

Where are We Going?

Reflections on Social Science Research in the
2020s

John Harriss

SSER Monograph 22/1



Where are We Going?

Reflections on Social Science Research in the 2020s

John Harriss



Published by
Jesim Pais on behalf of
Society for Social and Economic Research
S3/209, Saraswati Towers, Sector D6, Vasant Kunj, New Delhi 110 070, India
E-mail: office@sser.in

©SSER, 2021

ISBN: 978-81-948800-2-8

The designations employed and the presentation of material in this information product do not imply the expression of any opinion whatsoever on the part of the Society for Social and Economic Research (SSER) or other institutions with which authors may be affiliated. The views expressed in this monograph are those of the author(s) and do not necessarily reflect the views or policies of SSER.

SSER encourages the use, reproduction and dissemination of material in this monograph. Except where otherwise indicated, material may be copied, downloaded and printed for private study, research and teaching purposes, or for use in non-commercial products or services, provided that authors and SSER are appropriately acknowledged, and SSER's endorsement of users' views, products or services is not implied in any way.

Where are We Going? Reflections on Social Science Research in the 2020s

I will devote a significant part of this paper to discussion of the state of the social sciences in the United States. I'll not apologise for this. It is simply because, by virtue of the sheer numbers of social scientists working in the United States, and the resources they dispose of, there is a tendency for them to define the development of our disciplines. Some years ago, it seemed to me that, in the United States especially, the social sciences tended to be rather polarised between, on the one hand the dominance of deductive rational choice theoretic research, allied very often with an emphasis on quantitative modeling, and on the other post-structuralist work focused on language and signification that appeared to me quite often to reflect relativism. This was not a split that made much sense in economics, and in economics the dominance of the mainstream, choice-theoretic approach appeared to be almost absolute, except in a few outposts like Amherst where there was space for heterodox economics. I found myself therefore somewhere in a no-man's land, not being persuaded by either of what I perceived to be the dominant tendencies in social science scholarship. The sort of work that made most sense to me — and still does — is based on historically, and that means contextually grounded research. I was not and I am not opposed to quantitative methods — that would simply be stupid — but I do worry about the kinds of simplifying assumptions that are made, necessarily, in a lot of quantitative modeling, and about the power that can then come to be exercised by numbers that may result from it. My sometime colleague the Norwegian economic historian Morten Jerven, for example, wrote a fine book about studies of economic growth in Africa entitled *Poor Numbers* (Jerven, 2013). In the book he shows, amongst much else, that there are several different data sets on economic growth in Africa that have been compiled thanks to the assiduous work of good scholars — but, unfortunately, there is no clear correlation between them. This means that one can tell different stories about the trends of growth and the possible explanations for them, depending on which data set is chosen. We may think of somewhat similar problems that have arisen in India, too, in circumstances in which the GDP growth figures have taken on such totemic significance.

There are further and stronger reasons for being concerned about the emphasis that is placed on mathematical refinement in research.¹ I think of the work of

This paper is based on the text of a talk that I gave at the request of Professor Vikas Rawal for the conclusion of a conference of young researchers from across the social sciences, organised by the Society for Social and Economic Research, and held on January 15-16, 2022. Professor Rawal suggested that it would be appropriate for me to talk about what I see as trends in social science research. I was foolish enough to agree, thinking that it would be interesting to step back from my own immediate concerns in research and writing, and to survey what is going on in different fields, in terms of the problems that are being investigated and the approaches that are being adopted. Of course, I took on an impossible task for the few days that I had in which to prepare the talk, and inevitably the paper reflects my own hobbyhorses of a good many years. For their comments on the text of the original talk I am grateful to Jesim Pais and Vikas Rawal of the Society for Social and Economic Research, and to Poulomi Chakrabarti, Jeff Checkel, Tamir Moustafa and Sanjay Ruparelia.

¹What follows here is taken in large part from an earlier paper of mine: Harriss (2009).

O'Connor (2001) on the history of *Poverty Knowledge* — the title of her book — in the United States. She shows that early work on poverty in the United States linked it with unemployment, low wages, labour exploitation, and political disenfranchisement - but research was quite soon turned away from these matters of political economy. The movement in research away from study of the social processes that make people poor came to be associated with the influence of research foundations and government agencies that have provided large amounts of funding for poverty research and have been able to set the agenda.² They have required that research be 'policy relevant', 'scientific' and free from ideology, but in all the research that they have financed poverty has only ever been defined as an *individual* condition. Poverty knowledge rests on an ethos of scientific neutrality, but it is very clearly distinguished from what it is not. As O'Conner writes:

Contemporary poverty knowledge does not define itself as an enquiry into the political economy and culture of ... capitalism; it is knowledge about the characteristics and behaviour, and the welfare status of the poor. Nor does it much countenance knowledge honed in direct action or everyday experience ...[which] ... kind of knowledge does not translate into measurable variables that are the common currency of 'objective', 'scientific' and hence authoritative poverty research (p. 4)

The problems of the poor in the United States have not been related to questions about employment and wage levels or the consequences of rising inequality, so much as with issues framed as 'family values'. The crux of the poverty problem has come to be located, therefore, in the characteristics of poor people.

Many of the same features, I believe, have characterised much of the research on poverty that has been carried on in India, too. There has been a strong focus on measurement, for understandable reasons perhaps, but in a way that has drawn attention away from the processes of accumulation in contemporary capitalism and so as to evade the problems of the distribution of resources and of political power. The emphasis on 'objective' and 'scientific' research on poverty, in India as well as in the US, tends to support the assumption that there are possibilities of technical, scientific solutions to the problem. The 'anti-politics' machinery of so much of the discourse of international development affects India too.

But as Anirudh Krishna's research on poverty dynamics in the country, based on fairly large samples of households over time, has so clearly demonstrated, large numbers of people experience big fluctuations in their economic circumstances, moving into and out of poverty.³ A set of studies, for instance, showed that 4 per cent more households moved into than out of poverty in the early years of this

²For discussion of how funding bodies can exercise a powerful influence on directions in research, see Moustafa (2022). This concerns the funding of research in political science by the National Science Foundation. In the context of an analysis of the implications of recent changes in the way the NSF is handling the discipline, Moustafa comments, 'Of course, the NSF had always catered to a particular vision of the discipline. The Political Science Program primarily funded large-N data-gathering exercises. Positivist and behavioral approaches were embraced, and normative work was discouraged. This orientation is so well understood that political theorists, scholars using qualitative or interpretive approaches, and others have long known that they should look elsewhere for research support.'

³Krishna (2017) is a synthesis of many years of research, intended for the general reader, but with 128 pages of detailed scholarly notes and references

century, and that more than 80 per cent of households that succeeded in ‘escaping’ from poverty remained stuck within the ‘zone of poverty’. These findings lead Krishna to make the point that ‘The repeated observation of pervasive poverty creation suggests that something is deeply wrong with [policies] that have a lot to say about raising people above the poverty line but contain nothing at all about preventing future poverty’. Am I being overly critical in thinking that the emphases in poverty research in the country — in which so much effort has been devoted to measurement - have lent force to an approach that has increasingly emphasised tangible welfare handouts, to lift people above the poverty line, rather than the much more difficult problems of effective investment in education and health care, which have been so woefully exposed by the coronavirus pandemic?

As I move on, I think it is worth quoting O’Connor’s arguments for the reconstruction of poverty knowledge, several of which have, I think, wider relevance. She suggests five steps:

1. Shifting from explanation of individual deprivation to explanation of inequalities in the distribution of power, wealth and opportunity [which means - my addition - examining societal processes and social relationships];
2. Recognising that studying poverty is not to be equated with ‘studying the poor’ [a point about which I feel most strongly]⁴;
3. Getting away from the research industry model [less of a problem in India, perhaps, historically — but becoming more significant];
4. Challenging the privilege attached to hypothesis-testing models of enquiry;
5. Recognising that the ideas of value-free social science and of finding scientific ‘cures’ for social problems are chimeras

The last two of these suggestions resonate with the arguments of what is by now quite an old book but still one that I find important. This is the book *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again*, by the Danish scholar Bent Flyvbjerg (2001). In a sense Flyvbjerg argues that we should reject the whole idea of the possibility of social science — if this means aiming to emulate the natural sciences in developing cumulative explanatory and predictive theory of universal application. The crux of the difficulty for social sciences, Flyvbjerg argues, is that human beings are ‘skilful’ – referring essentially to the ability that people have to make judgments, and to change their ways of thinking and behaving. Human skills go well beyond following rules; they are fundamentally context dependent. The kind of theory that is developed in ‘normal science’, on the other hand, depends on freedom from context and the existence of rules. The social sciences, however, have distinctive strengths in areas where the natural sciences are weak — precisely in dealing with reflexive analysis and discussion of values and interests. Such analysis is necessarily context-dependent; but recognising the centrality of context does not mean descending into relativism.

The sort of knowledge that is possible about people and societies is interpretive, and dialogical. In social-science-as-normal-science the key task is taken to be the

⁴Emphasising earlier observations about the influence of funding bodies on research, I recall — rather to my chagrin — how research organisations which I served in the UK in the 1990s devoted so much funding to research on poor people.

making of deductions and discovering of general principles across large samples, and detailed case-study research is often regarded as unproductive — as I found it was by some economists in both of the ‘Conversations Between Economists and Anthropologists’, organised by Pranab Bardhan, in which I participated (Bardhan, 1989; and Bardhan and Ray, 2008). If we recognise the context dependence of human action, however, then the kind of concrete, context-dependent knowledge that may be derived from careful case-study research is ‘more valuable’, Flyvbjerg argues, ‘than the vain search for predictive theories and universals’.

Interestingly, a very similar conclusion was reached at about the same time that Flyvbjerg’s book was published, twenty years ago, by two economists, Kenny and Williams (2001), one from the World Bank and the other from the University of Oxford, in a review of theory and of empirical research — using large data sets — on economic growth. They argued that ‘the social world is more causally complex than the natural world’ and that ‘events rarely, if ever, have a single cause, but are rather the result of a conjuncture of several factors or conditions.’ Thus, they argued, economic growth is subject to circular and cumulative causation, leading to path dependency. No two cases will ever be exactly comparable, and particular historical analysis of different cases is essential. The two economists concluded that ‘more energy should be directed toward understanding the complex and varied workings of actual economies rather than trying to assimilate them into abstract universal models.’ Kenny and Williams, and Flyvbjerg too, emphasise the importance of detailed study of process, and one development in the social sciences over the last decade has been to see the elaboration of the methodology of process tracing in qualitative research (Bennett and Checkel, 2014).

The concerns expressed by Alice O’Connor and Bent Flyvbjerg resonated very strongly in the world of American political science early in this century, when the so-called perestroika movement took off. It was asked, by someone who called him/herself ‘Perestroika’:

Why are all the articles [appearing in] the *American Political Science Review* [APSR] from the same methodology — statistics or game theory — with a ‘symbolic’ article in political theory ... where is political history, international history, political sociology, interpretive methodology, constructivism, area studies, critical theory and last but not least — post-modernism? (quoted by Rudolph, 2008)

It turned out that a fair number of political scientists shared this view, and a movement took off in the American Political Science Association that — amongst other changes - brought Susanne Rudolph, whose work on Indian politics, much of it done in collaboration with her husband, Lloyd, is well known, into the presidency of the Association. Both Susanne and Lloyd Rudolph wrote about perestroika, arguing passionately for pluralism in social science research. Susanne Rudolph carefully contrasted what she refers to as the ‘Scientific’ Mode of Inquiry with the ‘Interpretive’ Mode — the former with the characteristics of certainty, parsimony, cumulative knowledge, causality, singularity of truth, universal and objective knowledge. The Interpretive Mode, on the other hand is characterised by scepticism (rather than certainty), thick description (as opposed to parsimony), non-linear succession of paradigms rather than cumulative knowledge, concern with meaning, recognising the possible multiplicity of truth, and contextual knowledge. The different modes, both of which for Susanne Rudolph have a place in scholarship

— she wasn't arguing for one of them and for the relegation of the other — are well illustrated in two recent publications with which I am familiar and that have to do with rural India. I am thinking of the latest installment of findings from the study of the village of Palanpur in UP by Himanshu, Lanjouw, and Stern (2018), in their book entitled *How Lives Change*, and of Jan Breman's (2019) summation of a lifetime of research in Gujarat in his recent book *Capitalism, Inequality and Labour in India*. Both books report on studies carried out in the same locations for over half a century, and they record some very similar facts, such as the increased importance of migration, short and longer term, for work. But they offer very different understandings of the meaning of this fact. For Himanshu, Lanjouw and Stern it is indicative of entrepreneurial behaviour as people seek out new opportunities. For Breman such 'footloose labour', as he calls it, is the outcome of compulsion brought about by the way in which capitalism has developed in the rural economy. There is a sense in which both views might be right. It rather depends on what we understand entrepreneurship to mean.

Let me return to perestroika and the Rudolphs' plea for pluralism in research (letting 'a hundred flowers bloom', as Lloyd Rudolph argued for). What has actually happened in social science research in the United States — as I judge it from a necessarily hasty scanning of leading journals — has been to see a reversion to exactly the dominance of the one particular mode of inquiry against which the perestroika movement took off. A scan of the most recent two issues of two leading journals in political science, the APSR, and *World Politics*, suggests that there remains a high expectation that published work will be based on quantitative modeling— the focus of Perestroika's intervention. A high proportion of the 44 research articles published in the two issues of the APSR — the particular object of Perestroika's intervention — are based on some kind of quantitative modeling, most commonly one form or another of regression analysis (for instance, in sixteen of the 23 research articles published in November 2021), and some on experimental work. The papers generally follow a very similar format: statement of problem with reference to literature; theory; elaboration of hypotheses; data and research design; testing of hypotheses; regression results; explanation of findings. Among the 44 papers published in the two issues there are just five that do not involve quantitative modeling — two of them articles on political thought, two on moments of political history (each of them as it happens having to do with the politics of race), and one on Constitutional 'originalism' in the Republican Party. At 11 per cent of the content of the journal over the two issues they perhaps have a presence that is a little beyond the 'symbolic' of Perestroika's statement, but not much more than that. This, it seems, is in spite of the intentions of the new editorial team of the journal that began its term in June 2020, and which, in a statement (APSR, 2020) to readers later that year said, 'Our team is committed to making space for work that adopts approaches, epistemologies and methods that challenge dominant disciplinary norms and boundaries'. Perhaps publication in the latter half of 2021 was too soon for the team's policies to have been implemented, given the length of time that it takes for an article to find its way through the mill of reviewing and revision.

The story from *World Politics* is much the same. Ten research articles were published in the two most recent issues, nine of them being based on various quantitative methods, including in all but one case one or other form of regression

analysis. Just one article is a text reporting on findings from interviews, analysis of Chinese propaganda materials and from the work of Chinese scholars.

My impressions of trends in American political science, from the content of recent issues of these two leading journals, are counter-balanced by the qualities and significance of some of the work being done on Indian politics by a group of younger scholars. They include Tariq Thachil (Thachil, 2014; Auerbach and Thachil, 2018), whose research on how an elite party, the BJP, has been able to win over poor voters through the provision of social services, is based on mixed methods, embracing ethnographic observation as well as carefully designed quantitative work, and Adam Auerbach and Gabrielle Kruks-Wisner (Auerbach, 2016; Auerbach and Kruks-Wisner, 2020; Auerbach, 2019; Kruks-Wisner, 2018), whose research on citizenship and clientelism in, respectively, urban and rural Rajasthan, displays the same qualities. Their work, I believe, greatly illuminates understanding of the relationships of citizens and the state in India. As the references I have listed show these young scholars have published in the APSR and in *World Politics*, showing that the gates are not quite as closed as the contents of their recent issues appear to suggest.⁵

The situation is not radically different in sociology, from that of political science, if I can judge from the two most recent issues of the leading journal in the discipline, the *American Sociological Review* (ASR), and one of its peers, the *American Journal of Sociology* (AJS). The former, the ASR, published twelve research articles in the last two issues of 2021. Ten of them involved formal modeling, usually including regressions, one paper was based primarily on a small set of interviews, and just one paper was an exercise in Foucaultian analysis – the ‘symbolic’ article perhaps, from the other mode of inquiry. The nine articles published in the two most recent issues of the AJS included just three that were not based on quantitative modeling, one reporting on a twelve-year longitudinal interview study of 45 women, one drawing on qualitative arguments about the diffusion of ideas, and one in the great tradition of writing on contentious politics in historical sociology. Work in the vein of Tilly, Tarrow and McAdam still lives, it seems, but only just. Perestroika’s challenge to political scientists in the early 2000s would seem to apply very well indeed to American sociology today.

I have not commented thus far on economics. The sheer volume of work that is published in such a journal as the *American Economic Review*, often taken to be the most prestigious journal in the discipline, or the *Journal of Development Economics*, defies any sort of a quick analysis, certainly on the part of a non-economist. I understand from my reading, for instance, of economists who write for *Project Syndicate*, that ‘Although mainstream economics has moved past the market fundamentalism of the 1970s and 1980s, it has yet to establish a new theoretical footing. One reason is that well-meaning reformers in the field continue to embrace the same false premises that they should be seeking to overturn.’⁶ With the rise of behavioural economics, the dominance of the rational choice theoretic approach

⁵I should say, too, that another journal published by APSA, *Perspectives on Politics*, set up as a consequence of the Perestroika mobilisations, has aimed to encourage a wider range in political science scholarship. But I fear that the APSR still sets the gold standard.

⁶Project Syndicate (www.project-syndicate.org) is a not-for-profit media organization for which leading public intellectuals write commentaries. Indian contributors include Kaushik Basu, Brahma Chellaney, Jayati Ghosh, Raghuram Rajan and Arvind Subramanian. The text quoted leads an article by James K. Galbraith (2022).

may be less absolute than it was, and conventional ideas are being challenged by the development, for instance of Modern Monetary Theory, as well by changes in the world that have been brought into sharp focus by the pandemic. What we have come to call neo-liberalism is fighting a fierce rearguard action, though, and the economics profession is still largely controlled by mainstream economists. I'll say more on this in a moment. On the other hand, it is hard not to be impressed by the sheer range of topics that appear, for instance, in *the Journal of Economic Literature* or in the *Journal of Economic Perspectives*. In 2021 the latter hosted symposia, among others, on the impacts of COVID-19; on the Washington Consensus Revisited; on minimum wages; but also on polarisation in courts, and on preventive medicine. The former — the *JEL* — included articles on mass atrocities and their prevention, and on foreign influence on domestic policy, as well as papers on more conventional topics such as global public goods, household finance and on theory and evidence about capital controls. There were also two articles on economic inequality, one on China and one on 'Economic Inequality in Preindustrial Times'. It is only an impression, and maybe superficial, but it seems possible that economists are taking up bigger questions than are their peers in the other disciplines — and economists do by now have a long history of applying their methods on the terrain, in terms of subject matter, of other social science disciplines.

The range of topics taken up for research, however, in all the disciplines I have referred to is very wide, and only much more detailed analysis — it would be a major research project in itself - would enable one to reach conclusions about trends and major themes. I have been struck, however, by the absence of work, in the journals I have examined, of any research on matters of environmental degradation and climate change. Perhaps this is because such work is published in more specialist journals — and certainly the *Journal of Peasant Studies* continues to publish a good many articles on environmental matters, including work on struggles over natural resources, and latterly on agro-ecology. It also has an important series on aspects of authoritarian populism. This has led me to reflect on something that David Easton, the leading political scientist of his day, said in his Presidential Address to the American Political Science Association (APSA) in 1968, that rings out for me. Easton called for his profession to address the pressing issues of the era, such as race, poverty, and gender (so what's new?!), and he said:

Substance must precede technique. If one must be sacrificed for the other — and this need not always be so — it is more important to be relevant and meaningful for contemporary urgent social problems than to be sophisticated in the tools of investigation (cited by Rudolph, 2008)

The new editorial team of the American Political Science Review said something very similar, when it began its term on June 1, 2020 — the day on which Black Lives Matter protests took place all over the United States and across much of the rest of the world:

what motivated us as we began our work as editors, was our shared conviction that the questions political scientists need to ask include those that were on full display that first day in June. Political scientists need to study power, domination, ideology, political violence, and structural injustice ... [and] ... we reminded ourselves of why we

were doing this work: because of our commitment to political science research that helps answer the questions that were so vivid that day. We believe that political science risks becoming irrelevant if it cannot help answer these critically important questions. And we worry that all too often our discipline operates with an overly narrow view of what counts as political science.

In the same vein, at least one President of the American Economic Association (R A Gordon in 1975) argued that ‘the mainstream of economic theory sacrifices far too much relevance in its insistent pursuit of ever greater rigour.’⁷

I fear that these arguments are often denied, and that in our different disciplines we frequently are judged more on the grounds of technique than of substance. I have referred earlier to the difficulties that the editorial team of the APSR seems to have been experiencing in implementing the credo that it set out and from which I have quoted above. I fear that we may be driven, effectively, to take up problems that emerge from within our disciplines rather than questions that matter in our societies, in a kind of introverted navel gazing. Hard for young scholars to get a job or a grant or to get their work published if they do not adhere strictly to the contemporary conventions of their disciplines. I know one eminent political scientist in an Ivy League university who says very frankly that he wouldn’t stand a chance of getting a job these days because he has never done work, nor had any interest in doing the kind of work that conforms with the standard format of the great majority of articles published in the leading journals in the discipline — the one that I have described.

This leads me to want to say something about the idea of a ‘discipline.’⁸ The word derives, of course, from the Latin word for a ‘disciple’, and it came to be used at an early stage to refer to ‘the training of scholars and subordinates (disciples in other words) to proper conduct and action by instructing and exercising them in the same’. It also has the meaning of ‘a system of rules for conduct’, and – as some of us may remember from school days - it has the sense, too, of ‘correction’ or ‘chastisement’, intended to maintain conformity with the rules for conduct. All of this describes an academic discipline — a ‘department of knowledge’ -pretty well. When we speak of an academic discipline, we refer to a particular subject matter but also to a system of rules, reproduced through training, for defining this subject matter and the ways in which it is to be studied. And we are ‘disciplined’ – perhaps by not getting our articles published, or not getting a job, or a grant, or promotion, if we don’t respect the rules and conventions which define our discipline. ‘Discipline’ is productive. It establishes conditions for building knowledge. But equally clearly, it is constraining, limiting thought, and at worst academic disciplines, like other kinds of sects, may come to be characterised by a kind of religiosity, when particular practices and ways of working — usually those defined by the senior exponents in our fields, mostly old men - come to be venerated, treated as being beyond question, and others treated as quite unacceptable. Minds become closed to different perspectives. These arguments are reflected very clearly in the Cambridge professor Diane Coyle’s recent book *Cogs and Monsters: what economics is and what it should be* (Coyle, 2021). James Galbraith (2022) in his discussion of the book argues, ‘the discipline

⁷R A Gordon (1975) cited by Lazonick (1991, p. 1).

⁸Here I repeat some of the arguments I put forward in an old paper (Harriss, 2002) that I believe remains as relevant today as it was when it was written.

is still exceptionally disciplined. Academic success demands publication in one of only five “top” journals, all of which are tightly controlled by acolytes of the mainstream orthodoxy’. I’m struck by one remark made in Galbraith’s discussion. As will surprise few readers of this paper, ‘there is ample evidence that what is really good for jobs is union-driven wage solidarity, as practiced over the years in Scandinavia [and some other countries]’. But, says Galbraith, ‘This fact has eluded mainstream economics and will continue to do so, because articles advancing such insights cannot get published in the “top five” journals’. Minds are closed to different perspectives.⁹

I believe that good scholarship requires both ‘discipline’ and if you will ‘anti-discipline’ — a healthy disrespect for particular systems of rules when they stand in the way of the pursuit of knowledge about problems that matter in society, substituting for it mere ‘drill’, or the reproduction of conventional ways of thinking. One way of maintaining the tension between discipline and anti-discipline is through cross-disciplinary research, when the approaches and perspectives of different disciplines are brought into engagement. Working across disciplines is important, too, because so many of the questions that matter in our world don’t fit neatly into the subject matter of one discipline. Research on the boundaries of law and economics, for instance, is extremely important, and - closer to my own research interests over a good many years - it has been healthy that scholars from different disciplines should have become interested in the institutional environment that conditions economic behaviour. What, for example, are conditions for the development of trust that facilitates much economic activity? In this vein I was pleased to find an article (Purayil and Thakur, 2021) in a recent issue of the *Sociological Bulletin* discussing the relative dearth of work in economic sociology by Indian scholars and starting to suggest a research agenda. The authors argue for:

An alliance between sociology and economics within the broad framework of political economy. At the same time, it would entail recalibration of the conceptual space of the ‘economic’ in a way that it goes beyond its earlier focus on production and exchange to incorporate consumption ... [It means loosening] the compartmentalisations between studies in the tradition of political economy and that of cultural sociology not only to contain the earlier fragmentation of the ‘economic’ as an epistemological category but also to frontally situate it in a way that builds on the critique of positivism and economic reductionism [p. 325]

In this connection I should make the point that, in studies of economic institutions, the explanations of the origin of institutions that have been arrived at in much of the literature of the ‘new institutional economics’ (NIE) — which works within the frame of mainstream economics — are functionalist and use the following logic: ‘This institution exists because it serves to reduce transactions costs and to facilitate cooperation. It was because of the need to reduce costs

⁹The members of the new editorial team of the APSR make this point, too: ‘Our discipline does not shy away from signaling its norms and expectations about what does and does not count as a valid research question and about which methods and approaches are and are not legitimate. As political scientists, we like to tell ourselves that our data and methods are cutting-edge. But all too often, we let our data and methods dictate the questions that we ask.

(etc) that these institutional arrangements were established' (Toye, 1995). A more sophisticated version of the same reasoning holds that an institution exists because it serves the interests of a powerful group of actors. But the work of a historical institutionalist such as Kathleen Thelen shows that this reasoning may be simplistic, and in studies of skill-training in Germany late in the 19th century she demonstrates that institutions designed to serve one set of interests often become 'carriers' of others as well — institutions inevitably have unintended, as well as intended effects. They are not necessarily 'reflections of the interests of the powerful' Thelen (2004). Essentially, historical institutionalists, like Thelen, hold that 'unless something is known about the context, broad assumptions about "self-interested behaviour" are empty' (Thelen and Steinmo, 1992; and later work, Thelen and Mahoney, 2009).

So, summing up, I want to emphasise the importance of pluralism. 'Discipline' in research is productive. Without it we cannot distinguish knowledge from opinion and are left floundering in a sea of relativism. But equally it is extremely important that academic disciplines — or the rules that dominate in any of them at a particular time — are subjected to critical scrutiny from other approaches. These often come from other disciplines. It has, for example, been immensely important for the economics discipline that historians and political economists studying the development of institutions should have raised questions about the dynamics of the economy and how constraints are changed. A focus 'on the optimal allocation of resources doesn't readily provide answers to dynamic questions such as that of how productive resources are actually developed' (Harriss, 2002, p. 492). Deductive theory, certainly, needs to be confronted with historical 'reality' — in the way that arguments taken from economic theory to explain the rise of the East Asian 'Tigers', years ago, were confronted by the careful studies of the political economy of Korea, by Alice Amsden (1992), and of Taiwan by Robert Wade (2004), which in their different ways supplied answers to the question of how productive resources may be developed. The same is true the other way round, as well. The Nobel prize winner Elinor Ostrom's (1990) classic work on common property regimes shows the value in bringing the logic of deductive theory into conjunction with ethnographic and historical observation of particular cases. But my final word must be to emphasise, as I see it, the value very often, of detailed historical analysis, perhaps involving some kind of process tracing, taking very fully into account context dependence.

References

- Amsden, Alice (1992), *Asia's Next Giant: South Korea and Late Industrialization*, Oxford University Press, New York.
- APSR (2020), "Notes from the Editors", *American Political Science Review*, 114 (4), pp. v–vii.
- Auerbach, Adam Michael (2016), "Clients and Communities", *World Politics*, 68 (1), Jan., pp. 111–148, URL: <http://dx.doi.org/10.1017/s0043887115000313>.
- (2019), *Demanding Development: The Politics of Public Goods Provision in India's Urban Slums*, Cambridge Studies in Comparative Politics, Cambridge University Press, New York.
- Auerbach, Adam Michael, and Kruks-Wisner, Gabrielle (2020), "The Geography of Citizenship Practice: How the Poor Engage the State in Rural and Urban India", *Perspectives on Politics*, 18 (4), Mar., pp. 1118–1134, URL: <http://dx.doi.org/10.1017/s1537592720000043>.

- Auerbach, Adam Michael, and Thachil, Tariq (2018), “How Clients Select Brokers: Competition and Choice in India’s Slums”, *American Political Science Review*, 112 (4), July, pp. 775–791, URL: <http://dx.doi.org/10.1017/s000305541800028x>.
- Bardhan, Pranab (eds.) (1989), *Conversations between Economists and Anthropologists: Methodological Issues in Measuring Economic Change in Rural India*, Oxford University Press, Delhi.
- Bardhan, Pranab, and Ray, Isha (eds.) (2008), *The Contested Commons: Conversations between Economists and Anthropologists*, Wiley-Blackwell, Oxford.
- Bennett, Andrew, and Checkel, Jeffrey T (eds.) (2014), *Process Tracing: From Metaphor to Analytical Tool*, Cambridge University Press, Cambridge, URL: <http://dx.doi.org/10.1017/cbo9781139858472.001>.
- Breman, Jan (2019), *Capitalism, Inequality and Labour in India*, Cambridge University Press, Cambridge.
- Coyle, Diane (2021), *Cogs and Monsters: What Economics Is, and What It Should Be*, Princeton University Press, NJ.
- Flyvbjerg, Bent (2001), “Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again”, URL: <http://dx.doi.org/10.1017/cbo9780511810503>.
- Galbraith, James K (2022), “What’s Left of Cambridge Economics?”, *Project Syndicate*, Jan., URL: <https://www.project-syndicate.org/onpoint/whats-wrong-with-economics-by-james-k-galbraith-2022-01>.
- Harriss, John (2002), “The Case for Cross-Disciplinary Approaches in International Development”, *World Development*, 30 (3), Mar., pp. 487–496, URL: [http://dx.doi.org/10.1016/s0305-750x\(01\)00115-2](http://dx.doi.org/10.1016/s0305-750x(01)00115-2).
- (2009), “Bringing Politics Back into Poverty Analysis: Why Understanding of Social Relations Matters More for Policy on Chronic Poverty than Measurement”, in Addison, Tony, Hulme, David, and Kanbur, Ravi, (eds.), Oxford University Press, Oxford and New York, Jan., pp. 205–224.
- Himanshu, Lanjouw, Peter, and Stern, Nicholas (2018), *How Lives Change: Palanpur, India and Development Economics*, Oxford University Press, Oxford.
- Jerven, Morten (2013), *Poor Numbers: How We Are Misled by African Development Statistics and What to Do About It*, Cornell University Press, Ithaca.
- Kenny, Charles, and Williams, David (2001), “What Do We Know About Economic Growth? Or, Why Don’t We Know Very Much?”, *World Development*, 29 (1), Jan., pp. 1–22, URL: [http://dx.doi.org/10.1016/s0305-750x\(00\)00088-7](http://dx.doi.org/10.1016/s0305-750x(00)00088-7).
- Krishna, Anirudh (2017), “The Broken Ladder: The Paradox and the Potential of India’s One Billion”, URL: <http://dx.doi.org/10.1017/9781108235457>.
- Kruks-Wisner, G (2018), *Claiming the State: Active Citizenship and Social Welfare in Rural India*, Cambridge University Press, New York.
- Lazonick, William (1991), *Business Organization and the Myth of the Market Economy*, Cambridge University Press, Cambridge.
- Moustafa, Tamir (2022), “Political Science at the NSF: The Politics of Knowledge Production”, *PS: Political Science & Politics*, pp. 1–6, URL: <https://doi.org/10.1017/S1049096521001487>.
- O’Connor, Alice (2001), *Poverty Knowledge: Social Science, Social Policy and the Poor in Twentieth Century US History*.

- Ostrom, Elinor (1990), *Governing the Commons: The Evolution of Institutions for Collective Action*, Cambridge University Press, Cambridge.
- Purayil, Mufsin Puthan, and Thakur, Manish (2021), “The “Economic” in Indian Sociology: Genealogies, Disjunctions and Agenda”, *Sociological Bulletin*, May, pp. 003802292110146, URL: <http://dx.doi.org/10.1177/00380229211014667>.
- Rudolph, Susanne (2008), “Perestroika and its Other”, in Rudolph, Lloyd I, and Rudolph, Susanne Hoerber, (eds.), *Explaining Indian Democracy: A Fifty Year Perspective 1956-2008*, Volume 1, The Realm of Ideas: Inquiry and Theory, Oxford University Press, Delhi.
- Thachil, Tariq (2014), *Elite Parties, Poor Voters: How Social Services Win Votes in India*, Cambridge University Press, New York.
- Thelen, Kathleen (2004), *How Institutions Evolve: The Political Economy of Skills in Germany, Britain, the United States, and Japan*, Cambridge University Press, Cambridge and New York.
- Thelen, Kathleen, and Mahoney, James (2009), *Explaining Institutional Change*, Cambridge University Press, New York.
- Thelen, Kathleen, and Steinmo, Sven (1992), “Historical institutionalism in Comparative Politics”, in Steinmo, Thelen, and Longstreth, Frank, (eds.), *Historical Institutionalism in Comparative Analysis*, Cambridge University Press, Cambridge and New York, pp. 1–32.
- Toye, John (1995), “The New Institutional Economics and Its Implications for Development Theory”, in Harriss, John, Hunter, Janet, and Lewis, Colin, (eds.), *The New Institutional Economics and Third World Development*, Routledge, London.
- Wade, Robert (2004), *Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization*, Director’s signature series, Princeton University Press, NJ.

Considering trends in political science, sociology, and economics, in the United States especially — because of the sheer numbers of social scientists in that country, and the resources they have available — but with some comparative reference to work going on in India, the paper shows the continuing dominance of quantitative modeling, and the influence of rational choice theoretics. Gatekeepers, those in senior positions in leading departments and journals, clearly ‘discipline’ their fields according to the standards of these approaches. Recurrent pleas for greater pluralism, and for addressing critical social issues – such as those recently expressed by a new editorial team of the *American Political Science Review* — have evidently been ineffectual. But with reference to ‘poverty knowledge’ in the United States and India, and to cross-disciplinary research regarding economic institutions, the paper points to the strengths of approaches that emphasise the study of social processes, historically and with respect to context dependency. Such studies can be no less rigorous than quantitative modeling.

John Harriss (jharriss@sfu.ca) is Emeritus Professor, School of International Studies, Simon Fraser University, Vancouver.

Recommended citation

Harriss, John (2022), “Where are We Going? Reflections on Social Science Research in the 2020s”, SSER Monograph 22/1, Society for Social and Economic Research, New Delhi (available at: <http://archive.indianstatistics.org/sserwp/sserwp2201.pdf>).



The Society for Social and Economic Research (SSER) is a charitable trust based in New Delhi, India. SSER undertakes research on a wide range of issues related to social and economic development. These include issues related to food security and nutrition, agricultural development and rural livelihoods, industrial development, employment and labour relations, discrimination and exclusion, and living conditions in rural and urban areas.