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2

**'MICRO' VERSUS 'MACRO'
SOCIOLOGIES OF SCIENCE AND TECHNOLOGY**

Donald MacKenzie



UNIVERSITY OF EDINBURGH

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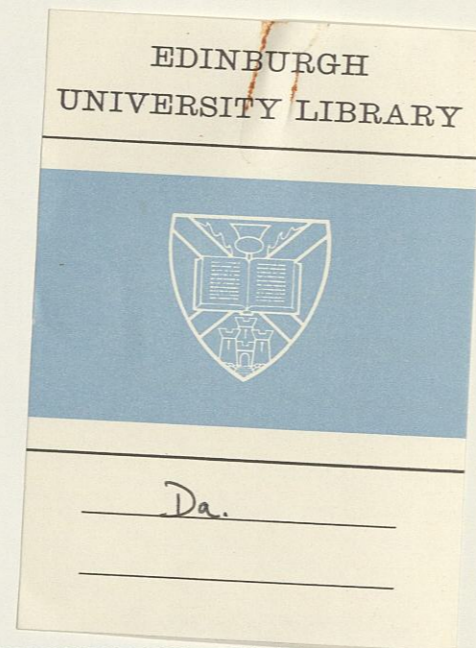
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'MICRO' VERSUS 'MACRO' SOCIOLOGIES OF SCIENCE AND TECHNOLOGY

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Micro sociology involves a focus on observed day-to-day face-to-face interaction; macro sociology involves relationships one might call 'historical' rather than day-to-day, and usually 'systemic'. This paper discusses the relationship between the two as perspectives on the sociology of science and technology.

It is argued that a shift has occurred in the way aspects of the development of science, technology and society. The shift, always present but increasingly so, has moved from the centre of 'technology' to the periphery, and thus from the centre of 'technology' to the periphery, is at the heart of what there is to be done. In a satisfactory approach to understanding this, there is both 'micro' and 'macro'.

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**'MICRO' VERSUS 'MACRO'
SOCIOLOGIES OF SCIENCE AND TECHNOLOGY**

Donald MacKenzie *

'Micro' sociology involves a focus on observable, day-to-day, face-to-face interaction; 'macro' sociology involves relationships one might call 'historical' rather than day-to-day, and spatially 'spread out'. This paper discusses the relationship between the two as perspectives on the sociology of science and technology.

It is argued that to see them as opposed is to miss a central aspect of the development of science, mathematics and technology. The shift, always potentially problematic, from the local (and thus from the terrain of 'microsociology') to the non-local is at the heart of what these are as human endeavours. So a satisfactory approach to understanding them must be both 'micro' and 'macro'.

Though traditional 'macrosociology' is typically defended as being more relevant politically than 'microsociology', the former is insufficiently puzzled by the phenomenon of structural power. The concerns of the latter have potentially great relevance to an understanding of power, for example in an investigation of how scientific or technical change is made 'available' for explicit management, by capitalists, democratic representatives, or whoever.

These abstract arguments are fleshed out by a discussion of one particular example: the construction of nuclear missile accuracy figures.

* An earlier version of this paper was read to the 1987 Conference of the British Sociological Association. It was revised as part of a discussion of theoretical frameworks taking place in the Edinburgh group of the Economic and Social Research Council's Programme on Information and Communication Technologies.

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Though traditional macro sociology is typically derided as being more relevant politically than micro sociology, the former is actually privileged by the phenomenon of structural power. The contents of the latter have potentially great relevance to an understanding of power for example in an investigation of how scientific or technical change is made 'available' for explicit management by capitalists, democratic representatives or whoever.

These abstract arguments are fleshed out by a discussion of one particular example: the construction of nuclear missile accuracy figures.

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Introduction

What do the terms 'micro' and 'macro' mean? Clearly, they are used quite differently by different people. Economists, say, have a distinctive usage. In the way the terms have informed recent debate around the sociology of science and technology, the intellectual tradition most strongly (if often implicitly) drawn on is the general one of post-1945 sociology. The most straightforward way to understand their usage here is as coding two different types of sociological practice: one focusing on observable, day-to-day, face-to-face interaction; the other studying, in a less directly observational sense, relationships one might call 'historical' rather than day-to-day, and spatially 'spread out'. Examples of the latter kind of relationships would be 'class' and 'the state'.

The coding of two different types of sociological practice has, of course, often been taken as corresponding to two different 'levels' of social life. Generally the 'macro' level (however it was conceived, which obviously differed greatly between different schools of sociology) was taken as at least partially determining the 'micro' level. The 'micro' was inside or beneath the 'macro', as it were. To the extent that it was concerned with the 'micro' at all, orthodox Marxism clearly took this view. So did structural functionalism, although the centrality of the 'family' and 'socialisation' to structural functionalism certainly made the 'micro' more significant.

Since the mid-1960s two challenges to the primacy of the 'macro' have emerged. One is from feminism. The notion that 'the personal is political' is a claim for the importance of the 'micro' - perhaps a claim for its role in determining the 'macro'. The other has been from ethnomethodology. The challenge here was to the independent existence of the 'macro'. Social structures were not 'out there', said ethnomethodology. They were outcomes, constructs - the product of 'interaction work' by actors.

As the originally fierce polemics between ethnomethodology and 'orthodox sociology' died down, and as feminism has won at least a grudging acceptance within sociology, the issue of the 'micro' and the 'macro' has somewhat slid

from view - though with nothing approaching a resolution.¹

It is not at all surprising that it should, however, reemerge within the sociology of scientific knowledge, for of all the fields of sociology that is probably the one where the influence of ethnomethodology has been greatest. (The influence of feminism, on the other hand, within what is counted as mainstream sociology of scientific knowledge, has been next to nil.²)

Locality

Why should the sociology of scientific knowledge be seen as 'micro-sociology'? In part, of course, it is simply a matter of research practices, notably the significance of the participant-observation 'lab ethnography'.³ But there is a deeper, intellectual, reason - it concerns the significance of the local (a better word here than 'micro', for reasons that I hope will appear later).

One way of reading the central claim of the sociology of scientific knowledge - one version of, for example, its hotly debated 'relativism' - is to say that there is nothing abstract that guarantees that an 'experimental result', a 'proof', a 'practice', an 'instrument' or a 'machine' will not remain a local achievement. There is no way of fashioning the procedures of experiment, deduction, testing, and so on, such that their results will be compelling in the abstract.

The studies of replication have approached this most directly. The attempt to replicate an experimental result is an attempt to check whether it is 'merely local' - a product of the particular experimenters, particular instruments, particular substance, particular day of the week, and the like. But because scientific practice cannot be reduced to a set of algorithmic instructions, there is no way of guaranteeing that the experiment done at place and time B is 'the same' as that done at place and time A. Nor, in the absence of agreement as to what the result of the experiment should be, is there a way of showing that what was done at site B was 'right' and what was done at site A was 'wrong' (or vice versa). So attempts at replication merely generate a series of local

¹Though see Knorr-Cetina and Cicourel (1981).

²See Delamont (1987).

³The classic is of course Latour and Woolgar (1986; first published in 1979).

practices and local knowledge claims, without an abstractly compelling way of differentiating the series.⁴

Delocalisation

But science, mathematics or technology are not only local achievements. Some work influenced by ethnomethodology - rightly noticing the local features of science - jumps quite mistakenly to this conclusion.⁵ However, to conclude that would be to miss the whole point. The success of science, mathematics and technology is constituted by the transcendence of the local. An experimental result is only treated as a 'fact' if it is accepted that it could be produced in Rio de Janeiro as well as Paris. A series of equations is only taken as a 'proof' if it is accepted that it seems compelling not just at 3.00 am but in the cold light of day. A 'test' of a technology is only taken as a valid test if the artefact was not 'gold plated' specially and if the circumstances were 'fair'. A technology is accepted as 'working' only if it is believed it will continue to do so outside its research and development laboratory. To remain local - in science, mathematics or technology - is to fail.

A consequence of this is that a 'microsociology' of science, mathematics or technology - in the sense of a practice and/or theory that focuses only on a study of observable, day-to-day, face-to-face interaction - is impossible. To the extent that it remains 'micro' it is not the study of science, mathematics or technology! It is a study of local achievements that might or might not become facts, proofs or machines. To understand whether or not they do, we have to move beyond the local.

But neither is a sociology of these subjects that neglects the local at all satisfactory. For it can then only be a study of finished science, mathematics and technology. Once local achievements cease to be local, the 'negotiation', 'construction' or 'social shaping' tends to vanish from sight - indeed, its vanishing from sight is the very process of ceasing to be local.

⁴The work of Harry Collins has been central as regards replication. See Collins (1985).

⁵See, for example, Livingston (1986), and the review of it by Bloor (1987).

Beyond the 'Micro' and the 'Macro'

It would be a bland and facile conclusion to say that both micro and macro factors must be considered, the dichotomy must be overcome, etc. Nor is it particularly helpful to focus on physical 'scale'. Thus in one piece of work I have done, the local site is a 4,500 mile missile test-range, employing over 2,000 people and raising issues of international law in its very existence (MacKenzie, 1989). Is looking at that doing 'micro' or 'macro' sociology? Rather, the reason any satisfactory sociology of science, mathematics or technology has to be both local and non-local, is that the shift from the local to the non-local (and sometimes back again) is at the heart of what these are as human endeavours.

Controversies

Hence the significance of controversy. It would clearly be utterly misleading to think that, say, technology is only 'socially shaped' when two alternatives were available and one was chosen. It is of course right to note that the most effective exercise of power is to prevent the very existence of alternatives (Lukes 1974).

No, the significance of controversy is that in controversy scientists, mathematicians and technologists struggle over locality. They seek to show that their opponents' claims are only local achievements. In the absence of controversy, the move from the local to the non-local tends to go smoothly. It can appear as the operation of 'method', 'logic', 'rationality', 'considerations of technical efficiency', and the like. Controversy opens it up to public view.

Controversy, then, is a mighty handy phenomenon that enables us to move from what had been produced to looking at the process of production.⁶ It enables us to 'leave the sphere, where everything takes place on the surface and in full view of everyone, and follow ... into the hidden abode of production, on whose threshold there hangs the notice "No admittance except

⁶This way of formulating the significance of controversy for the sociology of science and technology has been inspired - as has much else in this paper - by reading Latour (1987).

on business". Here we shall see, not only what effects of science and technology are, but how they are themselves produced...⁷ There are other ways of seeking to do this, of course. But in none of them is the problematic nature of the move from the local to the non-local so clearly highlighted.

Simplification

But what has all this to do with traditional 'macro' concerns for class, capitalism, the state etc? One simple answer is of course to say that one cannot understand what goes on at local sites like, say Tupolev's sharaga (scientific-technological prison institute), or an Alvey programme technology demonstrator project, or the Salk Institute, without attention to these 'macro' concerns.⁸ I agree.

But there is also at least one other issue involved. That concerns how any 'macro' institution is possible - whether that institution be a transnational corporation, a state, a class structure, or whatever. Any such institution involves delocalised power. The power of the capitalist over the worker, say, is evidently not a matter of psychological or physical superiority in face-to-face conflict.

Several strands of social theory are interested, in different ways, in this issue. Foucault is the most obvious example. Another is the versions of Marxism that refuse to take 'the state', or 'value', for example, as simply objectively existing entities but treat them as contingent, always potentially precarious, social achievements. A third has, interestingly, arisen from within the sociology of science: in recent work by Barry Barnes, on the one hand, and by Michel Callon, Bruno Latour and John Law, on the other.⁹

In varying degrees and with varying emphases, all three strands of thinking note connections (or, more strongly, the inseparability) of power, on the one hand, and knowledge and/or machinery on the other. Structural power,

⁷No prizes for guessing the author. The last sentence of the quote has of course been tampered with: see Marx (1976, 279-80).

⁸Ozerov (1973), Elder (forthcoming), Latour and Woolgar (1986).

⁹See, for example, Foucault (1979), London-Edinburgh Weekend Return Group (1980), Barnes (1983, 1988), Law (1986), Latour (1987).

science and technology all involve delocalisation; and it is surely not a coincidence that the societies in which structural, 'macro', power has become most developed are also the societies in which science and technology have been most developed.

The notion of simplification offers one handle on these issues. As Latour et al. most explicitly point out (but which is also implicit in the other abovementioned strands of thought), structural power is possibly only in situations where actors can treat large chunks of potentially infinitely complex and unpredictable behaviour in a radically simplified fashion - as stable, predictable, 'black boxed'.¹⁰

Though it is not one Latour et al. choose, capitalism offers a particularly clear example. In what Marx called 'the capital relation' all the complexities of subordinate/superordinate relationships in work processes are radically simplified into an (in principle) stable, predictable, self-reproducing exchange relationship - and one with a strong tendency to expand. Most importantly, so long as the simplifications hold (money is one crucial simplification), day-to-day physical coercion is unnecessary.¹¹

Simplification is also crucial where institutions take concrete organisational forms, such as state apparatuses or firms, particularly when we consider how those within them who possess formal power, those 'at the top', can possibly 'run them' and 'take decisions'. Clearly, again, it is by being able in practice to perform radical simplifications.¹²

There is a case here for re-examining some of the insights of organisational sociology and of the 'bureaucratic politics' school of political science. Herbert Simon's key notion (Simon 1955) of 'bounded rationality' - which informed the work of the most insightful writers in both approaches - is a statement of the unavoidability of simplification. Graham Allison's classic study of the

¹⁰See, for example, Law (1986) and Barnes (1988).

¹¹The 'labour process' approach to production technology, to be found in the work of Braverman, Noble, et al. is essentially about the shaping of technology to help maintain the capital relation in situations where aspects of 'simplification' could potentially break down. See Braverman (1974), Noble (1984).

¹²As Callon starkly puts it (with implicit reference to Lenin): 'one would be wrong to think it is more difficult to direct the state than to cook bread. The high official simplifies the society of which he has charge to such an extent that his job becomes child's play (Callon 1981, 57: my translation).

Cuban missile crisis - of a situation where 'any little thing', a 'routine' reconnaissance flight or change in the behaviour of military forces, could have frightening consequences - shows how precarious is the capacity of those at the top to 'keep things in order', even when no systematic internal challenge to them exists (Allison 1971).

Science, Technology and Simplification

Science and technology are of particular relevance here because they both involve simplifications and make possible simplifications. To say that they involve simplifications is just to recap the points about delocalisation. To say they make possible simplifications is to point to their role in making phenomena available for power, a role that one could easily delineate for fields as different as psychology, statistics, geophysics and information technology.

One of the many aspects of this analysis that is of empirical interest concerns the management of technical change. To what extent, and how, is technical change made available for management? To what extent, and how, is it simplified so that those at the top can take decisions about it (or can believe they are taking decisions about it)? This question is only at the most trivial level about the obvious issues of esoteric language, and the like. More fundamentally, how is it that an activity that is always potentially completely uncertain - the creation of new knowledge, new artefacts, and so on - is 'packaged' so that it is subject to 'sensible decisions'?

This is precisely the kind of topic where 'micro' and 'macro' concerns coincide. There would be no point in worrying about state or capitalist shaping of the research and development process - nor in calling for democratic control of it - were this simplification not possible: neither officials nor bureaucrats nor democratic bodies would be able to get hold of the process in order to control or shape it. Yet there is no way to understand it without tackling issues of the sort studied by so-called 'microsociology' of science and technology.

One example. The sort of advice one could imagine being given to a manager of technical change might be to identify 'technological trajectories' and plan on the basis of them - fund work that is along a known trajectory, prepare for the

consequences of the trajectory being followed, and so on.¹³ Yet technological trajectories do not exist - at least not in the simple sense that the mechanical metaphor of 'trajectory' suggests, or that some recent academic work using the concept has implied.¹⁴ It takes but a moment for an habitué of relativism to note that the trajectory is an actors' construct, that alternative paths of development are always potentially open, that technology is never self-directing in the way that the notion of 'trajectory' suggests.

But, though valid, that is a glib response unless we know how actors construct 'trajectories' and similar simplifications that are used in the management of technical change, and how these simplifications are (or are not) sustained in the face of the challenges to them that, abstractly, are always possible.

Although I do not recall seeing the notion in the literature on technological trajectories, it seems plausible that, at a first approximation, a technological trajectory has the form of a self-fulfilling prophecy:¹⁵ to the extent that it is believed to exist, sufficient interests are aligned to create it.¹⁶ Obviously, too, other judgements - such as that a line of work is 'technically unpromising' - also may have the form of self-fulfilling prophecies. Such judgements have a strong tendency to 'spread' in vicious or virtuous cycles. In my experience of interviewing technologists, I have often found a high level of interest in 'what other people are doing'. Nothing seems to make it harder to keep resources flowing than a sense that you are the only corporation doing work of a certain kind. Likewise the argument that others are actively pursuing the line of work you wish to pursue seems under many circumstances to count as a very strong

¹³On technological trajectories, see Dosi (1982).

¹⁴See, for example, Nelson and Winter (1982, 255-62). Without real explanation of the concept, they introduce the notion of 'natural trajectories' of technology. The adjective makes the metaphor worse, since 'natural' could easily be read as 'asocial'. Reflection on their examples - 'progressive exploitation of latent economies of scale and increasing mechanisation of operations that have been done by hand' (*ibid* 259) - quickly reveals how 'natural' trajectories are not at all 'natural' in this sense.

¹⁵The classic paper here is of course Merton (1948), now to be found in Merton (1968). A useful introduction to the very large literature on self-altering prophecies is Henshel (1982). Barnes - in an essay that nicely shows the relevance of the self-fulfilling prophecy to the overall issue discussed here - is however right to note that the reason why Merton's insight was not taken up more is that 'the prevalent contemporary view of social order treated social relations as independent entities, "there" independently of how they were described or acted toward. In conformity with this view, the self-fulfilling prophecy was identified as a pathological pattern of inference. The banks it destroyed [in Merton's original example] really were sound, and were brought down by mistaken inference' (Barnes 1983, 537).

¹⁶Another answer - probably one closer to the hearts of some of the proponents of the notion - is that a technological trajectory exists because 'nature' facilitates certain forms of technical change and inhibits others. It seems to me perfectly plausible that nature does this, but equally implausible that this is the entirety of the story.

case. It is taken as confirming that the work is 'technically promising'.

Though these ideas are enticing - they might even be subject to a degree of formal modelling, with slight modification to Brian Arthur's work (Arthur 1984; Arthur, Ermoliev and Kaniovski 1985) on modelling technological change as a path-dependent stochastic process - they are obviously at best only a small part of the answer. I would reiterate, however, that a satisfactory answer would surely have to fuse and transform the kind of concerns habitually divided up into 'micro' and 'macro'.

Politics

There are, of course, more than 'academic' issues involved in the 'micro'/'macro' divide. 'Macro' sociology of science sees itself as political (or, in its less radical forms, 'policy relevant'), and suspects 'micro' sociology of being unpolitical - or even, under that guise, covertly reactionary.

Part of this is, of course, a matter of what substantively one chooses to study: the bicycle or nuclear power, the Portuguese expansion or a weapons system. In part it may also be selective reading, as in the way the image of Latour's work in the Anglophone world has been dominated by Laboratory Life. (His Science in Action [1987] should, however, undermine any notion of his work as either 'micro' or 'unpolitical'.)

But rather than get stuck with a probably false and potentially damaging dichotomy, it is perhaps worth investigating what is implicitly political in an emphasis on the local, and on processes of delocalisation - if only on the minimal grounds that any political movement worth its salt should have its eyes open for unexpected opportunities and unexpected allies.¹⁷

If it is right that questions of locality and delocalisation have a great deal to do with power, then one answer seems fairly clear, at least in the abstract - that is

¹⁷Take a different example. There have, rightly, been a lot of calls for studies of science and technology as labour processes (a line of work, it is worth noting, which is logically quite distinct from the Braverman/Noble approach). Even though it emerges from a different tradition, should not Peter Galison's 'Bubble Chambers and the Experimental Workplace' (1985) be embraced, however critically, as such a study?

that power can be undermined by interrupting the processes of delocalisation, and that counter-power can be built up by creating different such processes. Understanding these processes, if this is correct, could be a most useful political resource.

Let me end with one example, the nuclear missile.¹⁸ A vital political issue about such missiles, it seems to me, is to prevent those at the top being able to 'simplify' them - that is to say to treat a missile as a reliable 'black box' that on receipt of the necessary input (the order to fire) will predictably produce the intended output. In the light of theories of 'fighting and winning' a nuclear way, the most relevant intended output is the destruction of an 'enemy' missile silo or command post.

People in the peace movement often contribute (of course, only in a small way and unwittingly) to this simplification by quoting figures such as that 'Trident or MX has an accuracy of so-and-so many metres and thus such and such a probability of destroying a certain Soviet target'. What this misses is the complexity - and vulnerability - of the processes that sustain the simplification of giving a clearcut 'accuracy' figure.

What is needed for the simplification is the assurance that the behaviour of missiles on test ranges is not merely local (that is, specific to these ranges) but can reliably be extrapolated to the novel trajectories of wartime.

This process is vulnerable both 'ideologically' and 'politically'. It was significantly dented in the United States in the early 1980s by a well-publicised set of criticisms - even though those criticisms were quite limited and 'technical' in their nature, and did not tap the very important resource of 'insider' uncertainties about the process, nor the more directly 'social' ways in which its legitimacy could be undermined.¹⁹

¹⁸Further details, and references, will be found in MacKenzie (1989).

¹⁹Thus there is some evidence that the products of this process are to a degree 'bureaucratically' determined - that faced with a challenge from more 'accurate' Navy missiles, there has been a change in the way the Strategic Air Command and Air Force Headquarters perform the final stage of the process of constructing missile accuracy figures, a change that has had the effect of shifting those figures from being 'conservative' to being 'optimistic'. In American political culture, that this has happened would be widely believed. But the point was never raised.

The process's political vulnerability can be seen in something the Americans currently cannot do, something which is a significant constraint on their development of 'warfighting' nuclear capability. Many proponents of such a capability look to the development of manoeuvring re-entry vehicles that take a 'fix' as they come down through the atmosphere so as to increase the accuracy with which they hit their targets. An important argument against this - in the light of the point about unexpected allies it is worth noting that it has been put forward by powerful figures within the US Navy - is that 'realistic' testing of such a system could only be done overland, since the system would probably work by taking radar fixes on terrain features. But the American defence establishment currently reckon it to be politically too dangerous to try to conduct ballistic missile tests over the Continental US (excepting those big, but still in the view of many not big enough, chunks that are owned by the armed services).

An obvious option would be to follow the example of Cruise and test in remote areas of Canada. One constraint on this is the fierce opposition that Cruise testing has generated there. But another is what is taken to be required for ballistic - as distinct from cruise - missile testing to be considered 'adequate': that is, to generate delocalisable knowledge. Insiders understand this process to be a theoretical one, to involve the construction and testing of a theory (or set of theories) of the missile.²⁰ Doing this involves sophisticated and expensive range instrumentation. It would be both difficult and extremely expensive (even in defence budget terms) to create new facilities for this in remote areas of Canada.

So all current US strategic ballistic missile tests are conducted over the ocean, where the only people around are reckoned politically powerless. Most numerous are the Marshall Islanders. Although, through the work of the Nuclear-Free Pacific campaign and Greenpeace, the situation of Marshall

²⁰Thus ballistic missile accuracy figures are not constructed simply in an empirical 'fire 'em and see where they land' sense. Extrapolation from test range to wartime trajectories involves producing and seeking to 'verify' what is called an 'error budget': a mathematical model of all significant error processes. Insiders accept that without this the extrapolation does not work. Three statements of the negotiated nature of error budgets that would warm the heart of the most relativist sociologist of science will be found in the 'insider' testimony of Drucker (1962), Topping (1981) and of Deputy Director of Defense Research and Engineering J B Walsh to the House Armed Services Committee (1976, 199).

Islanders has been publicised, the connection of their situation to the 'simplification' of missiles, and thus to the credibility of 'nuclear war-fighting' strategies, is only beginning to be made.²¹

So there is a direct link between this 'local' situation - the plight of the Marshall Islanders cleared from the military parts of Kwajalein Atoll, the end of the US Pacific Test Range, and crowded on to one small island - and questions of knowledge and power in its most 'macro' sense. What is at stake in the issue of 'simplification' of missile accuracy is nothing less than the role of nuclear weaponry in the global balance of power. If missiles cannot be 'simplified' - if they remain terrifying but fundamentally unpredictable entities, plausibly unleashable only in an act of desperate retaliation by a wrecked society - then they are, ipso facto, detached from having any real meaning as instruments of power. As right-wing strategists in the United States would complain, 'paralysis' is the result. Use of the nuclear arsenal cannot under any circumstances credibly be threatened, even implicitly, and thus no political purchase can be gained from it. In the Clausewitzian code in which the issue has been debated in the Soviet Union, war would have ceased to be an extension of politics by other means.

²¹See Johnson (1984, 46-48).

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Edinburgh PICT research is based at the Research Centre for Social Sciences and draws on expertise in the Departments of Business Studies, Economics and Sociology, as well as the Science Studies Unit. The group starts from the assumption that the development and implementation of new technologies cannot be wholly explained by technical considerations, but that complex social, political and economic factors are involved. The research effort therefore focuses on the 'social shaping' of ICTs, at the level of detailed technical design. It aims to elucidate the considerable scope which exists - for both producers and users of technology - to influence the direction and consequence of technological change. Much of the research involves building strong links with the policy community, in industry and in government.

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