Notes on revisiting "Methodological Prescriptions in Economics", Economica February 1959, pp60–74, by Kurt Klappholz and Joseph Agassi.

by

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ABSTRACT

I reconsider the paper 'Methodological Prescriptions in Economics', *Economica* February 1959, by Kurt Klappholz and Joseph Agassi. I criticise the thesis that 'there is only one generally applicable methodological rule, and that is the exhortation to be critical. . . ' I compare the methodology of physics with that of economics and discuss whether typical economic 'laws' are testable hypotheses. I consider whether an approach to economic policy based on welfare economics is biassed. In conclusion I sketch an approach to economic methodology and suggest some rules of method which may be useful to the working economist.

1. I welcome the opportunity to contribute an essay to the Festschrift in honour of Joske's 90th birthday, although my choice of topic is limited both by my unfamiliarity with recent work on Critical Rationalism and by health problems which at present limit my access to literature. So I have decided on an ancient, but still quoted, contribution of Joske's to a topic in economics, and at the same time make a gesture In Memoriam of my late friend Kurt.

The article under discussion was a product of the renewed interest in the methodology of economics which found expression in the well-known, not to say notorious, seminar on Methodology and Testing which ran at the LSE in the 1950s. Both K and A were members, as I was, although I was considered by the dominant faction (which did not include K&A) as heretical and was accused of such un-Popperian sins as Essentialism, Conventionalism and even Inductivism. (If I give the impression that the atmosphere of the discussion was at times coercive, rather in the style of Agitprop, that is how I remember it). The main programme of the seminar was of course to get rid of the *a*

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priorist methodology propounded by Lionel Robbins,[3] and to replace it by a Popperian[4] approach based on conjectures and refutations. These ideas are summarised in the paper under review.

2. But to begin at the beginning. K&A state their view that 'there is only one generally applicable methodological rule, and that is the exhortation to be critical and always to subject one's hypotheses to critical scrutiny. Any attempt to reinforce this maxim by a set of additional rules is likely to be futile and possibly harmful.' Taken at face value, this statement seems to deny that there is such a subject as methodology, so why read on? However, a clue that there is more to the thesis is afforded by the fact that somehow Popper's methodology seems to be exempt from the general stricture. Pursuing this line of thought, I have found the following brilliantly concise statement (albeit written at a later date) of Agassi's methodological position in his Apologia,[5] which suggests that the plea for a critical attitude may be taken as a coded invitation to adopt Popper's approach, perhaps with minor modifications.

'Please indulge me (*sic*) two paragraphs on the content of *Logik der Forschung*, since it is such a philosophical landmark. It raises two questions, discusses two traditional answers to each and presents alternatives to them. The questions: *One*: David Hume's problem of induction: how does theoretical learning from experience take place? *Two*: Immanuel Kant's problem of the demarcation of science: what theory is scientific? The inductivist-empiricist answers: *One*: theoretical learning from experience occurs when theories are backed by experience. *Two*: theories are scientific when they are backed by experience… The new criticalist answers: *One*: theoretical learning from experience occurs when theories are refuted by new experiences. *Two*: theories are scientific when they are given to empirical tests that may lead to their refutation in case they are false. By the inductivist-empiricist view science is the set of all presently empirically backed theories plus all known empirical information;… by the criticalist view science is all known refutable hypotheses and all known possible empirical refutations to them, potential and actual, true (whose truth refutes some hypotheses) and false (whose falsity corroborates some hypotheses).

This passage goes beyond my remit, which is limited to economics. However, I shall venture some amateurish remarks. Agassi's discussion is limited to empirical science, but I prefer to adhere to the dictionary definition of science as '(pursuit or principles

[3] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, 2nd ed., Macmillan 1945.

[4] K.R.Popper, *The Logic of Scientific Discovery*, Hutchinson 1959; see also *Conjectures and Refutations*, Routledge & Kegan Paul 1963.

[5] *A Philosopher's Apprentice: In Karl Popper's Workshop*, 1st Edition, Rodopi Amsterdam — Atlanta GA 1993, pp.170–171. The passage quoted is repeated with unimportant changes at p.212 in the 2nd edition, Rodopi Amsterdam — New York NY 2008.

of) systematic and formulated knowledge', and to add a qualifier when a particular science is considered. This incidentally avoids the frequently heard question whether economics really is a science. Next, as to Agassi's question 1, consider first the problem of testing a deterministic theory, i.e. one which predicts a specific outcome from an experiment. Presumably the attraction of a 'rejectionist' approach to testing is that refutation by given evidence can be regarded as purely a matter of logic, whereas a (universal) theory can never be proved to follow logically from a body of evidence, however extensive. However, it seems perverse to limit the concept of learning to episodes of rejection. Clearly learning from experience occurs all the time even when there is no stated hypothesis to be confirmed or refuted. More important, the attempt to reduce learning to a purely logical procedure ignores its subjective aspect. The observation that a given situation or experiment usually has a particular outcome creates an expectation that the same pattern will be repeated, at least if circumstances are not expected to change significantly, and in practice this expectation is usually justified. Everyday life would be very difficult if it were not so. In short, inductive learning, although subject to logical rules, is concerned with the dynamics of belief.[6] Further, it is well known that in the case of a statistical hypothesis, a finite set of data can at best yield an estimate of the probability that the hypothesis is false, or alternatively that it is true, which negates the attempt to reduce testing to an exercise in (non-probabilistic) logic; both rejection and acceptance become matters for decision, requiring consideration of practical criteria. Which brings me to my last comment under this heading, namely that the philosophical discussion of learning as summarised above seems to have little to say about the problems which face the scientist in practice, such as the selection of data and the design and interpretation of experiments.

Turning to Agassi's question 2, one wonders why a demarcation criterion should be sought at all, beyond the obvious requirement that an (empirical) science should try to explain some facts of experience. Here we seem to be concerned with the eternal human quest (so often disappointed) for reassurance, for firm ground, for *un doux et mol chevet*.[7] No demarcation for science in general is likely to be successful, because different sciences have different problems, standards and requirements. I shall return below to the question of a demarcation criterion for economics. To sum up this excursion into methodology *in general*, I venture to suggest that it offers little beyond what is evident to common sense.

[6] cf. de Finetti, *La prévision: ses lois logiques, ses sources subjectifs.* Annales de l'Institut Henri Poincaré, 7, no.1 1937, pp.1–68.

[7] Montaigne, Essais III, xiii.

3. Returning now to the article by K&A, we find the following further passage: '. . . scientific statements accepted without debate. . . are of little scientific interest. Scientific interest is aroused by controversial statements, and by the controversy surrounding them'. Again, 'the law of demand, the quantity theory of money, and similar propositions are of the utmost importance for economic policy. They are comparable, however, not to the laws of physics which are now the subject of controversy, like quantum theory, but rather to Gray's law of conductivity. . . which is obviously significant technologically. This law is as undisputed today as the law of demand, and equally manifest in everyday experience. Yet this is precisely why Gray's law is not of intellectual interest in current research in physics. Similarly, the present wide acceptability of the law of demand renders it an inadequate example of the significance of economic science. The significance of economics, as of any other science, lies in its search for new truths.' This comes pretty close to declaring that economics as a whole is not of intellectual or scientific interest, though it will hardly be disputed that economics has discovered new truths, or at least truths which were new when discovered.

Incidentally, going beyond my competence again, my impression is that quantum mechanics has aged a bit since the K&A paper was written and is now generally accepted (apart perhaps from its ontological implications) because it has survived a great variety of experimental tests. This surely does not deprive quantum mechanics of its interest for research. Indeed, because of its wide acceptance, it can be used as the basis for explaining further phenomena, e.g. the difference between superconductivity and 'classical' conductivity. Going further, I am uneasy about K&A's tendency to dismiss technological questions as lacking scientific or intellectual interest. Many, perhaps most, theoretical enquiries are suggested by problems or observations encountered in applications. Indeed theory and application cannot be rigidly separated, It is also clear that what is interesting is largely a subjective matter; a result which seems obvious to a specialist may come as a revelation to an outsider and point to new connections between different subjects. In any case we need a better criterion of what is interesting than simply what is new and not yet well understood.

4. I have the impression that the passage quoted above is not seriously about economics, but rather about physics. This suggests the need for a brief review of some of the obvious differences between these subjects which are relevant to methodology.

As a general point, it is necessary to bear in mind the distinction between theories concerning processes which are stationary in the sense that historical time plays no part — e.g. theories which are modelled by autonomous differential equations — and non-stationary processes, which include all of economic history. Einstein could cheerfully declare 'for us physicists, the distinction between past, present and future has no meaning other than that of an illusion, though a tenacious one.'[8] Indeed, in the stationary case, if the laws of motion are deterministic, then once they are fully understood, repeated observation yields no new information, provided that initial conditions are suitably adjusted. (The situation is more complicated in the case of stochastic stationary processes, where under suitable conditions a single sequence of repeated observations yields information about the asymptotic time average of the process). Suffice it to say that we economists are not in the happy situation which Einstein evokes, since every day brings numerous changes influencing economic variables, and this casts doubt on any attempt to simply transcribe methodological rules suggested by physics. In economics, the date to which an observation relates is always significant.

Some other differences must be noted, even in a brief discussion. Physics deals with an environment which is more or less constant, or at least does not respond 'consciously' to what we do; to adapt Fred Hoyle's splendid phrase, there is in fact *no* bastard in the cloud. Thus it is possible to distinguish relatively sharply between observations which agree with our conjectures and those which do not, (although there remain well-known ambiguities concerning observational errors, probabilistic conjectures and interference effects). Economics, by contrast, deals mainly with decisions by people, in particular decisions about the allocation of resources, and the interaction among the results of these decisions through markets, governments and other social mechanisms. The design and operation of these mechanisms is itself a subject for economic analysis. A further essential feature of the economic process is that, at any time, decisions about present actions are influenced by expectations of future developments, and the expectations of different actors are liable to be interdependent. What occurs is affected by what is expected to occur. (Admittedly the analysis of these effects is often 'fudged' by an assumption of 'rational' expectations or something similar, but that is essentially an acknowledgement of our ignorance about the way in which expectations are actually formed, which no doubt is a major cause of present dissatisfaction with the state of the subject). A further point is that the outcome of decisions is dependent on the particular rules governing markets and other institutions, and these rules are the results of a historical evolution which must be studied if a correct understanding of the economic process is to be achieved. Finally, the scope for repeated observations under similar

[8] Oxford Companion to Philosophy, ed. Ted Honderich 1995, page 806.

conditions is limited because history is ever-changing and irreversible.[9] Experiments in economics for purely scientific purposes are rare (outside the artificial setting of the psychological laboratory), and conditions for repetition of an experiment are liable to change if it is known that repetition is intended. So, when Max Planck said that he decided to study physics rather than economics because economics is too difficult, he clearly had a point.

5. Writers on economics often prefer to say little about the definition of their subject but rather plunge *in medias res*. Lionel Robbins clearly has some sympathy with this approach when he says, a trifle contemptuously, that 'economics is not one of those social sciences which are always discussing method before proceeding to deliver the goods'.[10] Nevertheless he devotes a whole book to an attempt to demarcate economics from related subjects and to establish a special methodology for it. I shall not review his arguments here since they are well known among economists. My view is that his effort is not entirely successful, mainly because of the great areas of overlap between economics and related problems in psychology, biology and various mathematical subjects such as statistics and decision theory etc. Besides, one wonders whether demarcations are of much use other than for departmental organisation and curriculum design. Nevertheless, Robbins has identified a core of problems which are essential to economics, namely the problems of resource allocation, which in a market economy means the theory of supply and demand and its various generalisations and applications. It seems reasonable to attempt a methodology for this area, although the differences from related fields limit any extension. I shall return to this topic below.

The article by K&A avoids any need to define its domain of relevance because it is organised as a series of disputes with earlier authors, who may be assumed to be talking about economics. Mostly these critiques are excellent, but they are rather arcane and I do not wish to review them after the lapse of more than half a century.[11] On the other hand, the lack of a constructive statement about economic methodology is a deficiency. The

[9]In this connection it is amusing to recall Poincaré's discussion (*Science and Hypothesis* Ch. IX; English: EBook edition. French original 1902.) of Carlyle's statement (*Past and Present*, Ch. 1, book II) that 'Nothing but facts are of importance. John Lackland passed by here. Here is something that is admirable. Here is a reality for which I would give all the theories in the world.' Poincaré comments that 'this is the language of the historian. The physicist (Bacon) would most likely have said 'John Lackland passed by here. It is all the same to me, for he will not pass this way again.' Where does the economist stand on this? Maybe King John's journey was the one on which he lost his treasure in The Wash, in which case it would be relevant to the economic history of England, and its effect on the money supply might call for discussion. On the other hand this unique event is hardly of significance for the validity of any law of economics.

[10]Robbins, op.cit. pp.115–116.

[11]However, one apparent error should be noted. K&A assert (p.61) that 'a law, to be true, must

authors try to combine a literal interpretation of their slogan ‘all you need is criticism’, which makes methodology an empty box, with an insistence on an (updated) Popperian methodology which is more or less a trans-scription from physics. This is inconsistent and unhelpful.

However K&A do offer some remarks about laws of economics, and it is of interest to pursue this topic briefly. Of course the concept of law is not without its difficulties — see the articles **laws, natural or scientific** and **science, problems of the philosophy of** in the Oxford Companion to Philosophy. Of the alternatives on offer, I like Ramsey’s suggestion that laws are ‘a consequence of those propositions which we should take as axioms if we knew everything and organised it as simply as possible in a deductive system’. This fits in rather well with Robbins’ characterisation of certain economic laws as ‘simple and indisputable facts of experience. . . they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious’.[12] So why not take them as axioms right away? Matters are not quite so simple, and I leave the point there for the moment.

6. I now turn to a discussion of some alleged laws mentioned in the K&A article. Recall that their discussion centres on whether these laws and related propositions are tautologies, and more generally whether they are testable.

First, the ‘Law of Demand’. This is regarded by K&A as generally accepted (and therefore boring), but they do not remind us of its content. Presumably what is meant is either (a) the ‘law’ of diminishing marginal utility, or (b) the observation that demand curves generally slope downwards. Definition (a) is common among older authors.[13] There are various problems of interpretation with this proposition and some exceptions

correctly refer to all the relevant factors (affecting its validity), which must of course be finite in number (since otherwise we could not mention them all).’ This runs counter to the common practice of lumping together in an ‘error’ term a set (possibly infinite) of ‘small’ factors which are assumed collectively to follow a particular statistical distribution. More generally, I am rather persuaded by a remark of Imre Lakatos (made in connection with mathematical theorems) that there is always an infinite number of ‘hidden lemmas’. Be that as it may, I leave the reader to whet his critical teeth on this one.

[12]Robbins, op.cit. p.78–79. To quote Poincaré again, (op.cit. Ch.IX): ‘L’expérience est la source unique de le verité: elle seule peut nous apprendre quelque chose de nouveau; elle seule peut nous donner la certitude. Voilà deux points que nul ne peut contester.’ Leaving aside whether this assertion is really incontestable, the reason for quoting it in French is that ‘expérience’ can mean either ‘experiment’ or ‘experience’. Given the physical context, the English translation naturally chooses the former rendering. However the quotation from Robbins suggests that, in economics at least, the wider concept is more appropriate for deciding the acceptability of a law.

[13]‘The law of demand is to the effect that the utility afforded by any increment of any kind of desired object diminishes with increase of the amount possessed’,W.E.Johnson in Palgrave’s *Dictionary of Political Economy*, quoted in J.N.Keynes, *The Scope and Method of Political Economy* 4th ed Macmillan 1917, p.245. Johnson regards this ‘law’ as a psychological datum rather than as a theorem of economics.

(e.g. addiction), but if suitably qualified it can reasonably claim the status of a law. Note that it does not follow from the modern theory of demand based on indifference curves unless utility is interpreted as 'cardinal' and the utility surface is strictly concave. As to (b), this does not in general follow from indifference curve analysis because it neglects the income effect.[14] Also, once one leaves the abstract world of indifference curves and allows for dynamic, speculative and behavioural considerations there are lots of familiar exceptions to (b), so that this 'law' is little more than an observation of what usually happens and is at best testable in particular situations.

The next alleged law to be considered is the law of diminishing returns. The usual distinction must be made between returns to scale and returns to a variable input, but in either case it is often difficult to say precisely what the law asserts about a particular industry or to show that the relevant convexity properties hold at some points on the production surface (however that is defined). These difficulties aside, there are plenty of examples to show that many industries operate at levels where there are increasing returns to scale and even increasing, or at least constant, returns to variable factors. The question whether this behaviour is consistent with equilibrium (apart from situations of monopoly or oligopoly) is at the root of the theory of monopolistic competition. Attempts have been made to subject this theory to testing, but it is doubtful whether it was ever meant to be testable, or whether 'rejection' would ever lead to its abandonment or serious modification. It is really an enquiry into the logic and limits of competitive models, and any attempt to make it statistically testable would probably require such distortion of the theory as to make the interpretation of tests quite uncertain. On the whole, I am inclined to agree with Neville Keynes that the law of diminishing returns should be regarded as a 'bare physical fact' about the productivity of labour applied to land, and not as a true economic law.[15]Note that in the competitive case we can consider a Law of Supply to the effect that supply curves generally slope upward, with various reservations as in the case of demand.

A third example of a possible 'law' is the Quantity of Money equation $MV = PT$. It is generally acknowledged that, with appropriate definitions of the variables, this is a mathematical identity. To make the equation into a predictive theory requires hypotheses about the behaviour of the variables. Until quite recently the usual interpretation

[14]The possibility that the income effects may outweigh the substitution effect, so that demand increases with price, is often presented as paradoxical, but it may be quite natural for some 'commodities', e.g. saving or leisure. Thus saving (=demand for future goods) may well fall as the rate of interest rises. Again, demand for leisure (=hours not worked) may well increase if the wage rate (opportunity cost of idleness) falls, i.e. if the supply curve of labour is downward sloping.

[15]J.N. Keynes, op.cit. p.85.

was to assume that the velocity of circulation V is relatively stable and that an increase in the quantity of money M would lead to an increase in transactions T until capacity constraints were reached, with a proportional increase in prices P thereafter (with no doubt an intermediate phase to allow for aggregation effects). With this interpretation we seem at last to have an example of a testable theory. And recent experience suggests a decisive rejection. Broadly speaking, central banks have created enormous quantities of cash, but rather than stimulate trade or raise prices the additional sums have largely been added to idle balances. This is not the place to discuss the causes of this behaviour, which are in any case familiar from the financial press. The point of interest here is that we have a testable hypothesis and a decisive rejection, at least in the short run; (over a longer period the vast accumulation of liquidity may yet lead to hyper-inflation, with the traditional interpretation of the Quantity Equation coming into its own). Of course the experiment with ultra-loose monetary conditions has not been undertaken in the interests of science, and it is amusing but sad to note that the failure of the policy has not led to its abandonment but (at least in some cases) to a feverish quest for ever more extreme measures in the same direction (negative interest etc.). From the methodological point of view, the natural inference is a rejection of the hypothesis of constancy of V for the period in question, but not a rejection of the Quantity Equation, even though formally the equation is being tested along with the hypotheses about V and the other variables. This turns out to be typical of the interpretation of tests where an economic law is involved.[16]

The economic laws considered so far have in common that they are basic building blocks for economic models yet, when correctly stated and hedged about with reservations, offer little scope for useful tests of general interest. As Robbins suggests, they represent more or less obvious facts of experience. The discovery of situations in which the laws seem not to hold does not lead to 'rejection' of the laws in general, but rather to the characterisation of these situations as exceptional, possibly because some parameters appearing in the laws take unusual values.

There are other examples of regularities which are so obvious once recognised that testing seems irrelevant (apart perhaps from measurement of parameters, which can be regarded as testing although such a presentation yields no special insight). An example is the demonstration that banks create money (and do not merely circulate a fixed amount

[16]This procedure may be regarded as comparable to the usual interpretation of the results of an experiment which seems to violate the law of conservation of energy, cf. H. Poincaré, op.cit. esp. pp.185 and 159–160, also the articles **Poincaré** and **conventialism** in the Oxford Companion to Philosophy.

of currency). Given a description of the way that banks operate, a very little algebra puts the matter beyond doubt. Of course it is necessary to state some assumptions, e.g. to specify reserve ratios and that banks can find customers to borrow their available funds. Similar remarks can be made about the national income multiplier.

7. One begins to wonder whether there are any non-trivial hypotheses in economics which follow the Popper/Agassi paradigm. Well, there are, up to a point. Consider for example Karl Marx's prediction that capitalism would lead to the progressive impoverishment of the proletariat (I cannot bring myself to say 'immiserisation'). It appears to be a consequence of a serious theory, namely Marx's Labour Theory of Value, and it is a bold, testable hypothesis. There are of course ambiguities and reservations, e.g. concerning the disappearance of much of the proletariat as Marx knew it, and the question whether the hypothesis refers to absolute or relative impoverishment. On the latter interpretation, the experience of recent years offers some confirmation of the hypothesis. (This might be regarded as a pity from Agassi's point of view, since rejection allegedly yields the best learning). However, the hypothesis relates to a unique historical sequence of events which cannot be repeated, and moreover a different result might be obtained if a different period were considered; clearly it would not be satisfactory to alternate between 'acceptance' and 'rejection'. It seems that, in the case of a historical process, the language of conjecture and refutation, while not incorrect, is not very helpful. Better insight can be obtained by comparing the evolution of variables of interest — say, the share of labour in the national product — in various sub-periods, relating this evolution to explanatory variables in each period, and perhaps using this analysis to form conjectures about future trends. The emphasis in such work is on the collection and interpretation of data to obtain a correct record of events and the inter-dependence of changes in the relevant variables in different periods. In short, the emphasis is on 'understanding' what has actually occurred rather than on conjectures about what might have occurred under different conditions. (Naturally I avoid defining 'understanding', just as K&A avoid defining 'criticism').

8. This discussion, while rather unsystematic and incomplete, does prepare the ground for a sketch of a possible 'landscape' of economic methodology. Briefly, I see economics as comprising two main related parts, called (economic) theory and (economic) history.

Theory comprises (mathematical and other) models, which may be deterministic or stochastic (or perhaps of other types). Models are abstract, i.e. devoid of empirical content, and deductive. They are identifiable as 'economic' models by the names of the variables appearing in them (prices, money etc.) and by the types of relations postulated among the variables. Specification of a model typically includes a statement of some structural conditions, e.g. assumptions about preferences, production possibilities, market structure etc., which usually incorporate one or more 'laws'. Usually there are also assumptions about 'input' variables, in particular a statement about initial conditions. From these input variables and structural conditions, relations among 'output' variables are derived by logical inference. These relations may be indexed by time, also by 'state of nature' in the case of stochastic models. The inferences will usually assert relations among the output variables, and possibly also (theoretical) predictions of future values.

History comprises all relevant empirical data, such as records of events and statistical series, and even forecasts of future events. It is appropriate to stress the importance in economics of the routine work of collecting and preserving data, without which there cannot be adequate explanation of events. We do not usually have the possibility of filling in gaps by repeating experiments, nor can we rely on light reaching us across the vastness of space to tell us what happened long ago. This is not to belittle the role of imaginative reconstruction and even folk memory, but reliably recorded facts are better.

Given the sets of models and history, we consider *explanations* (of historical data by means of models). Much — maybe too much — has been written about the nature of economic explanation, notably in the nineteenth century *Methodenstreit* which opposed the German historical, or inductive, method to the English abstract, or deductive, approach.[17] The following sketch tries to avoid this dichotomy.

Suppose that we are interested in the development of a certain collection of (possibly random) economic variables during a given period. We are given the historical record up to the present. We suppose that our stock of models contains at least one which formulates hypothetical relations among the variables in question (strictly, among the theoretical variables which have the same names as the empirical variables for which we have records). Given past and current observations of the variables, we use our model to formulate theoretical relations among the variables and to generate theoretical predictions for a suitable future period. If in due course the observed values of the variables are found to agree adequately with the theoretical relations and predictions, we are happy and go on to the next job. If not, we modify the model, usually by altering some parameters or introducing new explanatory variables. Complete rejection of the

[17] For a brief survey, see J.N.Keynes, op.cit. Ch. I.

model is unusual since, as we have seen, the building blocks of economic models usually include 'simple and indisputable facts of experience'. Nevertheless models are sometimes abandoned and replaced by new ones when they are considered inadequate, perhaps because of new insights or due to changes in the economic environment.[18] Whatever the extent of agreement between prediction and observation, the learning process will involve recording the results, reviewing the adequacy of the data and the model, and no doubt repetition of the procedure for a further period. A trivial and obvious account, but one with a very different emphasis from the Popper/Agassi version of scientific learning.

9. Much ink has flowed over the question of the 'value freedom' (*Wertfreiheit*) of economics. Note that the preceding discussion relates entirely to 'positive' economics, where the answer appears clearly in the affirmative. But freedom from value judgements does not imply freedom from bias. As all students know, economic theory is largely organised around a number of simplified benchmark models — perfect competition, monopoly, Keynesian macro etc. — and these serve as the basis for the analysis of real-world situations. This is essential because of the complexity of the economy and the great variety of practical situations. However the habit of treating real situations as departures from ideal situations does introduce a bias into the analysis, which can become exaggerated if the structure of the economy changes while the benchmarks remain the same. (Why do we have to spend so much time learning about perfect competition when there is so little of it about?) Now this bias can lead to the implicit introduction of value judgements when the discussion turns to political economy, i.e. to economic policy. It arises particularly in applications of welfare economics, with its optimality theorems which establish a correspondence between competitive equilibria and Pareto optima. (Note that these are existence theorems, not practical recipes for finding the income or wealth distribution making a given optimum into an equilibrium; but often the distinction is blurred). The presentation of the theorems sometimes creates the suggestion that welfare for all can be improved by implementing a competitive regime and then moving to an appropriate distribution; which in practice is not possible, not least because the necessary information is lacking, and certainly not if the necessary redistribution is not actually undertaken. One is often left with the impression that 'competition is best' because it will lead to an optimal allocation of resources, never mind the possible distributive consequences. The so-called compensation criteria involve similar sources of bias if compensation is not actually paid. Besides, adoption of a particular measure on the grounds that the gainers can over-compensate the losers may pre-empt alternative measures which are even more

[18] Compare the discussion by Poincaré, p.160.

desirable. Thus in practice the application of results from welfare economics tends to become a vehicle for the promotion of 'free market' policies which can be represented as having advantages for the allocation of resources while neglecting their distributive consequences. What would help to validate this approach would be a sort of law of large numbers for policies satisfying 'allocative' criteria, i.e. a result showing that repeated use of such policies would eventually lead, at least with high probability, to allocations making almost everyone better off. But I am not aware of any such result and an attempt to establish one might just add to the list of failed Ph.D.s.

10. I now return to the original issue of the existence of useful methodological prescriptions in economics. I have so far left aside the qualification implicit in K&A's assertion that 'there is only one *generally applicable* methodological rule. . . ' This qualification unfortunately makes their thesis quite ambiguous. May we not seek further rules of more limited applicability but perhaps of greater practical use to the working economist if not of great interest to the visiting philosopher? Such rules, I suggest, are embedded in the prolonged training which we give to students before admitting them to the ranks of qualified economists. After all, any teacher knows that adolescents are usually endowed with plenty of critical attitude, but that alone does not equip them to be researchers or policy advisers. Since acquisition of knowledge of appropriate methods is inseparable from the process of study as a whole, it is not possible in a short note (and perhaps not at all) to make a complete list. But in any case, the box of rules is far from empty. The more specific the problem under discussion, the more detailed are the available rules, though of course they are subject to criticism and revision. But even at a rather high level of generality, there is useful advice available. To give a few examples, consider the following:

(a) Consider separately the determinants of demand and of supply when analysing price formation, distinguishing the periods over which the various factors operate.

(b) Accompany any point forecast with an estimate of its uncertainty, or give a probabilistic forecast.

(c) When devising a policy, remember that people respond to incentives, often in undesired ways; for example, price controls are liable to create shortages and black markets. More generally, watch out for feedback and other systemic effects of a policy designed to bring about a specific consequence.

(d) When framing recommendations for policy, try to separate factual analysis of the consequences of action from value judgements.

(e) Be wary of formulations of policy problems which tend to predetermine the outcome of analysis, for instance comparisons of an actual situation with a standard defined by an idealised situation such as perfect competition, particularly where no actions are available to mitigate undesired side-effects such as redistribution of income.

(f) Economic experiments in the real world (as distinct from the psychological laboratory) are not carried out primarily in pursuit of theoretical knowledge, but rather for profit or to influence the behaviour of an economic system, and outcomes may be ambiguous and take a long time to appear. Besides, decision-makers commonly have various, often not clearly specified, hypotheses in mind. This usually makes the classification of a particular outcome as rejection or confirmation ambiguous.

Some of these proposed rules are so well established as to appear trivial, others are more difficult to apply or controversial, but all address situations which commonly face the working economist and which have sometimes been neglected with unfortunate results. Many other examples could be given. It is a boring list, and for my pains in drawing it up I have been compared to Polonius. But no matter, if I have shown that useful methodological advice can be given. Criticism is no doubt necessary, but not sufficient.