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INTERNATIONAL DEVELOPMENT*

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THE CASE FOR CROSS-DISCIPLINARY APPROACHES IN INTERNATIONAL DEVELOPMENTⁱ

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Introduction: 'Saving Disciplines from Themselves'

The English word 'discipline' derives from the Latin 'discipulus', which means 'disciple', and it was used at an early stage in the development of the language 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" (OED).

'Discipline' has the meaning, too, of "a system of rules for conduct", as well as of "the order maintained among persons under control or command" or "a trained condition"; and, relatedly, it has the further sense of 'correction' or 'chastisement', intended (clearly) to maintain the 'order' and 'proper conduct and action' that are intrinsic to what 'discipline' is understood to be.

It is helpful, I believe, to reflect upon these meanings of the term 'discipline' when we come to consider its use, also, in the academy to refer to a 'branch of instruction' or a 'department of knowledge'. When we speak of an academic 'discipline' we imply not just particular subject-matter but also the existence of 'a system of rules' – reproduced through training – for defining that subject-matter and the ways in which it is to be studied. This creates conditions that make for the cumulation of knowledge partly by establishing the basis for differentiating between 'knowledge' and 'opinion'. Those 'disciples' who do not conform to the 'system of rules' of the discipline are subject to various forms of 'correction' and 'chastisement', even if in only in forms such as failing their degrees or failing to get their work published. Of course sets of 'rules for conduct' are subject to negotiation and to change, but at any one time they will usually be well defined, principally by the more senior exponents of the discipline, or those in positions of control or command, who thus 'maintain order'ⁱⁱ.

'Discipline' is productive. In the academy it produces the conditions for cumulation of knowledge and deepening of understanding of the physical and social worlds. But it is also clear that 'discipline' is constraining and that it may be pushed to the point where it limits thought (and so becomes constraining and even repressive rather than productive). We are probably also aware that academic disciplines, like other kinds of sects, may be characterised by 'religiosity', when particular practices or ways of acting come to be venerated in themselves, and others treated as quite unacceptable for no other reason than that they do not conform to the currently accepted canon – or fashion.

These reflections on what we mean when we speak of academic discipline(s) suggest that the development of knowledge and understanding requires *both* 'discipline' in the key sense of 'instruction and exercise' that inculcates the system of rules, *and* a healthy disrespect for particular systems of rules when they stand in the way of the pursuit of knowledge, substituting for it mere 'drill'. Good scholarship must involve a

tension between ‘discipline’ and ‘anti-discipline’, therefore. Our starting point is that one of the most fruitful ways of maintaining this tension is through deep immersion in a discipline, combined with the constant subjecting of knowledge deriving from the discipline to that developed in others. In a sense academic disciplines are saved from themselves by cross-disciplinary work, whether through multi-disciplinarity, when arguments from within different disciplines are set side-by-side, or through more rigorous inter-disciplinary exercises that attempt to integrate the theoretical and methodological frameworks of different disciplines. It is precisely these qualities that have distinguished many winners of the Nobel prize for Economics, according to their citations. That for Myrdal and for Hayek, who were the joint winners in 1974, for example, said that they “carried out important interdisciplinary work ... studied possible changes in the organisational, institutional and legal conditions prevailing in our societies ... (and that they) have in common (an) ability to find new and original ways of posing questions ... a characteristic that often makes them somewhat controversial [presumably, it may be deduced, because they challenged conventions of the discipline]”. Of Amartya Sen in 1998 it was said that “By combining tools from economics and philosophy, he has restored an ethical dimension to the discussion of vital economic problems”ⁱⁱⁱ.

Specifically, we are concerned with the dominant position of the discipline of economics in studies of international development, and within economics of methodologically individualist, choice-based economic theory. We do not contest the power of this particular ‘set of rules’, and we recognise that in terms of rigour and of parsimony it is exemplary within the social sciences. But we believe that it is mistaken to assume that because of these qualities work in other disciplines is only a kind of a non-essential luxury to be afforded in the universities of rich countries; that it is mistaken to believe that the application of the same set of rules provides the most satisfactory explanations of political and other non-economic aspects of human action; and that it is a mistake, too, to de-emphasise the contributions of other approaches within economics itself. Here we are inclined to agree with a former president of the American Economic Association who said in his presidential address that “[this] mainstream of economic theory sacrifices far too much relevance in its insistent pursuit of ever greater rigour”^{iv}; and to wish, with him, to see stronger efforts to integrate the building of theory in economics with the study of reality. In general our aim is to make the case for cross-disciplinary work on international development, whilst respecting the importance of contributions from within individual disciplines.

We also wish to question a commonly accepted set of oppositions, taken to be aligned in parallel:

Economics	:	Social Sciences
Quantitative	:	Qualitative
‘Macro’	:	‘Micro’
‘Hard’/’Scientific’	:	‘Soft’/’Journalism’
Sampling	:	Case Study

Our point is that the economics discipline does not have a monopoly on quantitative work, nor of ‘macro’ studies nor of ‘hardness’ or rigour. Equally economics gains, for example, from qualitative or historical research (on the social relations in which economic transactions are embedded, for instance); and economics has its own kind

of softness, as for example when analytical rigour involves such over-simplification as to misrepresent reality, or (even more crassly) when analysis is allowed to be dictated by the availability of data sets.

‘Qualitative hardness’: the anthropological contribution to studies of development

The discipline of anthropology is centrally concerned with people’s understandings of the world in which they live. Other people’s ideas or understandings are obviously not easily apprehended and can only ever be interpreted. Yet anthropologists have developed exacting standards and subject the inferences derived from observation of what people do, and listening to what they say in different contexts, to rigorous scrutiny. As Clifford Geertz - perhaps the most distinguished of contemporary anthropologists - has said, in survey research people are treated as ‘respondents’ or as ‘data items’, whereas when anthropologists do ethnographic research they aim to build up an holistic understanding of those they study as social actors, in their relationships with others, both ‘emically’ and ‘etically’^v. In doing ethnographic research the possibility of making statistically exact statements about a population is sacrificed in favour of understanding of social (including ‘economic’ or ‘political’) action, partly achieved through the possibility of making connections between family and kinship relations, neighbourhood relations, economic and political roles, and people’s beliefs and ritual practices. Understanding how other people think and the implications of their ideas and beliefs is only really possible through ethnographic research. The sorts of answers that people give to survey questions about attitudes and values may be interesting but there is very often a lot of doubt as to how ‘respondents’ have understood the questions which are posed to them, and how their answers are influenced by the context in which they are interviewed. It is not of course that ethnography is free from these sorts of problems either, but studying people ethnographically means that we have a much better chance of ‘triangulating’. For example the anthropologist/ethnographer may have the chance both of hearing how someone responds to a direct question about a subject, and then how s/he talks about it with others in an informal setting. So the point of studying any social phenomenon ethnographically is to understand action (what people do) and the ideas and beliefs that shape it. But it is important also that the ethnographer contextualises her observations. Even some of the greatest ethnography is not free from the criticism that the ethnographer ignored the wider historical context of his observations. Evans-Pritchard’s work on ‘The Nuer’ is a case in point for it ignores altogether the possibility that Nuer society at the time at which he lived amongst them was undergoing a tremendous amount of change as a result of the circumstances created by colonial rule.

Anthropologists have made some very notable contributions to the study of international development. An early milestone was the ‘discovery’ of the informal sector by Keith Hart (as a PhD student in anthropology at the time, not –as Mancur Olson seems to have believed – as ‘an economist working for the ILO’^{vi}) in fieldwork in Accra, or, more recently, the recognition of non-income-metric aspects of poverty (for example, by N S Jodha, an economist pursuing anthropological research in India: 1989). Thus Jodha showed how villagers in parts of India themselves think about

poverty, and how, in spite of evidence of the increase in income poverty over the period in which he studied several villages, according to the conceptions of the people themselves their livelihoods had improved, because they enjoyed greater security and greater autonomy than before. Of course Jodha did not demonstrate that all village people in India think in the same way, but his research did show up the limitations of the then-conventional way of conceptualising poverty. Robert Chambers has devoted himself over many years to developing the kinds of insights that derive from work like Jodha's, posing the question 'Whose Reality Counts?'^{vii}, and advocating the position that much more account must be taken in international development of the perspectives and understandings of poor people themselves.

A further marker of the contributions made by anthropology to development is in the study of common property institutions, where anthropological field research like that of Robert Wade on local management of irrigation in South India^{viii}, has shown that the solutions identified by Garrett Hardin to 'the tragedy of the commons' – broadly 'state' or 'market' – do not exhaust the possibilities. In this particular field of enquiry, too, there is an outstanding example of the value of efforts systematically to develop general theory from comparative case studies, in Elinor Ostrom's *Governing the Commons: the Evolution of Institutions for Collective Action* (1990)^{ix}. In this book a general theory is developed not on the basis of statistical generalisation but from detailed analysis of a number of 'thickly described' ethnographic and historical cases..

Study of 'people's understandings of the world in which they live' has also been turned quite forcefully against those involved in international development. Another anthropologist, James Ferguson, pointed out, for example, how the then prevailing economic theory of development led World Bank economists in the 1970s to define Lesotho – because of its definition as a 'less developed country' - as having a 'rural' and largely 'non-market' economy, in defiance of the established facts that the majority of households in the country depended upon incomes from wage labour in South Africa, and that Lesotho's cattle-keepers had long been selling cattle in South African markets^x. It was because of a similar kind of questioning of accepted ways of viewing reality from within the perspectives of the academic discipline of economics that Hart recognised that conventional understandings of 'employment' obscured the ways in which very many people gain their livings.

A more recent, outstanding example of 'qualitative hardness' and its contribution to international development is in the work of two other anthropologists, James Fairhead and Melissa Leach, who set out to study deforestation in Guinee^{xi}. Their discussions about land use with village people gave them to understand that, quite contrary to what colonial and post-colonial administrators and ecologists had maintained for over a century, the area under forest was actually greater now than in the past. This made good sense, because of the land use practices involved, which should indeed lead to the establishment of forest. But not content with this oral testimony they then checked historical records and, most tellingly, a series of air photographs from different times. Further, they examined the assumptions and the methods of the ecological science that supported the deforestation view of events in Guinee, and show how it has built in within it assumptions about biological processes that are being challenged in contemporary ecological research. Finally, their field research on the administration of development in Guinee shows up the possibility that forest department officials may have a vested, rent-seeking interest in the perpetuation of the idea that their

country is suffering from massive deforestation. This idea underpins investment in lots of forestry projects, from which, in different ways, forest officers benefit. Fairhead and Leach challenge the justification for much of this investment and argue that very scarce resources would be better spent on basic health care and in basic education. Of course they do not demonstrate that deforestation is always and everywhere a myth. But they do show, very convincingly, how the assumptions of observers influence interpretation of what is observed, without – and in this they are like Jodha, or Keith Hart – leaving us in a morass of relativism. Their's is a case study, but there is a sense in which it is far more rigorous than mathematically modelled analyses of the causes of deforestation (when this may not, after all, actually be occurring).

More generally, and without necessarily involving anthropological analysis of how people understand or construct the worlds in which they live, it is important to try to grasp 'where people are coming from' in their analyses, in order to comprehend disagreements and so to increase the possibility of meaningful dialogue. Ravi Kanbur has argued this recently, in regard to studies of poverty. Different groups of specialists talk past each other because they fail to recognise the different ways in which they are constituting the objects of their study, operating as they do at different levels of aggregation, or with different time horizons. He cites the example of the Ghana Living Standards Survey which showed that poverty fell a little between 1987 and 1991, a finding which – justifiably – provoked a furore amongst other specialists. 'Justifiably', because though the analysis was carried out in conformity with rigorous disciplinary standards, the method itself does not capture very well the value of public services, and because of the kind of aggregation that the survey involved. It is sometimes the case, of course, that a quantitative survey generates a misleading impression because of averaging out across diversity. According to different perspectives "Different parts of the same objective reality are seen and magnified", and the failure to recognise this fact leads to a lack of mutual comprehension amongst specialists^{xii}.

'Rigorous softness': studying politics through the methods of economics

There is no doubt that the application of the same 'set of rules' on which mainstream economics now rests, with its key assumptions of methodological individualism and rational choice, to other areas of social life than the economy, produces powerful results. For example, Mancur Olson's studies of collective action, showing why groups so often fail to act in their collective interest, and of 'the logic of power', defining the circumstances in which power-holders have an incentive to use their power to promote production and social cooperation, are quite fundamental contributions to understanding^{xiii}. But work of this kind does not render substantive, historical analysis of politics redundant.

The application of rational choice in political analysis can lead, as Mick Moore has argued, to a pessimistic 'interest group economism'. He cites work on the politics of targeting that is entirely deductive, applying the rational self-interest principle, and which concludes that the majority of people will not support income transfers to poor people because they will derive no benefit themselves, so that income transfer

programmes will survive the competitive business of politics only if they are not 'targeted'. As he shows the analysis rests on a number of simplifying assumptions:

- *all actors pursue their short-term self-interest
- *individuals aggregate into interest-groups that are exclusive in membership (i.e they are non-overlapping) and are constituted according to national-level class-like criteria (such as rich-poor, capital-labour)
- *policy is made by competing interest groups and there is no possibility that policy could be shaped by a leadership or government motivated by some encompassing, long-term appreciation of the general interest
- *there is a high level of information about the actual and potential uses of public resources, and the implications for individuals and groups
- *every policy decision is treated by the combatants as a unique event, so that they do not concern themselves about how their behaviour in a particular case might affect their chances of finding cooperative allies in other contests

Of course there is nothing intrinsically wrong about making such simplifying assumptions. But in this case their implications are such as to exaggerate the conflict and polarisation that redistribution is likely to generate, and the extent to which success in pursuing such a policy depends upon the mobilisation of large numbers of beneficiaries, whilst underestimating the extent to which elites may have an interest in redistribution and the autonomy that governments can enjoy to exercise leadership in favour of redistribution. As a matter of historical fact the deductive theory is falsified, for example in the experience of a number of Latin American countries, or the United Kingdom under its present government. Not least, history shows that there is a great deal of scope for political leaders to shape understandings of 'interests'. Yet it is possible, because the deductive theory is mathematically formulated and so appears to be extremely rigorous, that it will be accepted by policy-makers as being 'right' and thus be more influential than it deserves to be. But there is a 'softness' combined with mathematical rigour in this case, because of the mechanical reductionism involved in the assumptions that are made^{xiv}. Rather ironically, the public choice theorists have been ready to make assumptions that have long since been recognised as misleading by Marxist scholars. The latter have had to confront the limitations of conventional class theory and to acknowledge that actors' definitions of their 'self-interest' are not necessarily clear-cut and are rather rarely aggregated so as to form class-like interest groups. In the real world political identities are ambiguous and plastic and rarely grounded in any very simple way in economic interest, and the notion that politics is fundamentally like economics is consequently liable to be very misleading.

A similar set of concerns arises in connection with the burgeoning interest in 'social capital', seminally understood (by Robert Putnam) to mean 'features of social organization, such as networks, norms and trust, that facilitate co-ordination and co-operation for mutual benefit'^{xv}. There has been a strong tendency to reduce this apparently simple, but in fact rather slippery concept, to mean 'membership in local, voluntary associations'^{xvi}. Efforts have then been made to come up with measures of 'membership' as a proxy for 'social capital' and to investigate the relationship between social capital thus defined and various development outcomes. The results have been to encourage efforts to support the formation of voluntary associations in civil society. Yet both logic and empirical observation demonstrate that the significance of social capital, understood in this way, is entirely context-specific. It is

perfectly possible that poor people, say in inner-city areas, have a richer associational life than others, but that this social capital has much less use-value than the few associational connections of powerful individuals. Enrique Pantoja, who studied social capital in the coalfields of eastern India for the World Bank, shows this very clearly. He demonstrates, from survey research and ethnography in different village communities: (i) that ‘The value of a given form of social capital for enabling some action depends on the social and economic location of the social capital in a community’; and (ii) that ‘Access to social capital is differential while its use value is context dependent. Accordingly the value-added of social capital resources to community development can be positive or negative’^{xvii}. Deductive exercises taking no account of the historical and political context of particular societies will give rise to misleading results and, when translated into policy interventions may well be quite counter-productive, leading to the reinforcement of privilege.

We may note finally, in this connection, the struggle that has been going on within the American Political Science Association over the last year or so over the way in which the Association, and the discipline in many universities in the United States, have been taken over by rational choice theorists. Those distinguished professors of politics, like Lloyd and Susanne Rudolph of the University of Chicago, who have campaigned against this dominance readily acknowledge the strengths of the rational choice approach. Their’s is not some kind of would-be pogrom of those scholars who follow this approach. Rather their point is that the vitality of any academic field depends upon controversy and a diversity of approaches. It is not healthy for an academic field to be so ‘disciplined’ that alternative sets of assumptions – or ‘systems of rules’ - cannot be entertained.

‘Getting the social relations right’: studying the economy sociologically

The limitations of the methodologically individualist, rational choice theoretic ‘set of rules’ of mainstream economics, and the importance of the perspectives of other disciplines, have come also to be recognised within economics itself. Since the time (1975) that R A Gordon, as President of the American Economics Association, made the statement that I quoted earlier about the sacrifice of relevance to the pursuit of rigour, there has been a revival of interest amongst economists, exactly as Gordon wished that there should be, in “the changing institutional environment that conditions economic behaviour”^{xviii}. It is not only sociologists now who are interested in ‘getting the social relations right’ (in relation, that is, to desired development outcomes) and there is a widespread recognition, reflected nowhere more clearly than in recent World Development Reports, that institutions, and organizational forms, matter hugely. There continues to be debate, of course, about the extent to which they can be comprehended adequately – as they are sought to be by the new institutional economists - through the application of the same methodologically individualist, rational choice theoretic set of rules. But it is our contention that there are strong grounds for recognising the importance of substantive historical, sociological and political studies of institutions and organizations. Of course it is not only mainstream economics which needs to be confronted in this way: sociologists themselves long drew, for example, on the imagery of the protestant reformation promoted by Max Weber, long after historians had shown its lack of empirical foundation, and

economists on the one hand and anthropologists on the other, had successfully challenged the kind of cultural determinism to which it gave rise.

The economics discipline itself has moved on, with recent advances in the theorisation of economic growth, since 1991 when William Lazonick (a professor of economics) published his book *Business Organization and the Myth of the Market Economy*. But his book still illustrates the importance of confronting deductive theory with historical fact; and it has been as a consequence of the kind of questioning that Lazonick put forward that developments in economic theory have come about. His point was that the focus on the optimal allocation of resources in mainstream economic theory doesn't readily provide answers to dynamic questions such as that of how productive resources are actually developed. He contended that "neo-classical theory cannot analyse an innovative response – when the entrepreneur does not merely adapt to given technological, organizational and market constraints but, by investing in organization and technology, seeks to overcome constraints"^{xxix}. His argument was that "The superior development and utilization of productive resources (has increasingly required, historically) that business organizations have privileged access to productive resources. Inherent in such privileged access is the supersession of market coordination to some degree .."^{xxx}. (Robert Wade put forward a somewhat similar argument, at about the same time, with regard to industrial development in Taiwan^{xxxi}). Lazonick's answer to the key question of what the circumstances are that will encourage those who make strategic decisions in business organizations to overcome constraints rather than simply adapting rationally to them (as, it seems, British industrial managers were content to do by the end of the 19th century) is: "when they control an organizational structure that they believe provides them with the capability of developing productive resources that can overcome the constraints they face"^{xxxi}. Organizational capability matters. By the later part of the 19th century, for example, British firms lacked this capability, and: "In contrast to the small, vertically specialized proprietary firms that had characterised Britain's rise to economic dominance, US comparative advantage came from managerial enterprises that operated a number of geographically dispersed plants ... and that integrated a number of vertically related activities"^{xxxi}.

There are those who claim that methodologically individualist, rational choice theoretic reasoning can perfectly well explain the development of the kinds of business organizations that Lazonick describes, and he devotes a substantial part of his book to criticism of the arguments of transactions costs theorists and of 'new institutionalists' like Douglass North. He comments, for example, on North's relative lack of attention to business organization, and his narrow view of the basis of managerial control. Lazonick shares in the wider critique of neo-classical institutional economics, which has shown up its tendencies to tautological, functionalist reasoning and its emptiness as a theory of socio-economic change, in spite of the claims that have been made for it. In Douglass North's account of it NIE ends up by emphasising the constraints upon change: "once an economy is on an 'inefficient' path that produces stagnation it can persist (and historically has persisted because of the nature of path dependence" and because "the individuals and organizations with bargaining power as a result of the institutional framework have a crucial stake in perpetuating the system". Given its role in specifying and enforcing formal rules, the nature of the state is bound to play a central role in determining the path of development. So, for North,, "a dynamic model of economic change entails as an integral part of that model

analysis of the polity”. But it is not at all clear that the NIE actually has a theory of how and why polities differ. It offers no explanation of the fact that the same economic institutions can have very different consequences in distinct contexts. As Robert Bates has argued this shows “the necessity of embedding the new institutionalism within the study of politics”, for the reasons for the differences observed – for example between the outcomes of the establishment of coffee marketing boards in Kenya and Tanzania – have to do with the political context^{xxiv}. Ultimately this means studying institutions historically and so integrating theory building and the study of reality.

Conclusions

1. The contributions of disciplines other than economics to the understanding of development processes seems evident enough. I have spoken here of anthropology and of politics. The anthropological study of how people understand their worlds, including the ways in which specialists involved in international development themselves conceptualise the world in which they work (Ferguson, or latterly Peter Uvin, on the conceptualisations of humanitarian workers^{xxv}), has illuminated our comprehension of such vital matters as employment (Hart on the informal sector), poverty (Jodha and others on dimensions that are neglected by the income-metric approach), and environmental degradation (Fairhead and Leach on myths about the environment) . Only the substantive study of politics can show up, for example, what the specific identities are around which people mobilise, or how interests may be aggregated in practice by political leaders and coalitions built, say, in support of pro-poor policies. While the reduction of politics to the calculus of economic interests may stimulate basic hypotheses, it cannot substitute for substantive analysis.
2. ‘Rigour’ is not the exclusive preserve of economists or of quantitative research. Competent ethnographic research , for example, demands that researchers very carefully cross-check their interpretations of other people’s understandings of their own worlds, for example by comparing what people say in formal situations and in informal settings, and by checking the connections between what they say and what they actually do. And as the work of Fairhead and Leach shows, it is perfectly possible for rigorous mathematical modelling to be completely misplaced, because it starts out with false assumptions – such as assuming that deforestation is taking place when in fact what is going on is the reforestation of a landscape.
3. It also follows from these observations that different disciplines have different contributions to make and that it is very far from the case that all development research has to be in some way cross-disciplinary. Just as there is no good reason for supposing that anthropology has a particular contribution to make to analysis of exchange-rate policies, for instance, I have argued that the application of economics models to political analysis, or to the analysis of social capital, leads to doubtful results

4. But there is a much-to-be-desired tension between ‘discipline’ and ‘anti-discipline’. ‘Discipline’ in research is productive. Without it we cannot distinguish science or knowledge from opinion and are left floundering in a sea of relativism. But equally it is extremely important that academic disciplines, or the particular ‘sets of rules’ that predominate within any one of them at a particular time (like rational choice rules in American political science in the recent past), are subjected to critical scrutiny from other approaches, and these often come from other disciplines. Thus, as I argued in the last section, it has been immensely valuable for the economics discipline that economic historians studying the development of institutions should have raised questions about the dynamics of the economy and how constraints are changed. Actually the same is true of the study of politics (as the critics of rational choice predominance in the American Political Science Association have argued). Deductive theory needs to be confronted with historical ‘reality’. But the same is true the other way round. Elinor Ostrom’s 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.

The case for cross disciplinary work in studies of international development is a strong one, too, because research priorities should be set by the practical problems that development involves, more than by the puzzles that are generated out of theoretical speculation. Of course it is difficult, and there are well known dangers of sloppy superficiality. But these are outweighed by those posed by the ‘learned ignoramuses’ (or what Germans call, as I understand, *fach idioten*):

Previously men could be divided simply into the learned and the ignorant, those more or less the one, and those more or less the other. But your specialist cannot be brought in under either of these two categories. He is not learned, for he is formally ignorant of all that does not enter into his specialty, but neither is he ignorant, because he is ‘a scientist’ and ‘knows’ very well his tiny portion of the universe. We shall have to say he is a learned ignoramus, which is a very serious matter, as it implies that he is a person who is ignorant, not in the fashion of the ignorant man, but with all the petulance of one who is learned in his own special line (Ortega y Gasset 1932)^{xxvi}

In concluding with this pungent remark I do not mean to imply that any one academic discipline has a monopoly in this regard. The point, returning to my starting point, is that there is a sense in which ‘disciplines need to be saved from themselves’, as they can be through the encouragement of dialogue between them.

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ⁱ I use the term 'cross-disciplinary' in my title because I wish to discuss both multi-disciplinarity and interdisciplinarity in this presentation. I take it that the former means that different disciplines flourish side-by-side, and perhaps that perspectives from different disciplines are deliberately brought together, whereas the term 'inter-disciplinarity' refers to more rigorous attempts to integrate the frameworks of different disciplines and to explore research questions 'which would not otherwise arise within the boundaries of a single discipline' (Jackson, this volume). Interdisciplinary 'development studies', as an academic field, for some of its practitioners, at least, has the high ambition of attempting to restore the

holistic social science that was broken up with the emergence of different specialisms in the 19th century. As the noted anthropologist Eric Wolf has put it, there was a critical turning point around the middle of that century “when enquiry into the nature and varieties of humankind split into separate and (unequal) specialties and disciplines”. It was at this time that the “severance of social relations from the economic, political and ideological contexts in which they are embedded and which they activate was accompanied by the assignment of the economic and political aspects of human life to separate disciplines” (1982: 7-9). The development of distinct social science disciplines has made possible great advances in knowledge and understanding, but always at the risk of misleading simplification when social phenomena are treated out of context.

ⁱⁱ What I refer to in the text, figuratively, as ‘rules for conduct’ include both epistemological and methodological ‘rules’. There is, of course, a good deal of sharing of these across disciplines, but different academic fields are defined by distinctive sets of rules about epistemology and methodology, and subject matter, or – to use another figurative description – distinct ‘traditions’. These, like all traditions, are contested, and change over time.

ⁱⁱⁱ Many of the winners of the Nobel prize for Economics have been distinguished, according to the citations, by qualities similar to those noted for Myrdal, Hayek and Sen.

^{iv} R A Gordon, in 1975, cited by Lazonick, 1991, p 1

^v These terms refer to the possibility of understanding another empathetically (‘emic’) or by ‘external’ observation (‘etic’).

^{vi} Olson 2000, p. 178 Hart’s entry on the ‘informal sector’ in *The New Palgrave Dictionary of Economics* gives a short account of his ‘discovery’ of the idea.

^{vii} This is the title of Chambers’ most important book. See Chambers, 1997. His arguments about the conceptualisation of poverty, which draw from Jodha’s research, are found in Chambers 1992.

^{viii} Wade 1988

^{ix} One of the case studies on which Ostrom drew – modesty shall not prevent me from remarking - was my own research on irrigation in Sri Lanka.

^x Ferguson, 1990 chapter 2

^{xi} Fairhead and Leach 1995

^{xii} Kanbur 2000.

^{xiii} Olson 1965 and 2000.

^{xiv} Moore 1999. The paper that he critiques here is a Policy Research Working Paper of the World Bank.

^{xv} Putnam 1993 ‘The prosperous community: social capital and public life’, *The American Prospect*, no

^{xvi} This direction was set by World Bank specialists in a publication entitled ‘Social capital: the missing link?’ (in, World Bank 1997)

^{xvii} Enrique Panjoja 1999 ‘Exploring the Concept of Social Capital and its Relevance for Community-based Development. The Case of Coal Mining Areas in Orissa, India’, World Bank Social Capital Initiative, Working Paper No 18. See also World Bank Social Capital Initiative, Working Paper No 3

^{xviii} Gordon, 1975, cited by Lazonick, 1991, p.1

^{xix} Lazonick, 1991, p. 308

^{xx} Lazonick, 1991, p. 8

^{xxi} Wade 1990. This argument against the earlier view that the remarkable success of Taiwan and South Korea in bringing about rapid economic growth was due to their pursuit of ‘free market’ policies, has been stoutly resisted of course, but gradually won acceptance. See Wade’s account of the debates within the World Bank over its own report on ‘The East Asian Miracle’: Wade 1996

^{xxii} Lazonick 1991, p. 328.

^{xxiii} Lazonick, 1991, p.14.

^{xxiv} Quotations here are from the Introduction to the collection edited by Harriss, Hunter and Lewis, 1995, which includes essays by Douglass North and Robert Bates.

^{xxv} Uvin’s work for WIDER

^{xxvi} Quotation taken from a presentation made by James Putzel, 2001, the influence of which in other parts of this paper is also acknowledged.