is unlikely that RCTs alone can adequately explain the central role of human agency for project success.

In terms of methodology, social experiments have long been criticized for only measuring effects rather than studying the underlying causal mechanisms that lead to the effects (Heckman and Smith 1995). There have been efforts to try to get around this, for example by including several variations of an intervention in an experiment, as well as including mediators (as in Imai et al. 2012), but the understanding of causality provided by experiments are still limited due to their lack of attention to underlying social, economic and cultural structures that affect the results (Deaton and Cartwright 2018).

Since RCTs cannot fully explain what mechanisms are involved when something seems to work, they also do not tell us whether the policy in question can be reliably implemented elsewhere. In order to make such a judgement, a broader assessment of economic and social realities is unavoidable. This has led even critics within the mainstream to argue that there are misunderstandings about what RCTs are capable of accomplishing (e.g. Deaton and Cartwright 2018). Furthermore, while there have been recent efforts to improve external validity of RCTs (e.g. Meager 2019), these efforts have not been able to address RCTs’ limited ability to provide a broader understanding of the socio-economic realities in which the experiments take place. To make matters worse, the specific circumstances that are needed to even evaluate whether experiments may have external validity are often inadequately reported (Peters et al. 2018).

Why do these weaknesses in the laureates’ approach to poverty matter? As the Sveriges Riksbank Prize Committee stated, the “experimental research methods now entirely dominatedevelopment economics”. A serious epistemological problem arises when the definitions of what rigour and evidence mean in the discipline gets narrowed down to one single approach with so many theoretical and methodological limitations.

The discipline has not always been this way. The history of thought in development economics is rich and stretches back to Ibn Khaldun’s analysis of the importance of the division of labor for growth in the 14th century. Since then, many fundamental debates followed. Classical political economists debated the role of competition, money, and trade for growth; developmentalists debated the particularities of developing countries, such as dual sectors and lack of profitable investment opportunities; dependency theorists debated the specific constraints developing countries face that lead to uneven development, such as those related to balance of payments, technology, finance, and industry; and Marxists debated the legacies of colonialism and imperialism that led to the persistence of unequal exchange. These important, yet difficult, theoretical and empirical debates are still going on, but they are now mostly between heterodox economists and other social scientists - while the mainstream of the economics discipline is more preoccupied with debates about what kinds of interventions lead to poverty alleviation, within a neoclassical theoretical framework. The 2019 Prize is, therefore, in many ways, merely a symptom of the theoretical and methodological shift that has taken place in the mainstream of development economics since the 1970s.

Given the above-mentioned limitations to experimental approaches to poverty and development, it is important to challenge the Prize’s legitimization of a methodologically and theoretically prescriptive view of how to find solutions to global problems. To tackle global poverty, it is necessary to push for a field that takes a plurality of methodologies, theoretical frameworks, and forms of knowledge seriously. Furthermore, there is important research to be done on systems that produce poverty and inequality, but this requires systems-level thinking rather than measurement of impacts of targeted projects. Such systems-level thinking will require economists to try less to be like plumbers and more to be like the development economists of the past, who tackled structural questions and openly engaged with scholars from opposing theoretical camps.

For example, Raúl Prebisch (1950) exposed the structural and technological gaps across countries that lead to unequal terms of trade, thus reinforcing the gap between developing and developed countries (which UNCTAD 2016 recently found to still be empirically relevant). Prebisch pushed for a range of policy solutions to counter this tendency towards polarization, including a commodity fund and international cooperation to stabilize commodity prices - to support commodity exporting countries facing volatile and declining terms of trade. While the radical versions of his proposals were unsuccessful for political economy reasons, his research and policy proposals generated much debate across disciplines and institutions in the 1950-70s. The fact that such analysis was then marginalized in the 1980s is a dramatic loss to development economics, given that the global challenges we face require broad, pluralist and interdisciplinary debates about possible systemic solutions.

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