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Human Economists and Abstract Methodology

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ABSTRACT

Many economists, and even methodologists, believe that the very abstractness of abstract methodology (AM) betrays it into either authoritarianism or vapidity. But though AM can certainly suffer from these defects, abstractness is not the cause of them. The root problems, arising from AM's justificationist history, are its impersonalism, its assumption that the logic of research is Aristotelian, and its stress on the distracting empiricist distinction between observation and non-observation. Perhaps, then, AM can be revived in a form which attends to individual researchers and their actual use of logic, and applies to all branches of research. A revived normative AM, attempting to foster 'logical progress', would consider the situation of the individual researcher, learning from the approach to ethics called 'virtue theory' as it did so. Methodologists of economics, despite considerable agreement about the deficiencies of modern economic research, have proved impotent to correct them. They should switch some of their attention from research outputs towards individual researchers and their progress; and to developing 'research utopias' in which progressive researchers, and hence research, might flourish.

1. INTRODUCTION. ABSTRACT METHODOLOGY: ABOLITION OR REFORM?

THE VIEW HAS GROWN in the last few decades that traditional methodology, which looks for common logical themes among all or many of the different branches of research, must be either a dangerous subject, encouraging ignorant authoritarian meddling in the well thought out practices of experienced and knowledgeable specialists, or else a bland and irrelevant subject, too vague and introverted to affect research for either good or ill. However, it may be that though many criticisms of traditional methodology are correct, thinkers about methodology have not drawn the correct conclusions. The symptomology may be accurate, but the diagnosis false and the terminal prognosis unjustified. Taking full account of the criticisms, we may be able to develop a form of methodology that can benefit researchers by means other than decrees and prohibitions.

I shall argue that this is indeed the case. The article covers much ground rather quickly, being intended as a thought-provoking prolegomenon, rather than a definitive statement of unalterable views. As regards its normative conclusions, it tries to set an agenda for discussion, rather than laying down many detailed recommendations. Section 6 makes more specific recommendations about economic research, but does so introducing supplementary claims absent from earlier sections.

Two words in the title need clarification. By 'methodology' I mean 'the study of the logical aspects of research'. Such a definition, emphasising logic, is more controversial now than it would have seemed a few decades ago. Machlup (1978) gives (Chapter 1) a helpful history of the term 'methodology', quoting with approval (p. 9) a mid-twentieth-century dictionary definition of methodology as 'a branch of logic dealing with the principles of procedure whether of theoretical or practical science'. My definition is in the same tradition, except that it takes methodology to study the procedures of research, rather than of science. Focussing on a process rather than on a body of knowledge allows us to evade several epistemological preoccupations (such as the nature of truth or knowledge) which usually irritate researchers and indeed yield them scant practical help.

Today there is little agreement among methodologists about 'methodology'. Dow (2002, p. vii) says that 'Methodology is the field which is concerned with the foundations of economics: what the role of foundations is, what is meant by foundations, and what they might consist of'. For Boland (2001), in contrast, methodology is the union of two disjoint subjects: 'the big-M methodology that interests … philosophers of science and economics' (p. 4) — useless, he believes, to mainstream economists — and 'The only methodology questions of interest in mainstream departments' which are 'about modelling techniques' (p. 6). Such discrepancies occur because the claim of 'methodology' to apply to many research subjects has gradually come under a cloud (see especially Weintraub, 1989). If the word is to survive, Boland and others feel, it must be given a new, particular meaning within economics. I shall argue, on the contrary, that the abstract study of research is both possible and necessary; in particular (section 6), that the deficiencies of modern economic research can never be cured unless we are prepared to consider the common qualities of all research.

The adjective 'abstract' also needs clarification. It describes an approach (as in 'abstract algebra') that deliberately, but usually temporarily, ignores certain aspects of a subject, in order to explore commonalities with other subjects. Once progress has been made with the relatively abstract analysis, the details can be seen in an illuminating context, and are re-introduced. Microeconomists, for instance, have found it useful to develop a theory of goods, preferences, and risk, that abstracts from — identifies important common features between — extremely diverse economic situations. Of course an economist discussing, for instance, futures markets for energy, will draw both on this general theory and on her knowledge of the special features of energy contracts, regarded as goods.

What criterion of abstraction is chosen, what is to be retained in the process of abstraction, is crucial. Since methodologists have grouped together different research subjects at different times, the scope and meaning of 'AM' have varied accordingly. The most influential criterion since the eighteenth century has been that of the empiricists, according to which we should consider together all branches of research that rely on observation. I shall argue that this was a mistaken grouping, and that it would be better, as a first step, to consider together all branches of research that use the logical devices permitted in natural language. Natural sciences such as physics; social sciences such as economics; prescriptive subjects such as ethics and (prescriptive) decision theory; highly deductive subjects such as logic and mathematics; even crank subjects, whatever those may be; all will be grist to the mill of AM, if the reform proposals below are accepted. For there can be progress even in crank subjects, as errors are gradually eliminated.

I start by listing in roughly chronological order the normative conclusions of some well-known theories of AM. It will do no harm, given the aims of this article, to represent each theory by a single mnemonic phrase. We can find a number of ideals, either implicit in researchers' work, or explicitly urged on researchers by methodologists. In roughly chronological order, starting in mediaeval times, some of these ideals are as follows. Researchers wished, or were urged, to show that their theories or conclusions:

Were stated by, or were implicit in, some recognised textual authority. (Notably Aristotle, but in the later mediaeval period classical authors generally.)

Were 'induced' from repeated observations. (Associated with Bacon, 1620.)

Were logical consequences of very clear or indubitable perceptions. (Associated with the seventeenth-century rationalists Descartes, Spinoza, and Leibniz, and in economics especially with Robbins, 1932.)

Derived ultimately from sense perceptions. (Associated with seventeenth- and eighteenth-century empiricists such as Locke, Berkeley, and Hume; the programme has not been directly influential in economics.)

Had been established by the methods of the natural sciences; especially using replicable experimental evidence, or at least public evidence. (Associated in the physical sciences with Comte's nineteenth-century positivism, and in economics notably with Hutchison, 1938; Samuelson, 1947.)

Had survived severe empirical tests without refutation. (Associated in the physical sciences with Popper 1934, 1963; and in economics especially with Blaug, 1980; see also Blaug, 1994.)

Led to surprising but correct empirical predictions. (Associated with Lakatos 1978; for the relevance of Lakatos's general position to economics see Latsis, 1976; Weintraub, 1985; Backhouse, 1998.)[2]

Let us call the analyses that generated these proposals 'traditional' AMs. The first five were justificationist in spirit: each proposed some touchstone allowing us to distinguish truth from error, or from mere opinion. Most were empiricist, in the special reliability they ascribed to observation. (Note at this point the conflict with AM conceived as a logical subject. A researcher, rather than a logician or methodologist, should be the one to make judgments of reliability or otherwise.) The final two analyses were explicitly fallibilist, repudiating touchstones of truth; however each had alternative touchstones, sharply demarcating research activities that were approved, from those that were deprecated.

Now I review, almost equally cursorily, accusations that have been levelled at several or all of the traditional AMs, as they were applied in practice. Some of these make telling criticisms which any would-be reformer of AM must surely accommodate. I start with this group of accusations, including brief descriptions of the action to be taken about them in this article. The major positive contentions of this and later sections are italicised.

The belief of the old justificationist AMs that they could establish truth was doomed from the start, by the well-known infinite regress problem of justification. The negative force of this argument finally came to be recognised during the twentieth century. Opinions have varied about the appropriate fallibilist response. I will argue that we should think of methodology as being about progress in research, rather than about science, truth, or justification; that we should think of it as asking questions, rather than making truth-judgments or issuing orders.

No traditional AM refers to researchers' opinions and judgments, though these are in fact the mainspring of the research process. Indeed. Every conception of research implies some conception of the researcher, just as every theory of microeconomics, decision theory, or game theory implies some conception of the economic or decision-making agent. And if this notional agent is an unlikely or even inconceivable human being, so much the worse for the theory in question. The only solution seems to be to base AM from the outset on the situation and judgment-making potential of the individual researcher.

Though there may be 'something in' each of the traditional AMs, in favourable circumstances, there is no compelling reason to afford any of them special status among research methods. The exclusivity which was claimed for them resembles the unjustified totalitarian claims made by big governments, with analogously unpleasant consequences (hence the disparaging term 'big-M methodology'.) I will argue that the roots of methodological authoritarianism lie in impersonalism, which we have already mentioned, and in an oversimplified account of logic. As regards the latter, we should re-analyse research logic, in order to discover whether one logical method is necessarily pre-eminent. We shall find that it is not. Research throws up extremely varied logical situations. These cannot be foretold by methodologists, but must be made explicit, problem by problem, project by project.

Each traditional AM implies a demarcation criterion, or methodological touchstone, separating practices that accord with the prescription, from those that do not, leaving us ignorant how to proceed in research areas that cannot be pursued by the recommended method. These areas naturally become regarded as inferior, or even beyond the scope of rational discourse. Such a fate befell ethical enquiry in the empiricist period. But if we consider the logical situations of actual researchers, and what actions they might rationally take in response, we shall find that these are much broader than the traditional AMs allowed, and certainly include research into ethical matters. Most economists would agree that the theory of social choice, for instance, can be pursued by rational means, devoid though it is of observation. For its chains of inference sooner or later collide with *a priori* moral intuitions, in the same way that physical scientists' chains of inference collide with observations. Whether the inferences should give way, or the intuitions/observations, then has to be decided.

The empiricist AMs in particular provide little clue as to how to proceed in economics and other social sciences, where many disturbing factors obscure the link between theory and observation (the point is stressed by for instance Lawson, 1994); or in ethics, mathematics, or prescriptive decision theory, where observation plays little role. The apparent methodological importance of the observational/non-observational distinction springs from the *confidence* a researcher sometimes places in the observational parts of her world view. We shall construct methodology round these degrees of researcher confidence, rather than round the empiricist distinction. We shall find that supposedly empiricist issues and methods (such as falsification) then generalise naturally to non-observational contexts, including ethical ones (such as Arrow's, 1950, analysis of ethically acceptable societal preference functions; see the discussion in section 3 below).

Researchers can and do simply ignore the traditional AMs. Or if some big-M regime wields power, and researchers are forced to pretend to themselves or others that the prescriptions are being followed, a hypocritical and misleading style of writing results. The problem is again impersonalism. Accordingly we seek ways of addressing researchers in ways less easy to ignore. One is to assume an interrogative rather than imperative stance towards them. Another is to try to learn from the tradition in moral philosophy called 'virtue ethics'; we expand the point in section 5 below. Either way we should treat research hypocrisy as a distinctive problem to be taken seriously, rather than as an irritant to be ignored as far as possible — if researchers are indeed hypocritical, it is probably because we methodologists are making unreasonable demands on them.

So far we have considered common criticisms of the traditional AMs that must, I think, be accepted. In contrast, we now look at criticisms that should be rejected, or accepted only in modified form. Again, our responses to them will be amplified as we proceed.

It is often argued that AMs' determinedly abstract nature — a deliberate ignoring of the true complexity of the research situation — leads them to misrepresent actual research, opening the way for authoritarian meddling. However, as a matter of simple logic, abstraction (the deliberate ignoring of special features) implies relatively weak conclusions, and cannot be blamed for authoritarianism. We should therefore seek alternative explanations for the authoritarianism to which the traditional AMs were undoubtedly prone, and adjust AM appropriately.

Next, some authors argue that AMs seem to assume that research is governed entirely by logic, ignoring the importance of extra-logical methods such as the use of metaphor. Now it is true that 'methodology' as defined above governs only the logical aspects of research, and could be renamed 'logical methodology'. But there is no intention to denigrate other approaches to the study of research, such as literary-critical or economics of science approaches. Not denigrate, but perhaps re-interpret: the scope of (logical) methodology is currently under-estimated, because 'logic' itself has been too narrowly construed. Consequently, the insights methodology offers are sometimes interpreted as purely literary or post-modern, without reference to their logical aspects.

A more explicitly post-modern claim is that there is in fact no fixed point from which to survey and appraise all research areas. Claims to have found common ground between them are pretentious and false. This is the position of, for instance, Weintraub (1989) in his sweeping assault on 'big-M methodology' in economics. We deny the claim head-on, arguing that, on the contrary, there is important common ground between all research areas, which allows AM to contribute to their appraisal and, more importantly, their improvement. This common ground includes: the language and logic of enquiry; researchers' possession of beliefs; their use of judgment; their research practices and research institutions; the sociology of research. So we can expect to find that research into business cycles resembles research into auctions, black holes, Renaissance art, and medical ethics in important *infrastructural* respects, while differing in other important ways.

Some authors are wary of the claim that AMs can help researchers make progress. To these authors, such a claim sounds suspiciously like the old and discredited claim that scientific method could establish truth. Yet for researchers to repudiate 'progress' would surely be a grave decision, to be avoided unless we have first done our utmost to make sense of the concept. In fact some aspects of progress (for instance, the elimination of inconsistencies) are entirely logical, and can be discussed by AM. After extending our conception of logic in sections 3, we will find (section 4) that there are surprisingly many ways to make 'logical progress'.

A final criticism we should mention is that if AM manages to avoid authoritarianism, it can do so only by becoming so vague and obscure that it avoids saying anything that applies directly to actual research practice. However our decision to study research rather than knowledge allows us to avoid many well-known problems of epistemology that are in fact irrelevant to research. In studying research as the occupation of actual human researchers, whose beliefs are crucial, rather than as some impersonal activity, we can help ensure that our observations relate directly to research practice.

The remainder of the article implements the actions and expands the responses suggested above, developing a reformed AM as it does so. Section 2, 'Individualistic abstract methodology', distinguishes between abstraction and impersonalism. The former can help; the latter usually hinders. The decision to deal with individual researchers opens the door to methodological consideration of researchers' fallibility, lack of omniscience, and their need to make judgments about what are promising lines of enquiry. Section 2 also suggests the real reasons for the authoritarianism and other problems of the traditional AMs.

Section 3, 'The unity of research logic', noting that researchers' use of natural language is central to research, suggests that we should pay special attention to natural logic (defined as the logic of natural language), which is much richer than the Aristotelian logic to which the traditional AMs confine themselves. Natural logic can deal with tentative, qualitative, evaluative, comparative, prescriptive, and moral judgments; thus a reformed AM could permit discussion of research containing any or all of these types of judgment. The logic they share, across different research areas, unifies the research world.

Section 4, 'Logical progress', argues that, far from 'progress' being an unintelligible concept, some important aspects of 'progress' can be defined even at the abstract level. A researcher is making logical progress when she eliminates *inconsistencies*, or extends the *scope* of her judgments, or becomes more *confident* in them. The three criteria often war with each other; for instance in order to facilitate advances in scope and confidence we might need to tolerate temporary inconsistency. Research methods do not (as the traditional AMs usually claimed) fit a unique logical pattern; consequently researchers should be pressed harder to explain why or whether their own research strategies are really appropriate to their own research situations. The conclusion of the first four sections is that the correct relation of a methodologist to a researcher is as a Socratic interlocutor, not a judge or dictator.

Section 5, 'Prescription, virtue, and progress in methodology', argues that one can exploit structural similarities between methodology and moral philosophy to understand the relationship between impersonalism or its opposite, and prescription. This is because moral philosophers realised before methodologists that if we prescribe impersonally (disregarding, that is, the nature of those we address), then we shall usually be ignored. One solution is to emphasise (individual) *virtue* rather than (impersonal) *justice*. In 'virtue ethics', character rather than behaviour, actions, or choices, becomes the main preoccupation. The section advocates adding 'research-virtue-ethics' to the methodologist's agenda, and devoting systematic thought both to what the 'research virtues' are, and to what institutional changes might succour them.

Section 6, 'Abstract methodology and the reform of economic research', argues that AM can help us think about the strategic problems of economic research. There is a rather surprising degree of agreement among methodologists of economics about which research activities are undesirable. But despite this agreement, the criticised features become ever more entrenched. Specific reforms are suggested; however the strategic conclusion is that the methodology of economics needs the tools provided by a reformed AM. Without them, we will remain baffled and impotent in the fight against ritualistic and trivial economic research. Section 7 provides a concluding summary.

2. Individualistic abstract methodology

The present section argues that we should differentiate sharply between abstractness and impersonalism. Researchers' well-known dislike of methodological abstractness is really a (justified) dislike of three things: impersonalism, logical crudity, and the empiricist over-emphasis on the role of observation in research (economic research certainly, but also research in the physical sciences).

Insofar as AM suffers from authoritarianism, abstraction is not to blame for it.

An opponent of abstraction in methodology might argue as follows: 'Abstraction, by your definition in section 1, means deliberately ignoring certain aspects of a subject. So AM means proceeding on the basis of a self-confessedly limited knowledge of, for instance, economics. This systematic ignorance forms the background for the prescriptions of any AM, and explains their numerous defects. AMs, to immunise their simple-minded dictates against criticism, disregard competing suggestions from knowledgeable specialists as to how to research in their subject. Indeed, specialist criticisms made against AMs cannot even be *considered* using their abstract, non-specialist terminology. So it is abstraction that is the root cause of the AMs' ignorantly authoritarian, even totalitarian, aspect.'

The argument is advanced with panache by McCloskey (1985, 1994; but see also Blaug, 1994) against some exponents of the traditional AMs in economics, but it fails as a general criticism of AM. It accuses the traditional AMs of being rash and naïve in how they derive conclusions from their abstract analysis of research. But AM itself should no more be blamed for such rashness and naïveté, than the subject of statistical methodology should be blamed when some simple-minded fanatic claims that chi-squared tests are the key to all statistical problems, and that any statistical enquiry must culminate in such a test. If we do correctly identify things true of all branches of research, then these things will certainly be true of research in economics, and economists need not fear them.

AM, as I have suggested reforming it, is closely connected with logic, and a comparison of the two subjects shows directly why AM need not be authoritarian. Logic is an infrastructural subject used, semi-consciously, by all researchers. There are specialist logicians, but no economist would dream of placing them 'above' economists, because of course logicians have, as logicians, no economic knowledge. Nonetheless, logic is central to economic research. And so, we claim, is AM. An AM of all research is, likewise, part of the infrastructure of economics (and of all other research subjects). Since it is included in the methodology of economics, it is always and forever weaker in economic implications than that subject. Its value lies not in giving commands, but in isolating for attention the logical aspects of a research problem. We must try to explain, then, why the traditional AMs seemed so oppressive:

The ills of the traditional AMs have three linked causes, arising historically from their originally justificationist aims. These are their impersonalism, their incorrect assumption that the logic of research is Aristotelian, and the failure (notably of the empiricists) to choose a natural criterion of abstraction.

Impersonalism. For justificationist AMs, the central preoccupation was to distinguish truth from error; for their fallibilist successors it has been to distinguish good practice from bad practice. Such AMs sit naturally with an impersonal form of language; with recommendations stated in the passive voice, regarding observations that should be made, experiments that should be conducted, conclusions that should be drawn. Researchers, insofar as they make an appearance at all, are exogenous theory-generators, or mere lab-assistants for methodologists. They have neither opinions nor judgments, no tentativeness, no arguments with their colleagues, no hunches, no inconsistencies, no moral views. Such heroic omissions reflect the belief that science should deal with 'the objective', which is supposed to command the assent of any rational person, irrespective of prior opinion. Impersonal methodology, which by definition cannot focus on the researcher, therefore focuses on the products of research: projects, theories, articles, books. Likewise its implicit target audience, far from being the researcher, is the funding council, to be advised for instance about the progressive or degenerating nature of particular research projects.

Logic. The *logic* of justificationist and impersonal systems has always been Aristotelian, in the sense that these systems use a small number of logical values: *true, false* in Aristotle's original scheme; *uncertain* was allowed later. Non-Aristotelian values such as *probable* and *unlikely*, and relational judgments such as *X is less certain than Y* are excluded. The twentieth-century systems of Popper and (especially) Lakatos were more tolerant. Lakatos's (1978) distinction between hard-core and auxiliary hypotheses was in effect a crude but important concession to the role of *researcher world-view*. Lakatos's scheme, accordingly, proved less inapplicable to economics than its predecessors (see Backhouse, 1998).

But on the whole the Aristotelian assumptions of the first traditional AMs were retained by their successors, and research was treated as having its own special, narrow, language and logic. The traditional AMs' inevitable failure (because research actually draws on many sophisticated logical resources of natural language) brought abstraction itself into disrepute. The traditional AMs were both abstract and authoritarian, and association was taken for causation. The necessary reforms are discussed in sections 3 and 4.

Criterion of abstraction. Among the traditional AMs, the most influential in recent times have been the *empiricist* ones, which treat *observations* as authoritative, perhaps even the source of *justification.* Empiricism affords high status to branches of research such as physics, where observation is often unproblematic, and moderate or low status to those such as economics, where observation often entails major problems of interpretation. Researchers such as ethicists and mathematicians, who hardly use observation, are sidelined. I argue in section 3 that empiricists fixed on observations, because in some physical sciences researchers are particularly *confident* in them. If so, it is confidence, not observability, that has fundamental methodological importance; and the empiricist grouping is a confusing one.

If we correct these errors, but maintain abstraction as a valuable method, the resulting account of methodology will be radically different. We begin by dealing with *impersonalism.*

Methodology needs a 'notional researcher', analogous to the Homo economicus who appears in both micro- and macroeconomics.

If the traditional AMs erred by ignoring individuals, a reformed AM should resolve to introduce individuals at the outset. We need not consider researchers in all their uniqueness and complexity. The move embodied here is suggested by the introduction of *Homo economicus* (a facetious expression, but the concept is a valuable one) into economic discussion. *Homo economicus*, understood as 'the economic agent as envisaged by economic theory', provides a stark presentation of the commitments made by a theory. If a theory presents *no* picture of *Homo economicus*, then that itself is interesting information. Macroeconomics is of course a contentious area in this regard. Hoover (2001) argues, against the micro-foundationalists, that macroeconomists should definitely dispense with *Homo economicus* and regard the concepts of their subject as autonomous.

The macroeconomic debate about microfoundations may eventually go either way (Bailey, 2002). In methodology, however, the outcome seems preordained: methodology cannot dispense with the 'microfoundations' provided by a picture of the individual researcher. For in research, as opposed to gas kinetics or (possibly) macroeconomics, individual differences certainly cannot be smoothed out. Extremes in research are at least as important as averages, in the long run.

That the notional researchers of the traditional AMs differ so sharply from actual researchers is, we have argued, a grave problem for these AMs.

But are the revisions to AM proposed in this article any better in this regard? Is the proposed new form of notional researcher any more realistic than the old? Not in all respects, admittedly, since like most methodologists I shall leave systematic consideration of researchers' economic (for instance, pecuniary) motivations to the economics of science. Once AM starts to discuss individual researchers, it becomes important to distinguish between the two approaches. The economics of science (see for instance Stephan, 1996) discusses research as a good with an associated labour market and incentives, while methodology deals with a more technological and normative subject: how to make this type of good. The economics of science, we could say, deals with the production of research, while methodology deals with its manufacture.

The most striking features of this manufacturing process are the constant interplay of the notional researcher's *judgment* and *reason* in the attempt to *solve problems*, the routinely *tentative, delicate* and *fallible* nature of these judgments, the use of *natural language* and *natural logic* to express and manipulate them, and the *interaction* between researchers which language and logic permit, even when their judgments differ. Where I do claim greater realism than the traditional AMs is in trying to recognise the centrality of all these qualities to research. Against the empiricist objection that this characterisation omits observation, I contend that the methodologically important aspect of observation is that it sometimes leads to judgments that the researcher regards as *particularly secure*. But it is not unique in this; for instance a firm moral judgment can have logically identical methodological effects. The matter is pursued in section 3.

Abstract methodology is a natural field of study.

There is point to AM if there is something substantial and interesting in common to the methods of research, across the huge variety of subjects in which research is conducted. But there is indeed substantial, interesting common ground: namely the language and logic common to all researchers. I argue (*contra* Weintraub, 1989; regarding which see Mäki, 1994) that the centrality of these subjects to research makes 'the study of the logical aspects of research' a promising field of study, a claim the coming sections attempt to validate.

Insofar as methodology is abstract, it considers the logical structure but not the content of the researcher's world-view.

In appraising a piece of economic research, the methods of AM are, we shall argue, necessary but certainly not sufficient. The abstract methodologist, *qua* abstract methodologist, never disputes the researcher's particular judgments, except where they are logically inconsistent. It is the researcher's own beliefs, judgments, doubts, questions, and perplexities, that are to count. The abstract methodologist, like the logician, the mathematician, the librarian, and the computer technician, offers a different, infrastructural, type of help from what the researcher's co-specialists offer. As we shall see, one type of help (which may not be perceived as such) is to coax into explicitness the relationships between the researcher's initial world-view and her research activities.

Though the distinction between content and method is conceptually clear, things are muddier in practice. All but the most dunderheaded researchers form a conception of their own method, a role they perform with varying degrees of expertise. But it will be convenient to speak as though methodologists and researchers were disjoint tribes.

3. The unity of research logic

An actual researcher has a command of language (which has received much attention in the recent period of reaction against the traditional AMs) and therefore a command of the logic of the language she uses (which has received less emphasis). I now argue that the rejection of impersonal methodology requires a more capacious account of logic than was given by Aristotle, or even by modern epistemologists and mathematical logicians.

The benefits of 'objectivity' in the eyes of empiricists arose from the attempt to identify what was publicly known. *These benefits come, however, not from the reliability of observational knowledge, but from researchers' desire for consistency in their own world-view.*

Uncontroversial observational facts of the type sometimes available in physics, and even occasionally in economics, found their way to the heart of the empiricist AMs. They provided an indispensable constraint, an invaluable safeguard, the empiricists felt, against theories that were merely well-packaged or emotionally appealing.

But in considering the methodological role of 'uncontroversial observational facts', assuming that these exist, we realise that neither their uncontroversialness, nor their observational nature, nor even their 'fact-ness', are necessary for the purposes to which the empiricists put them. Take as an example the logic of falsification so important in Popper's (1934) and Blaug's (1980) analysis. To see that there is nothing intrinsically empirical about falsificationism, suppose that I believe that it is wrong to steal. If, confronted with the possibility of a mother stealing to prevent her child from starving, I accept that in these circumstances it is not wrong to do so, then my original belief is refuted-for-me, and I must set about revising my moral system. What is methodologically important is that I *accept* the particular judgment that falsifies-for-me my original moral belief; just as in the case of a puzzling observation, it is the theorists' *acceptance* of the observation that triggers the theoretical upheaval. Beliefs drive research directly; facts drive research only via beliefs.

Economists are familiar with at least one important application of Popperian falsification in moral reasoning, namely the application that appears in Arrow's (1950) discussion of social constitutions. Arrow's Impossibility Theorem, however it is stated, is usually proved (see for instance

Geanakoplos, 1996) in an arrangement which derives a striking consequence – unshared dictatorship – from axioms that at first appear benign. For Arrow and any of his readers who reject on ethical grounds this logical consequence of the axioms, reforms must clearly be made to the moral system initially suggested.

The empiricists Popper (1934) and Blaug (1980, 1994) both afford empirical testing a pre-eminent role, because they believe it to command the assent of all rational researchers. Of course there may occasionally be such concord between the individual researcher and her research community that the distinction between the two ceases to matter, temporarily and locally. However this scenario of near-unanimity is obviously a special and limiting case, which occurs in observational science only when researchers have very similar beliefs not only about what is actually observed, but also about the theoretical context of the test. For reasons we saw in the ethical examples just given, a logically equivalent situation could easily occur in ethical debate, if there were sufficient agreement about particular moral beliefs. But unanimity is really a side-issue raised by the attempt to create an impersonal AM. Whether or not it is present, the question 'Do you really accept such-and-such an implication of your moral system, and if not what are you going to do about it?' always presses on and harries moral theorists, or any proponent of a moral code. Falsificationism has proved in fact a more potent weapon in moral debate than in positive economics or even in physics. It is potent because crucial, noise-free, and inexpensive ethical thought-experiments are very easy to devise.

The contention that AM is possible and useful is strongly linked to the claim that all research subjects are conducted using the same or similar logical resources. I have just given an example of the fact that there is no special 'empiricist logic' which segregates the methodology of observational subjects from that of non-observational ones. The reason is that the research process is rooted in the judgments of practitioners. Even if observations (or intuitions for that matter) are 'authoritative' in some way, their authority can affect research only via these judgments. So a logic incorporating *judgments* is essential to the understanding of research:

To understand the rational motivation of research, we must understand the logic used by researchers. Research logic is 'rich' in that like natural logic (the logic, that is, of natural language) it allows inferences regarding the degree to which a researcher endorses particular propositions, thus helping us understand the paths her research might take when difficulties are encountered.

The verb 'endorses' in this contention is chosen for its versatility. It could be taken to mean 'believes', 'regards as likely', or perhaps even 'commits herself to', as when a Lakatosian researcher (a micro-economist perhaps) espouses certain propositions (axioms of consumer choice perhaps) she believes methodologically progressive, because they have testable implications rather than being necessarily true.

Like many vague and qualitative concepts, endorsement nonetheless has precise inferential properties. A researcher, interpreting endorsement as relating to likelihood, will realise that if she maintains that 'It is very likely that *p*', and '*q* is likelier than *p*', she cannot consistently maintain: 'It is unlikely that *q*'. Being human, she will frequently in fact violate consistency, but I assume that she will wish to rectify the situation, if she notices it, and it impinges on her research aims.

What is important is that natural language provides such a sophisticated machinery for expressing the degree to which its adjectives (for instance *green, likely, similar, believed-by-me, competitive, contestable, fiscally neutral*) apply. The logic of judgmental predicates is indispensable to the researcher's exercise of her judgment, as becomes evident when we hear researchers in any field discuss what course their research should take, attempting in the presence of radical uncertainty to decide what is a reasonable bet. To do so they must draw on (i) unary predicates (*likely*); (ii) modified predicates (*fairly likely, unlikely*); (iii) binary relations (*is likelier than*); (iv) modified binary relations (*is much likelier than, is slightly less unlikely than*).

The logical manipulation of these highly graded judgments is necessary, though insufficient, for the making of intelligent research decisions. The sparse evaluative apparatus of the two- or three-valued Aristotelian logics, on the other hand, condemns the traditional AMs to endless difficulties of the Duhem-Quine type — situations which of course really call for the researcher to make (fallible) judgments about how to respond when an obstacle is encountered. (For examples of Duhem-Quine difficulties in the testing of economic hypotheses such as the Phillips curve, the ineffectiveness of monetary policy, and the non-existence of super-normal profits in asset markets, see the discouraging article by Sawyer *et al.*, 1997).

Duhem-Quine problems are usually set in the context of an anomalous observation or experimental result, but they also arise when a purely logical error in a theory, or a conflict between two accepted theories, is noticed; thus they arise in prescriptive subjects just as much as in descriptive ones. In fact our analysis of research logic shows that the positive/normative distinction has no abstract methodological implications whatsoever.

(Reformed) AM makes no distinction between prescriptive subjects (such as normative economics and prescriptive decision theory), and descriptive subjects (such as positive economics and descriptive decision theory).

This statement, though definitionally true given the characterisation of AM provided in section 1, may appear to conflict with the high prominence usually given to the Humean positive/normative distinction. So some clarification may be helpful.

A passage by Hume (1740, pp. 203-4), it is often claimed, shows that from non-ethical premises alone one cannot validly draw ethical conclusions.[3] Even if we accept the claim, it is devastating for ethics only if there is some good reason to exclude ethical premises from the premises of prescriptive ethical statements. And if there is a good reason, let us hear that. Note that Hume's point is completely symmetric, and works equally well in reverse. One can imagine a morally-minded economist deprecating positive economics on the grounds that its propositions could not be validly inferred from those of normative economics. Hume favours the empirical over the ethical only if the scales have already been weighted in favour of the empirical.

But methodologists need not intervene in such disputes: need not attempt to rule which ethical or factual statements are more reliable than others. For the dynamics of research are a matter not of content as such, but of logical relations between (i) the different parts of the researcher's network of endorsements; (ii) her research aspirations. Whether beliefs she regards as particularly secure arise in the observational world, or the moral world, or somewhere else, and the extent to which these come under attack, are matters for her and the fellow-researchers who influence her. The fact-value distinction, or more generally the descriptive/prescriptive distinction, is of no abstract-methodological significance.

To see how little attention methodology need pay to the descriptive/prescriptive distinction, consider a game theorist, a die-hard empiricist initially resolving to have nothing to do with prescription. But the logic of her situation will soon draw her into prescriptive research, when she comes to test – by experiment and observation – a powerful null hypothesis about descriptive game theory; namely, that agents behave as they 'should', according to the prescriptive parts of the subject. Before devising her experiments, she must of course consider these prescriptive parts, to extract the descriptive predictions they entail (under the null hypothesis just mentioned). In doing so she will notice to her official surprise that her research has taken a purely rational, non-experimental turn (for some of the difficulties involved in such a prescriptive enquiry see Sugden, 1991).

A researcher (to summarise) is not simply a locus of theories. She has attitude as well. She endorses some things more strongly than others. Unlike any researcher envisaged in the empiricist AMs, she may well have relevant prescriptive and moral beliefs. Again, she has differing degrees of commitment to, or against, each of these beliefs. The first task for a methodologist in studying an individual researcher is to construct a sort of 'logical map' of the researcher's world-view; its doubts and certainties, logical interconnections, inconsistencies and areas of aspiration. Having done this, the methodologist can ask whether there is hope of her making discernible progress, the subject of the next section.

An illuminating introduction to moral logic — the logic, that is, of moral language — appears in Hare (1952). On the fact-value distinction and the taxonomy of economics, see Weston (1994). On its unimportance for the strategy of economic enquiry, see Roy (1989).

4. Logical progress

If 'progress' in research or enquiry means 'getting closer to the truth', then understanding 'progress' seems to involve understanding 'truth'. Pragmatist and relativist onslaughts on 'truth' have therefore left methodologists shy of analysing 'progress' (as opposed to using the concept). 'Progress' does not appear as a head-word in the handbook of economic methodology of Davis et al (1998), nor as an entry in its index. It is likewise absent from the 10-page index of McCloskey's (1994) 445 page book on knowledge and persuasion in economics. The decline of 'progress' is usefully chronicled by Dow (2002, Ch. 6).

Our understanding of AM as an abstract subject clarifies matters. AM by our definition avoids discussing the content of research, and therefore avoids entanglements with 'truth'. Whatever we manage to say about 'progress' at the AM level will, therefore, likewise be secure from contamination by problems with 'truth'.

To this end, one can define a useful concept I call *logical progress*. This limited concept of progress would seem unsatisfactory in most of the traditional AMs because, as we shall see, *logical progress* can take place in 'crank' fields of research as well as 'genuine' ones. However, AM is concerned with the logic and methods, not the subject matter, of research, and there can surely be good or bad research even in crank fields. Often we do not know whether they are 'crank' until we have done the research. One who rejects astrology need have no objection to the concept of *progress* in astrology, if the elimination of error counts as progress.

The most important aspects (for AM) of a researcher's world-view are its consistency, its scope (in the area of interest), and her confidence in her judgments. The abstract-methodological aspects of 'progress' involve advances in one or more of these three qualities.

Empiricists may think that we are making things too easy for the researcher, by ceasing to regard *empirical observation* as the final test of her work. However, the need to conform with observation is not the only source of difficulty for a world-view, as we have already seen in connection with moral theorising, for every subject makes severe logical demands on sincere researchers. If we realise that the language of research is not the relatively feeble logic of Aristotle, but one that allows subtle comparisons and greater possibilities of error, then we can identify at least three aims, which can be defined at the abstract level, for research. The difficulty of reconciling the aims means that research, in our new vision of AM, does remain reassuringly demanding. The aims are as follows:

First, *consistency,* not only within a theory, but of a theory within the researcher's world-view; a consistency which should hold across judgments as well as definite beliefs.

Next, *scope.* Researchers should be ambitious not just because they are striving for greater content *per se*, but also because as they try to extend the scope of their theories, they are likely to encounter stimulating inconsistencies in their own world-views, and productive disagreements with other researchers.

Thus economists such as Gary Becker have tried to extend the traditional framework of rational choice to analyse investments in human capital, behaviour in the family, crime and punishment, discrimination against minorities, religion, and democracy. (For an overview of all but the last two of these topics see Becker, 1993.) Becker's research motive is his desire to understand issues of intertemporal choice, society, and family. But even if such understanding is not achieved, his attempt is methodologically laudable, since it provides the opportunity for severe testing of the traditional framework. If Becker's approach fails, it is important to know where and why it fails. For criticisms of some of Becker's work by an economist who shares much of his world-view, see Pollak (2002). For a more radical, sustained criticism of the 'economics imperialism' of Becker and others, see the work of Ben Fine, for instance Fine (2001).

Though consistency provides a negative heuristic for research, and scope a positive one, the non-Aristotelian nature of research logic means that they do not together set a stringent enough standard. The researcher can achieve infinite scope and consistency by registering a 'don't know' against every proposition put to her. We must therefore value, as a third objective, the researcher's *confidence* in her judgments, meaning that, ceteris paribus, it is better for her to judge things very likely, or very unlikely, than neither of these.

I shall argue in section 6 that the research culture of economics, by striving too hard to avoid even temporary inconsistency, fails to attach sufficient weight to the other *desiderata* of scope and confidence.

AM in its relation to the individual researcher is naturally interrogative, rather than prescriptive.

The complex picture developed above, of a researcher, her world-view, her judgments, and the many ways in which she can make progress, clarifies why the traditional AMs are now rightly seen as inappropriately prescriptive, as straitjackets. Methodology need not be a matter of ordering a researcher around. Instead it may involve, for instance, pressing her on her willingness or ability to identify and take the risky and often dispiriting steps which may lead to progress.

One hopes that methodologists, ceasing to be overbearing, do not slip into the opposite fault of being blandly descriptive. One hopes that they retain some irritating qualities; those of the Socratic gadfly, trying to get the researcher to explain her research strategy; continually prodding the possibly sore spot of whether there is serious intent to make discernible progress on worthwhile issues. This direct style of interaction with individual researchers surely carries more weight than impersonal praise or condemnation of their activities. Questions are harder to ignore than commands or unsolicited appraisals.

These considerations suggest the proper type of conversation to have with a postmodern or relativist economist, who thinks that her views are correct 'for her', however strongly we argue against or contradict her. If she believes this, then further argument or contradiction is pointless. But Socrates showed us long ago that constructive engagement is nonetheless possible. The aim should be, through questioning (what do *you* think about such-and-such a case?), to get her to expand her world-view until she herself starts to notice contradictions, hiatuses, and dilemmas. At this stage she will be undertaking genuine research.

Ideally, though, each researcher carries the methodological gadfly within herself, goading her to put her logical house in order, but also to push outward towards the difficult, important issues of the economic world, where she may do work which makes her research life worthwhile.

5. Prescription, virtue and progress in methodology

Impersonal methodology ignores a central question: how to encourage researchers to follow its own prescriptions. In this section we look to ethics, in which impersonalism has long been recognised as a problem, for some of the answers.

Methodologists can benefit from the study of moral philosophy. Meta-methodological debates are often reprises of earlier debates in moral philosophy. This results from a deep structural analogy between the two subjects. Moral philosophers discuss whether 'good' (or perhaps 'better': see Broome, 1991) can be defined, how it should be defined, how to achieve good once it is defined, and what it means to be a 'good person'. Is a good person simply one who does good, or is there more to it than that? The same problems face methodologists, except that 'good' is replaced by 'good research', and 'good person' by 'good researcher'.

A priori there seems no particular reason why the analogy should help methodology more than moral philosophy. However moral philosophy got started sooner than methodology, and is in some respects more mature. By studying certain aspects of moral philosophy, therefore, we might hope to predict the course of future methodological debate. We obtained some assistance from moral philosophy above, when we drew on its observation that prescriptive subjects are no less amenable to logic than descriptive ones. We now exploit a second strand, one usually seen as antithetical to the impersonal systems of Kant, Bentham, Hare, and others, but which seems to me to be complementary to theirs.

Prescriptive methodology is pointless, unless its prescriptions are compatible with some possible psychological and sociological context. MacIntyre (1981, p23) makes the point as regards moral systems: 'A moral philosophy … characteristically presupposes a sociology. For every moral philosophy … [must offer] a partial conceptual analysis of the relationship of an agent to his or her reasons, motives, intentions, and actions, and in so doing generally presupposes some claim that these concepts are embodied or at least can be in the real social world'. The point translates exactly to the methodological context. In trying to meet it by discussing what psychological and sociological context might lead to 'good' or 'progressive' research (however defined), we can learn from the answers developed in moral philosophy.

Impersonal systems, of either moral philosophy or methodology, almost inevitably fail to obtain the free assent of the agents whose behaviour they try to constrain. According to MacIntyre's point, it could only be fortuitous if the impersonal systems of traditional AM or indeed of Bentham were freely adopted, by researchers or moral agents as the case may be. By instinctive good taste the advocate of an impersonal system might manage tacitly to comply with psychology and sociology. But the deep unpopularity of most impersonal systems, noted wryly by their supporters, and gleefully by their opponents, argues strongly against this unlikely hypothesis.

MacIntyre's point supports the view urged in section 2, that we must consider the notional researcher (or the notional moral agent as the case may be), from the outset. We must ask what makes it relatively likely that researchers will accept such help as methodology can offer.

From the tradition of virtue ethics methodologists can learn that the inculcation of 'good' or 'progressive' research cannot confine itself to defining such research, but must additionally look for ways to encourage 'good research character', those aspects of a researcher's personality that lead us, before considering her work, to expect that work on average to be good or progressive.

As a response to the implementation problems of impersonal systems — systems of 'justice' in the usual terminology — moral philosophers, beginning with Anscombe (1958) (see also Pence, 1991; Hursthorne *et al*, 1995; McKinnon, 1999), have tried to re-orient moral philosophy towards concepts which interest actual moral agents, and which therefore stand some chance of influencing them. Anscombe consequently instigated modern discussion of so-called *virtue ethics*, focussing on issues of *character*; on being for instance courageous, honest, resolute, kind, and so on, qualities most of us admire. One achieves better effects, virtue ethicists argue, by inculcating good character, than by devising choice algorithms for moral situations.

MacIntyre (1981) took Anscombe's argument an important stage further, relating an individual's virtues to their *integrity* (in the original sense of 'wholeness'): how for instance a doctor's (or, I add, researcher's) sense of self, vocation, and location in society can provide the framework which supports and coordinates all her professional skills and virtues into a morally worthy life (Pence, 1991).

Now: can we devise a 'virtue ethics' of research? What might a 'research virtue' be? Those who subscribe to some concept of progress can answer that research virtue consists of those qualities of an individual likely to lead, on average, to her making progress. Those who reject 'progress' can still fall back on a definition in terms of 'goodness'. (Of course, neither 'progressive' nor 'good' researchers in this special sense are necessarily morally good.) McCloskey would, I think, accept the concept of 'research goodness' in economics, defining it in terms of rhetorical and persuasive skill. The 'research virtues' would include the qualities of character likely to lead to these, notably the virtues of good conversation.

Does our concept of progress lead us to identify virtues of a type underemphasised by impersonal progressivists such as Blaug, and those such as McCloskey who seem uninterested in progress? I mention just one. A major feature of all existing theories (and anti-theories) of methodology can easily escape our attention. This is their evident disinclination to enquire what a researcher might discover by introspection about her research aims and methods, even though introspection about economic motives is rightly considered entirely respectable.

Yet the quality that breathes fire into research, that gives it force, direction and even urgency, can most easily be discovered by self-examination. This is *sincerity*, the short-term effect of the research 'integrity' we mentioned above. Of course Blaug and McCloskey both want sincere, committed economic research. They try to identify it from its methods and products; Blaug wants to see serious empirical testing; McCloskey wants to see writing whose persuasiveness shows that its author too is persuaded, that she has done the preliminary work that allows her to be persuasive. But neither empirical testing nor persuasive expression guarantees that the research virtue of sincerity is present. More fundamental is the inner drive to make important progress, even when this means painfully relinquishing one's own initial, comfortable, beliefs; even, perhaps, when the researcher's career may suffer.

A 'virtue' analysis cannot be kept on an individual level. It requires us to consider which characteristics of a research environment, and research community, foster the individual research virtues. Just as the extremes of impersonal methodology led to the present 'anti-big-M' reaction, attempting to close down the subject of AM altogether, so in moral philosophy there has been an *eliminatist* movement (described by Pence, 1991), advocating the abolition of abstract rights and principles, and their replacement by an ethical theory based entirely on character.

But few if any of the individual virtues, or research virtues, can either flourish or yield benefits if practised in the wrong type of environment. If methodologists are progressivists in the sense just defined, we should concern ourselves with institutional reforms as well as with individual virtue: such reforms as will inculcate research virtue. The resulting attitude to methodology and research is analogous to O'Neill's (1996, flyleaf) belief about moral philosophy, that we should attempt to construct 'a linked account of the principles which are basic for moving towards just institutions and virtuous lives'. Some of what this might mean in practice, in economics, is indicated in the next section.

6. Abstract methodology and the reform of economic research

World religions are said to differ far less in the moral codes they support, than about the rationales for those codes. Something of the same phenomenon affects methodologists of economics. It is surprising and somewhat comforting that radically different methodologists of economics, such as Hausman (1992, pp. 247-263), McCloskey (1994, pp. 111-163), Lawson (1997, especially Parts I and II) and Blaug (2002) agree more about what constitutes ignoble research, than about the theoretical basis for their dislike.

A certain list of accusations has become familiar and fairly uncontroversial among methodologists: that many economic researchers, especially those who feel professionally insecure, are technique- and publication-driven rather than problem-driven; that they misrepresent their critics and provide only formulaic responses to them; that they place excessive reliance on mathematical models, and afford excessive prestige to those good at manipulating such models; and that they ignore research in related fields — especially related social sciences - and the real economic world. A stark summary of such accusations is that many researchers do not make a sincere attempt to get anywhere they themselves would regard as intrinsically important; that therefore they are most unlikely to provide value for money to those who support academic institutions; that they employ a range of well-developed stratagems for concealing these misdemeanours from themselves and others.

What has become striking over the years is the remarkable and growing stability of the charge sheet, despite frequent, widespread, and eloquent re-statements of the charges. This stability constitutes an interesting and important problem for methodologists. It seems that opponents of insincere research, fighting local and unsuccessful tactical skirmishes, are steadily losing the war. Clearly, more strategic and probably radical thinking is needed. Perhaps AM can help, by directing our attention both to *individuals* and to *research contexts*. We need to develop our war aims: the type of research utopia we should strive for. However, according to the analysis of this article, we should first consider what we want of individual researchers, paying due attention to their relevant characteristics. Then we can think about what kind of institutions and practices would best satisfy those wants, by providing the right opportunities, contacts, stimuli, and motivations. Finally the massive task of getting from here to there could begin.

By 'ideal researcher' in what follows, I do not mean one who has superhuman creative, investigative, and ratiocinative powers, but rather the best researcher a given individual could become, given her actual levels of energy, intellect and so on. The ideal researcher suggested by the analysis above is one who tries sincerely to make progress that *she* regards as important, given her initial world-view. Since research is 'the art of the soluble', she should find problems, and preferably a research vocation, not entirely beyond her. The requirement of *importance* will tend to pull her outside her existing comfort zone but, according to the requirement of a*chievability*, not too far from it. (We can consult history here. In economics, the most innovative work has often come from those who have been willing to use concepts from several branches of the subject, in discussing problems that affect many lives. One thinks of Keynes. The concepts are forced together in an arena where they clash. They, and the problem under study, bite at each other, and the difficulties demand resolution).

Much hangs on the word 'progress' in this description. If the analysis of 'progress' in section 4 is correct, the 'ideal researcher' could never commit the research crimes described above, which fail to produce a meaningful extension to the *scope* or *confidence* of her views. On the other hand she may soon find important *inconsistencies* in her expanding world-view. This is the price she is likely to pay for ceasing to tread the well-trodden and well-policed paths of a restricted field of study. Two related questions now present themselves. Why do so many of us researchers differ from the ideal just portrayed? And how are ideal researchers to be produced and nurtured?

When economists consider such matters of implementation they naturally think in terms of incentives and disincentives (though Frey, 1997, has wider horizons). Such solutions are systematically incomplete, since they generate a recursion: what incentive is there to change the incentives? Moreover, is it certain that a poor incentive structure is the main culprit? One could argue that researchers of the 'ideal' type pictured above have tended, historically, to win the highest prizes, in terms of professional status, money, and the intrinsic enjoyment of their work. Perhaps they still do so, but the fact is concealed by their relative scarcity. The many young researchers who choose technically demanding, dull, and unimportant research, where there are numerous competitors and few grants, may have miscalculated, or been launched by a faulty education on a misguided trajectory. Think about incentives by all means, but they are not a panacea.

Some piecemeal reforms could be made with little institutional change, yet might provide some encouragement to economists once more to disagree with each other, give and take fundamental criticisms, and be willing to struggle on in the face of real difficulties, not just technical glitches. One possibility is to replace $2n$ technical seminars by n two-person debates. Or, in place of a debate, might we schedule a Socratic interrogation? A forensically-gifted methodologist might agree to administer one; a self-confident economist might agree to submit to one. Such events might lower the audience's resistance to disagreeing with and criticising each other, starting to reverse the present fragmentation of economics. They might even be enjoyable. One other proposal: we should reward directly, with a prestigious Gadfly Prize perhaps, those who criticise current research practice, or promote heated intellectual interaction. The judges should give a high weighting to the criterion of how much scholarly anger has been generated.

Such changes are comparatively easy to make. It is less easy to affect what we have called a researcher's 'integrity', their vocational sense. One can only concur with Blaug (2002, p. 45): '[Significant change in research practice]

… will not happen unless the older members of the profession show the way with … research grounded in the attempt to confront outstanding policy issues …'. At certain times and places research integrity has been instilled by both culture and education. What would even a Keynes have achieved had not his methodologist father taught him to use all relevant approaches in trying to solve real-world problems; had not his other mentor Marshall channelled him towards circles and responsibilities in which economics was not a game, but had deadly serious consequences; had not a belief of the time been that the gifted and privileged have a duty to contribute to society?

I suspect that the reforms discussed so far, even Blaug's suggestion, can do no more than alleviate the situation, because research insincerity in economics seems to have a dug-in, institutional nature. Young economists, intimidated by their perception of the profession, are diverted into technique-driven research rather than problem-driven research, even possibly against their own long-run interests. But how (if so) this came about, and how it might be radically changed are large subjects, beyond our present scope.

In attempting to draw conclusions about economics from the main position developed in earlier sections, I have had to supplement the logical analysis with certain personal opinions. These are, however, widely held among methodologists. Moreover, they could be modified considerably and the overall conclusion still survive: that the most pressing strategic problems of economic research, as identified by many different types of methodologist, will never be solved by the 'methodology of economics', as presently constituted, which repudiates the personalistic, comparative, and institutional thinking that should lie at its heart. The insights of a reformed AM are needed.

7. Conclusions

The study of any subject, including research, can be compared with the exploration of a landscape. Methodology is needed, just as maps are needed; and AM is needed as well as the methodology of economics, just as small-scale maps are needed (providing context and the possibility of comparisons) as well as large-scale ones (providing detail).

Yet traditional approaches to AM performed the context-providing task unsatisfactorily. They muddied the distinction between logic and content. They stressed the outcomes rather than the processes of research, considered idealised and impersonal research situations rather than the actual problems of individual researchers, and allowed themselves to be dominated by the ill-chosen empiricist division between observation and non-observation. They ignored the fallible *judgments* on which research relies, and the importance of repairing and extending the logical structure of a researcher's world-view even when there are no disturbing new 'facts' to accommodate. Individual researchers came to think they could ignore methodologists, just as methodologists appeared to ignore them and their preoccupations.

Accordingly, I have argued, AM needs to be reformed radically, bringing it into much closer relation with actual research processes. From this base it could expand so as to consider the strategic problems of improving the research world. A reformed AM would not tell researchers what to do, but might suggest good questions to ask them. It could focus the attention of both economists and methodologists on the issue of progress, over a research lifetime as well as over the next project; on how researchers and research environments, as well as research, might be improved.

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Endnotes

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2. The list omits two recent types of proposal made by authors specifically concerned with economics. McCloskey (especially 1985; 1994) thinks economists right to ignore the 'big-M methodologies', suggesting that they should become more kindly disposed towards rhetoric, meaning the arts of persuasion, in economic discourse, and more expert in them. She sees rhetoric as supplying a wise flexibility, indeed humanity, absent from authoritarian and impersonal methodological systems. Here I argue that methodology and logic need not be authoritarian and impersonal, but in fact support subjectivity, flexibility, and humanity. I claim that many of McCloskey's points can be accommodated within a somewhat traditional account of methodology, stressing both logic and progress, if our conception of 'logic' is expanded. See section 4.

3. Lawson (see especially his 1994, 1997, 2003) is critical of mainstream academic economic analysis, believing that it has systematic faults springing from methodological mistakes associated with Hume's belief that science is to do mainly with 'event regularities' of the type 'whenever x, then y'. With Bhaskar (1978, 1979) he believes that a better method is to try to explain relatively transitory surface phenomena (like the complicated path of a falling leaf, or relatively low productivity growth in the UK) by identifying and elucidating their deeper causes, operating steadily (like the laws of gravity, meteorology, and aerodynamics, or the UK norms of collective worker organisation, originating before the mass-production period; see Lawson, 1994, pp. 276-278). The contentions of the present article neither contradict nor confirm Lawson's beliefs, being located at a more abstract position along the epistemological continuum. From the article's point of view, Lawson's main interest is directly in economic reality itself, whereas 'abstract methodology' tries, while deliberately abstaining from economic theorising, to analyse how such projects as Lawson's can be made as productive as possible.

4. That this claim is widely accepted by economists is documented by Roy (1989, Chapter 2). The claim seems to be one we should reject in its unqualified form: MacIntyre (1981, p. 57) discusses the following type of counterexample, due to A. N. Prior. From the factual statement 'He is a sea-captain' we can validly infer the prescription 'He ought to do whatever a sea-captain ought to do.'

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