Abstract. This paper identifies four criteria used by mainstream economists of trade unions to judge the adequacy of their various theories. Three of them, however, are hardly ever satisfied. The root cause of this failure is the method adopted by mainstream economic theory, namely deductivism. The perspective of critical realism identifies and explains the cause of this failure and points towards an alternative method.

Introduction:

According to Oswald: "Those who wish to construct analytical models of the unionised economy must first find a plausible microeconomic theory of trade union behaviour" (1982: 526 emphasis added). Layard et al (1992: 86) argue for "sensible" models, and Booth (1995: 83) for "tractable" models, whilst the terms "adequate", "useful", "valid" and "satisfactory" also appear in the literature. Since terms such as these all appear to express a similar sentiment, there is obvious utility in fastening upon one of them, and I opt (because it suggests generality) for the word adequate. If theories of trade union behaviour are required to be adequate, then something like the following questions need consideration:

1. What criteria are commonly adopted to judge the adequacy of various theories of trade union behaviour?
2. Do these theories satisfy the adequacy criteria adopted?
3. If not why not?

Assuming one does not want to produce an inadequate theories, one must consider, and perhaps have answers to, something like these questions - even if one declines to go into print on such matters. And such considerations and/or answers involve methodology. Methodological issues appear to worry thinkers like Pencavel, who notes the literature on the economics of trade union behaviour is replete not only with "plausible", but also with a good many "implausible" theories (1994: 190). He is also concerned that economics in general is becoming dichotomised into "toy models" (pure theory) on the one hand, and correlation without theory or explanation on the other (1991: 84). I too am concerned that the literature on economic models and theories of trade union behaviour reveals little or nothing about the reality of trade union behaviour. At the very least, the possibility must be countenanced that these theories and models are inadequate. And this raises a problem. Because this same literature carries no discussion whatsoever of methodology, there is no obvious basis upon which to differentiate plausible or adequate theories from implausible or inadequate ones.

The objective of this paper, therefore, is to bring to the attention of those economists worried by the current state of
trade union economic theory, a set of debates that are currently taking place within the discourse of methodology. It consists of four parts. The first two parts address the first two of the questions previously noted. After identifying four criteria used to judge the adequacy of their various theories, it reveals how three of them are hardly ever satisfied. The paper changes tack in parts three and four to demonstrate that the reason for this failure lies in the method adopted by mainstream economic theory, namely deductivism. Critical realism identifies and explains the cause of this failure and to points towards an alternative method.

1. Criteria for assessing adequacy
A survey of the literature identified four criteria that are commonly used to judge the adequacy of various theories of trade union behaviour. They will be introduced in turn.

1.1 Predictive power
A theory is often said to be adequate if it generates predictions that are borne out, validated, verified, confirmed, not falsified or some such, when tested against reality. Booth (1995: 83) probably speaks for most economists by describing “the accepted methodology of economics” as a loosely defined amalgam of instrumentalism and falsificationism whereby simplifying assumptions are used to formulate theories and generate a prediction. Prediction assumes two forms: (i) as a statement about future events, that is, a forecast; and (ii) as a statement about events that have occurred, but observations on which have not yet been made (Friedman 1988: 214). In the latter case, theory generates a prediction about what might be expected to occur under certain circumstances. It is this interpretation of prediction that most economists of trade unions appear to use (Cf. Booth 1995:6) and that will be used throughout this paper.

1.2 Realisticnes of assumptions
Assumptions are theoretical devices used to buttress axioms by bounding or narrowing down the field of inquiry. For example, the axiom that unions are rational maximisers, might be buttressed by the assumptions: (a) that they maximise utility; and (b) that utility is derived from wages and employment. Whilst it is often difficult, it is not impossible, to differentiate between an assumption that ‘locks on’ as it were to some real feature of reality, and one that is merely a convenient fiction. Many commentators appear to believe a use of realistic assumptions is a criterion with which to judge the model's adequacy. For exa

1.3 Correspondence with reality
To suggest an adequate theory is one that corresponds to reality is not to imply the theory be isomorphic with reality. Moreover, there is no a priori meta-theory about how one constructs a theory that corresponds to reality. Whilst it is often difficult to say what constitutes correspondence with reality, it is not impossible. A theory that corresponds with reality might be one that (a) singles out certain elements of the phenomena under investigation on the grounds that they are essential to the existence or operation of this phenomena, and then (b) re-creates these elements ideally to form a theoretical category. For example, an investigation of the trade union/non-union wage differential might necessitate the singling out of certain bargaining processes and the construction of a suitable set of theoretical
categories. A theory that is to correspond with reality has to use these categories to illuminate the processes at work. Whilst proceeding in this manner does not, of course, guarantee a theory that corresponds to reality, compare this to a procedure that recognises the importance of these processes, and then consciously disregards them. In this case, lack of correspondence is virtually guaranteed. In the literature, an adequate theory is often presumed to be one that "corresponds" to, "reflects", "captures", or even "describes" certain aspects of, reality. Pencavel views "economics as a science which requires...a meaningful interdependence between theoretical models and real world experiences" (1994: vii). McDonald and Solow (1992: 93) even go as far as claiming that: "[t]he contract curve probably has some approximate descriptive value".

1.4 Explanatory power
Most economists appear to value a theory that explains. Whilst Parodi, 1989: 93) it is equally true that most economists employ the notion of "explanation" without further comment or reflection. Nonetheless, there is what Hausman (1992: 288-9) claims is a "dominant view" among economists, and I see no reason why this does not apply to economists of trade unions. This view employs the deductive-nomological [DN] model of scientific explanation" (ibid) whereby a statement is explained when the following conditions are satisfied:

a) When and if it is deduced from a set of true statements which specify the initial conditions and must include a law.

b) When and if it satisfies the intuition of non-contingency. That is, the thing being explained does not just 'happen', it was the sort of thing that ought to be expected in the circumstances.

c) When and if it satisfies the intuition of causality. That is, the initial conditions are the causes of thing being explained.

A model that generates statements satisfying these conditions might be said to possess explanatory power.

Having identified the four criteria employed by economists of trade unions to judge the adequacy of their theories, it should not be supposed that these criteria are employed in a mutually exclusive manner. Most of the authors cited thus far, implicitly adopt two or more.

2. Do trade union theories satisfy the adequacy criteria?
This section marshals a series of observations to demonstrate that three of the four adequacy criteria are hardly ever satisfied. In part it turns on the confusion that appears to be the outcome of a literature that, whilst making use of methodological presuppositions, never subjects them to scrutiny.

2.1 Predictive power
Ascertaining whether the general predictions arising from trade union theories are borne-out by empirical data is far
from straightforward. In the light of no single compelling argument, I offer a compendium of four arguments designed, not so much to convince outright, as to sow seeds of doubt about the suitability of this criterion.

i) Reflecting upon general sentiments
In order to streamline matters, I will make use of Pencavel's (1994) and Booth's (1995) surveys of various facets of union behaviour such as the effects of unions on: wages (Booth: 157, Pencavel: 29); productivity levels (Booth: 194-200); productivity growth (Booth: 199-200); investment (Booth 210-11); profitability (Booth 215); employment (Booth: 218, Pencavel: 42); and cyclical adjustment (Pencavel: 51). Reading their concluding comments on each of these issue, one gets an overwhelming sense of how little confidence they have in predicting the effect unions have on these economic variables. Similar sentiments appear elsewhere. Stewart (1987: 140) admits that "existing micro-level studies only represent a first step, however, and in some cases their evidence is hard to interpret". Ulph and Ulph (1990: 102) are forced to admit that neither the right-to-manage model nor the efficient bargain model they are commenting on, "seems to account satisfactorily for the data". This is quite a devastating confession since virtually all economic theories of trade unions utilise one or other of these models. Denny and Nickell (1992: 884-5) recognise that their predictions "are, of course, subject to wide margins of error given the number of extraneous assumptions required to generate them".

ii) Isolation and quantification
A key problem facing any attempt to test a theory empirically is how to isolate the factors included in the model from the multiplicity of factors that, for purposes of tractability and quantification, have to be excluded. This problem is exacerbated when one knows full well that many of the excluded factors are likely to be influential. Many excluded factors are socio-political or even ideological in nature and, therefore, resistant to (meaningful) quantitative measurement. Consider an example:

In the construction of a model of a unionised labour market, it is necessary to consider the objectives of the two principle actors - the trade union and management...It seems reasonable to suppose that the government's behaviour is exogenous (Booth 1995: 83).

Appeal to government "exogeneity" is, arguably, a smokescreen for legitimising the exclusion of something that is extremely difficult, perhaps impossible, to (meaningfully) quantify. There can be little doubt that various struggles between the "two principle actors" have influenced, and in turn been influenced by, a series of actions by another "principle actor", namely the government. Moreover, who would doubt that some of the most important factors, in terms of economic effects of and on unions, have been the completely unquantifiable forces of political ideology? It is most unlikely that dummies, proxies, or any other technique for constructing (meaningful) quantification

The related processes of isolation and quantification, then, appear to blend and metamorphose, imperceptibly, into a process that boils down to simply ignoring those aspects of the social world that are problematic vis-a-vis empirical
testing. And this has a damaging implication that is not, generally, noted.

Even if the included explanatory variables generate a prediction that is successful (at least for some restricted space-time region) the success is likely to be short lived. The very success of the theory in respect of its predictive power, would serve to highlight the weakness of the theory in another respect. The successful prediction (say) of a union/non-union wage differential coupled with an open acknowledgment that many relevant causal factors are excluded from the model, is an open admission that many aspects of this differential remain unexplained and unexamined.

Stewart successfully predicts the existence of wide variations in the union/non-union wage differential across industries. He is, however, aware of a weakness in the general approach to the study of wage differentials, suggesting that "there has been little systematic investigation of the variation" in wage differentials across industries (ibid: 111). Comments about the "explanatory power" of his equations not withstanding, it is clear that many factors that might account for the variation in wage differentials remain unexamined and unexplained. When he does offer an explanation of the variation, he does no more than touch the surface by offering (not entirely uninteresting) conjectures about skill-related issues, and industry and unionisation anomalies (ibid: 120-21). These conjectures do not, however, constitute a serious explanation or examination of the variations.

To be fair, Stewart claims not to be offering a "systematic investigation of the variation" (ibid: 116). However, should he (or others) actually want to examine and explain the source of this systematic variation, the deductive method will not be suitable. In order to isolate the model for the purposes of tractability and empirical testing, many factors known to be influential have to be excluded from the vector of explanatory variables. And once they are placed out of bounds, as it were, they cannot subsequently be recalled as part of an in-depth examination and explanation. Because of the exclusion of influential factors, the theory is unable to render intelligible any predictive success it might enjoy.

Whilst it is doubtful that Blanchflower would agree with my reasoning vis-a-vis isolation and measurement, it does give some grounds for the following comment:

Relatively little progress has been made on the issue of how labour markets work...Instead the focus has been on empirically valuable but conceptually narrow matters of economic measurement" (1991: 483. See also 1994: 24,30).

iii) Nature of testing

Whilst the criterion of predictive accuracy is widely employed, and based upon some loosely defined amalgam of instrumentalism and falsificationism, economists rarely subject their theories to the kind of empirical testing that this method warrants. Consider how theory ought to be tested from this methodological perspective.

1. A model would be specified and a theory tested using standard econometric techniques.
2. The theory would then be re-tested using the same model specification at some later date. This would allow the inclusion of more data, and reveal whether or not a prediction that held for one time-space region continues to hold for some larger region.

3. The theory would then be tested, and re-tested using the same model specification by other economists.

Many papers have a section where they compare their findings to previous studies. Main and Reilly write of "general consensus" provided by studies on wage gaps, and of estimates being "dimensionally comparable" (1992: 49). Similar predictions are subsequently used as confirmation that the theories are, at the very least, on the right lines. Blanchflower even goes as far as arguing that one of the strengths of research using cross sectional data (on pay) is that "its chief findings have been replicated countless times", adding that its adherents "stress its scientific credentials" (1991: 483 emphasis added). Scientificity, in other words, appears to be linked to replicability.

Let us consider some examples taken from Booth's (1995: 165-6) compendium of British union wage-gap surveys and see if there is any evidence of replicability and hence scientificity.

* Shah (1984) uses individual data to find: manual males have a mean union mark up of 10-13%

* Blanchflower (1984) uses plant level data to find: semi-skilled manuals have a mean union mark up of 10.2%, whilst the figures for unskilled manual, clerical and middle manager are -0.4% (insig.) 0.7% (insig.) and 4% respectively.

* Blanchflower and Oswald (1990) use private sector plant level data to find: clerical females have a mean union mark up of 3% (insig) in 1980, 3% in 1984, whilst the figures for middle managers and foreman/supervisor are 3% and 1% (insig) respectively.

* Main and Reilly (1992) use the SCELLI data to find: full-time females have a mean union mark up of 14.6% whilst the figure for part-time females is 15.3%.

Whatever the merits of these studies considered individually, one thing is clear: they are not attempts at replicating each other's findings. Mirowski and Sklivas (1991) observe that most economists do not "replicate" the findings of others, rather they "reproduce" them. Replication involves subjecting a theory to the same empirical tests, using the same data source and the same model to see if the original prediction: (a) can be generated again (b) remains valid when additional data is added. Reproduction involves subjecting a theory to the same empirical tests, but with a data source and model that can be quite different from the original.

Now there are a few instances where something like replication is attempted. Machin et al (1993) use the same data set (WIRS, 1984) and model as Stewart (1991) to estimate union/non-union wage differentials in 1984. Although
Machin et al's study replicates Stewart's, it must be said that this is only one replication of research on one particular data set. When studying union/non-union wage differentials:

- Stewart (1987) uses data from the 1980 WIRS
- Stewart (1991) uses data from the 1980 and 1984 WIRS
- Stewart (1995) uses data from the 1984 and 1990 WIRS

Whilst it appears that he is engaged in a process of replication, even Stewart who is wary of these pitfalls, fails to replicate for two reasons. First, there are problems of comparability of the data: (a) between 1980 and 1984 (Stewart 1991: 159-60, especially footnote 12); (b) between 1984 and 90 (Stewart 1995: 159-60); and (c) between 1980, 1984 and 1990 (Gosling and Machin 1995: 178).

Second, and more importantly, the changing nature of reality forces Stewart to re-specify the model he uses on the data sets of 1984 and 1990. So, whereas the same model is used on the data sets of 1980 and 1984, a different model is used on the data sets of 1980/84 and 1990. The ending of closed shop arrangements in 1984 necessitates a change in the vector of explanatory variables used. In addition, his 1995 model contains an explanatory variable indicating whether or not the establishment belongs to an employers association. If the later models do not use the same explanatory variables as the earlier models, then the later studies, whatever their merits in other areas, do not replicate the earlier ones. The original study is a one-off and remains un-replicated. It would be rather like a natural scientist conducting a one-off experiment, claiming to have discovered nuclear fusion in a test tube, and never having this claim investigated by having his or her experiment replicated.

In short, it appears that whilst economists employ the criterion of predictive accuracy to judge the adequacy of a theory, they rarely subject their theories to the kind of empirical testing that their method warrants. Moreover, the way economists actually proceed makes it is extremely difficult to know if one study bears out or rejects the findings of another. This is replicability between studies, not necessarily the small number of studies per se, that is the source of the problem.

iv) Prediction in theory and practice.

Consider two typical examples of the kind of predictions generated by trade union economic theory.

The wage chosen in any one sector of a multi sector union varies inversely with the level of membership in the other sectors (Pemberton, 1988: 762).

The elasticity of substitution between employment and real wage...is 1.34. This means that on average, the union is willing to trade a one percent increase in wages for a 1.34% fall in employment (Forslund, 1994: 87).
Taken out of the context of an academic paper, and placed in a realistic context, (even ignoring the actual magnitudes as spurious precision) I submit that very few would be prepared to stand by the following predictions: the wages of UNISON (a UK trade union) nurses will vary inversely with the level of membership of UNISON porters; or that UNISON will trade the employment of nurses against an increase in nurses wages.

This compendium of four arguments has, I feel, sown seeds of doubt in what is normally, if implicitly, presumed to be the rather straightforward procedure of empirically testing a theory via its predictions. The various predictions appear: not to be held with any great confidence by those in the field; to omit many factors known to be influential; to be ensnared in problems raised by the meaning of empirical testing; and to look implausible when placed in a realistic context. There do appear to be grounds, therefore, to support the claim that many economic theories of trade unions do not satisfy the adequacy criterion of predictive power.

2.2 Realisticness of assumptions

Layard and Nickell reject the efficient bargaining hypothesis that unemployment is lower when unions bargain over employment and wages. Although their polemic appears to be technical, it is, upon close inspection, methodological. Advocates of this hypothesis are said to characterize the bargaining environment external to the firm incorrectly, resulting in "false" and "limited propositions" (773). The cause of the incorrect characterization is that the "environment outside the representative firm is assumed to be the same whatever bargaining scheme is in operation" (ibid). Layard and Nickell's aim is to characterize the external environment correctly thereby avoiding the problem of "eliminating a key feature of the real world from our model" (780). Whilst they never state explicitly that the assumptions used by advocates of the said hypothesis are unrealistic it is difficult to know how else to describe them. If assumptions "eliminate features of the real world", then these assumptions can legitimately be described as unrealistic in the sense that they do not 'lock on' to features of reality.

Whilst, however, they are prepared to criticise others over their use of unrealistic assumptions when dealing with the bargaining environment, they feel no compulsion to extend this criticism when dealing with other issues. In fact, the rest of their argument is based upon patently unrealistic assumptions. For example, their results:

are based on monopolistic competition in the product market, with a fixed number of firms and constant elasticity product demand curves...Capital is fixed. Unions are assumed to be utilitarian. There is Nash Bargaining (776; see also: 779, 782, 785).

Moreover, they use all the usual technical devices and theorems that are based upon various other (unstated) unrealistic assumptions - e.g. rational agents, equilibria, well behaved supply, demand and indifference curves, marginal productivity theory, and so on.

Caruth and Oswald (1987) leave one in no doubt that they value some form of realisticness of assumptions (and
correspondence with reality), writing variously of: “adequate effects”, “adequate assumptions”, “inadequate results”, “correct and incorrect intuitions”. They even suggest that “it seems preferable, for the sake of realism, to continue to use [a] conventional assumption” (ibid: 433). Yet throughout the rest of their paper they employ assumptions that are patently unreal, such as: the union has utilitarian preferences; an industry in which one large union negotiates with one employer; firms maximise profits; two agents together fix a Pareto optimal bargain; the union is so strong that it can force the firm to accept the profit level regardless of the strength of product demand...and so on.

Unwilling or unable to practice what they preach, it is difficult no to conclude that their claim to employ assumptions for “the sake of realism” is a red herring. Indeed at one point (ibid: 433) they are prepared to violate an axiom of rationality “for the sake of realism”, yet several pages later are prepared to do the exact opposite, namely violate reality for the sake of using the axiom of ”selfishness” (ibid: 441).

There do appear to be grounds, therefore, to support the claim that the assumptions employed in many economic theories of trade unions do not satisfy the adequacy criterion of employing realistic assumptions.

2.3 Correspondence with reality

Kuhn (1988) considers two “limiting features” of general equilibrium models of unionism. “First, they typically consider a world where all firms in an industry are identical in underlying productive characteristics...Second, the exit and entry decisions of firms” are grossly simplified. His comment that this “renders the analysis less realistic than it needs to be”, suggests a concern for realisticness. No sooner has he said this, however, than this concern evaporates:

I consider an economy that lasts two periods, has no capital, and has a fixed population of risk-neutral individuals [who] possess publicly known endowments of two factors of production: simple labour and entrepreneurial skill... [I]ndividuals make an ‘occupation choice’ decision in the first period, which determines whether they will work or manage a firm in the second period.

He then adds:

*simplicity and realism are both added* to the model if we assume that, in the second period...workers cannot decide to ‘set up’ a firm (Kuhn (1988: 63-4, emphasis added).

Kuhn’s model might be simple, but it is difficult to describe it as having any correspondence with reality.

According to Pencavel (1994: 14-5), work on union-nonunion differentials is essentially a “conceptual experiment” where the values in unionised firms are compared with the values taken by the same variable in nonunionised firms *ceteris paribus*. He then adds: “[n]eedless to say, this is not how the world works”, before going onto give some examples of how the world really does work, and the problems reality raises for empirical work. A few pages later he
adds that reading the literature one would not learn a great deal about the real labour market because not only has "the
literature been one of measurement without much underlying theory, it has also been one of measurement with little
reference to the institutions associated with the observed wage effects"(ibid: 24). But Pencavel cannot offer a way
around the problems he raises and ends up engaging in "conceptual experiments", thereby using categories he knows
do not correspond to the real world, whilst giving the continual impression that he values such correspondence.

Farber's frankness on the issue of tractability versus correspondence with reality is refreshing:

    The attractiveness of the...models presented thus far is not their congruence with the operation of
actual labour unions, but it is the ease with which these models can be operationalised (Farber 1986: 1082, see also 1040).

What correspondence to reality requires, in essence, is the appropriate use of the process of abstraction. As Marx once
put it: "In the analysis of economic forms, neither microscopes nor chemical reagents are of use. The power of
abstraction must replace both" (1983: 19). Unfortunately, virtually no economists take the process of abstraction
seriously. Instead of reflection upon, and development of, this crucial epistemological processes one finds evasion,
typically, in the form of the truism that all theory must abstract - Cf. Booth (1995: 82). This does not, however, license
the use of pure fiction - unless, of course, one takes instrumentalism seriously, which most economists do not. What is
usually (illigitimately) referred to as a process of abstraction, is often little more than the invention of convenient fictions
or idealisations, whence the process of abstraction becomes a fig leaf to cover methodological nakedness. Without a
legitimate process of abstraction in the early stages of category formation, there is little chance of constructing a theory
that corresponds to reality.

There do appear to be grounds, therefore, to support the claim that many economic theories of trade unions do not
satisfy the adequacy criterion of correspondence with reality.

2.4 Explanatory power

Using the DN model, Parodi focuses upon the individual, the union and the firm, characterising them by the usual
axioms, assumptions and laws. Parodi's model satisfies the three conditions noted in part 1. Condition (a) is satisfied
because concluding statements can be deduced from the initial conditions - e.g., a 5% reduction in the number of hours
worked causes a 4.39% reduction in union membership. Condition (b) is satisfied because statements can be
interpreted as non-contingent - e.g., it is sensible to expect a reduction in the number of hours worked to reduce union
membership. Condition (c) is satisfied because these statements can be interpreted as causal - e.g., the introduction
of labour saving technology increases fears of unemployment, causing workers to apply for union membership. Parodi's
paper is not a carefully selected exception; virtually all trade union theories employ the DN model and virtually all such
models satisfy the three conditions. There do appear to be grounds, therefore, to support the claim that economic
theories of unions do satisfy the adequacy criterion of explanatory power.
It cannot pass without (brief) comment, however, that the ability to satisfy this criterion is due to the exceptionally weak conceptualisation of explanation used in the DN model. A more stringent version would see these theories fail drastically. This issue will not be pressed because the objectives of the paper do not extend to discussing the strengths and weaknesses *per se* of the various criteria chosen by mainstream economists themselves. For an elaboration of this issue within the methodology of economics, see Blaug (1987: 2-9) and Runde (1996). For an elaboration within the philosophy of social science, see Bhaskar (1978, 1989); Lipton (1991); and Ruben (1992).

3. Philosophy and methodology underlying trade union theories

3.1 The challenge of critical realism

Whilst most economists are aware that they adopt an *epistemology* (a theory of how knowledge of reality is possible) they are not usually aware that they also adopt an *ontology* (a theory of the nature of that reality). One has no option but to choose some concepts with which to think about the world, and whatever concepts one chooses already involves ontological presuppositions. For example, if one chooses to think about society in terms of individuals that, when aggregated, constitute society, one has already presupposed an atomistic ontology. Ontology has, however, been neglected as philosophers of science and methodologists (especially those writing on economic methodology) throughout this century have concentrated upon epistemology. *Critical realism (CR)* redresses this imbalance by placing ontology firmly on the agenda.

Lawson, a leading CR's argues that the dominant, mode of theorising consists in the application of a particular method, namely *deductivism*, often in the guise of the DN model noted above. From the CR perspective, the deductivist method stems from the philosophy of *empirical realism* (ER), and particularly the ontology it presupposes. ER may be summarised as follows:

1. Although ER prioritises epistemology, ontology is not banished. ER conceals an implicit ontology consisting of the objects of sense experience. Reality is reduced to knowledge about what is experienced.

2. What is experienced are unique, unconnected, atomistic episodes or events. These events cannot be other than atomistic, since any connection or relation between them is impervious to experience. Ontology is not only of sense experiences, then, it is also atomistic.

3. If *particular* knowledge of reality is gained through experiencing events, then *general*, including scientific, knowledge must be of the constant patterns, if any, that these events reveal.

From the ER perspective, scientific (including economic) knowledge is completely reliant upon the existence and ubiquity of constant patterns, regularities, or *constant conjunctions, of events*. A law is, therefore, a statement about a constant conjunction of events, whether observed, conjectured (in the case of pure theory), deterministic or
probabilistic. Such a law may be styled: ‘whenever event \( x \) then event \( y \)’, although it is often represented in the form of a functional relation such as: \( y = f(x) \). Laws of this kind, encouraged as they are by Hume's notion of causality are referred to as Humean.

The deductive method favoured by orthodox economic theory, relies crucially upon the existence and ubiquity of constant conjunctions of events in or form or another. Herein lies the crucial flaw: CR’s, maintain that the notion of constant conjunctions of events and the ontology that generates it, are misconceived. The reasoning starts with two observations.

1. Virtually all of the constant conjunctions of events that are of interest to science (including economics) only occur in experimental situations. The point closes the system (see below) by creating a particular set of conditions that will isolate the one interesting causal mechanism. This mechanism is then allowed to operate unimpeded and the results, the constant conjunctions, recorded. Hence, the Humean law is more accurately styled as: ‘whenever event \( x \), then event \( y \), under condition \( z \)’.

2. The conclusions derived from experimental situations where conditions \( z \) exist (i.e. in closed systems) are often successfully applied outside experimental situations (i.e. in open systems).

It seems reasonable to draw the following problematic and/or counterintuitive conclusions:

a) Outside closed systems, where constant conjunctions of events appear not to exist, there are no laws - at least none of the Humean variety.

b) The question of what governs events in open systems is unaddressed.

c) The observation that the results obtained from closed systems are often successfully applied in open systems has no valid explanation.

d) Laws of nature, appear to depend, upon the intervention of humans, in which case the epithet law of nature is inappropriate.

e) Although Bhaskar tends to discuss the practice of natural science, it is evident that it can readily be extended into social science by making the following claims about human agency. If human agency is real, then (a) human agents could always have acted otherwise, and (b) human action must make a difference to the social world. The conclusions derived from an investigation of the practice of natural science, therefore, hold for social science.

The fact that these conclusions arise, not only casts grave doubts upon intelligibility of this Humean conception, it also has implications for ontology. Consider the following diagram.
Rejecting constant conjunctions of events as most unlikely features of social reality, CR turns towards investigating the structures and mechanisms that govern the events of experience. Rather than the ontology being restricted to the fused domains of the actual and empirical, it is more appropriate to conceive of it as being stratified. The best way of understanding what this stratified ontology entails is via an example.

**Domain of the empirical**
One might perceive a union/non-union wage differential.

**Domain of the actual**
Most, but not all, unionised workers actually do obtain higher wages than non-unionised workers most, but not all, of the time.

**Domain of the deep**
There are (metaphorically) 'deep' structures and mechanisms such as the economic, social, political relations entered into by labour market agents as well as institutions such as power. These deep structures and mechanisms causally govern events in the domain of the actual, that is, they govern the union/non-union wage differentials.

These domains are, typically, out of phase with one another meaning one cannot equate the occurrence of an episode in one domain with its manifestation as an episode in another domain. This is because these structures and mechanisms act transfactually: once set in motion, they continue to govern events irrespective of the influence of other mechanisms, and/or of the resulting event patterns. The reason for the non-existence of constant conjunctions of events is that countervailing structures and mechanisms such as government legislation might also be governing the behaviour...
of the agents engaged in the wage bargain.

For economists, constant conjunctions of events appear to be found only in the "conceptual experiments" (Pencavel 1994: 14), that constitute closed systems. Herein constant conjunction are engineered by satisfying four closure conditions. Whilst there are, conceivably, other conditions suitable for consideration, the present state of economic theory leads one to suspect these four are the most appropriate.

3.2 Intrinsic closure conditions (ICC)

The internal state of the individuals that constitute the system must be engineered in such a way that when acted upon by causal factors $x_1,x_2,...,x_n$, the relevant individual always responds in the same unique, a priori predictable way, by initiating action $y$. Most economic analysis is specified in terms of individual entities which can take the form of individual human beings, or individual agencies such as firms and unions. Whatever form they take, these individuals have some form of internal or intrinsic qualities, complexity or structure. How an individual responds when acted upon by a causal influence, depends, in part, upon this intrinsic state. The decision taken by a workforce in response to a pay offer by the firm is likely to depend upon things like perceptions of fairness, level of political consciousness and so on. In this case, perceptions and consciousness are characteristics of the workforce's internal or intrinsic state. A perfectly constant state exists if the individual is specified atomistically. The atomistic individual is inert so that when acted upon by a causal force, it responds passively. This is analogous to the way a billiard ball responds when struck by a billiard cue: the trajectory of the ball is completely predictable, unique and constant - under certain conditions. One commonly used social scientific homologue of the billiard ball is the constantly rational optimising individual. The following analytical moves (or something similar) are necessary:

a) Make the human individual the basic unit of analysis.

b) Engineer the human nature of this individual to be that of a rational optimiser - e.g. homo economicus.

3.3 Extrinsic closure condition (ECC)

The ECC ensures that the system is completely isolated from any external influences. This occurs when: (i) all relevant causal factors are internalised within the system, or, if there remain relevant influences extrinsic to the system, either (ii) these factors are specified such that they exercise a perpetually constant influence, or (iii) the elements within the system are isolated from their effects. There are numerous context specific ways to satisfy these conditions, many linked to ceteris paribus clauses, making it impossible to suggest the analytical moves that are necessary for the satisfaction of the ECC. There is, however, one particular analytical move that is worth mentioning partly because it will be used in examples below, but more importantly, because it is a crucial part of many trade union models.

Not only must the internal state of the entity be specified as that of a constantly rational optimiser, but that which it optimises over must also be constant. Without a specified set of objectives, when faced with causal factors $x_1,x_2,...,x_n$, the individual might optimise over $a_1$, $a_2$ and $a_3$, on one occasion, thereby initiating action $y$; and optimise over $b_1$, $b_2$ and
b. on another occasion, thereby initiating action z. Constancy is restored by restricting the objectives to one unchanging set. The ECC is satisfied, in addition to moves (a) and (b) above, by making the following analytical move:

c) Specify one and only one unchanging set of objectives to be optimised over.

3.3 Aggregational closure condition (ACC)

Even if the ECC and ICC are satisfied, there is still no guarantee that when faced with relevant causal factors the entity will initiate one predictable, unique and constant course of action. This is because economic analysis often has to deal with individual entities combined into groups. Engineering closure conditions for a unionised workforce as a whole is more problematic than for a single individual. The whole point of the union is to initiate a course of action that one individual acting alone would be unlikely to take. The response of the workforce to causal factors \(x_1, x_2, \ldots, x_n\) will vary depending upon the characteristics, and distribution, of individuals or sub-group of individuals that constitute that workforce. Internal constancy must be maintained over an aggregate of individuals. One way of doing this is to focus upon the objectives of some appropriate sub-group. The ACC is satisfied, in addition to moves (a), (b), and (c) above, by making the following analytical move:

d) Specify the objectives of the group as those of an appropriate sub-group.

3.4 Reducibility closure (sub) condition (RCsC)

Finally, a sub-condition needs to be appended to the ICC, ECC and AGG to ensure that the number of potential courses of action an individual might initiate is reduced to one and hence is unique. This condition relates to the deductive machinery of the DN model. Deducing a unique solution requires that the system is fully specified via a series of auxiliary assumptions that might be referred to as assumptions of tractability. These are merely technical assumptions whose sole purpose is to ensure the relevant functions are well behaved, thereby preventing perverse outcomes. There are no typical analytical moves necessary to satisfy this sub-condition.

In sum, then, rather than understanding the kinds of analytical moves made by economists merely as \textit{ad hoc}, they can be rendered intelligible by understanding them as attempts to satisfy the closure conditions necessary to engineer constant conjunctions of events.

4. The difficulty of satisfying the adequacy criteria

The next three sections demonstrate how difficult, (if not impossible) it is to incorporate into a model, phenomena that will satisfy the adequacy criteria, whilst at the same time making the kinds of analytical moves necessary to satisfy the closure conditions. Because there is no context independent way of stating what kinds of analytical moves are necessary to satisfy the closure conditions, it is difficult to sustain a general claim that the objectives of closure and
theoretical adequacy are antithetical. What can, arguably, be claimed, however, is that whenever the goal of satisfying adequacy criteria runs up against the goal of satisfying closure conditions, the former goal is the more likely to be abandoned.

4.1 Predictive power
If the CR perspective is correct, the belief that one can deduce a statement or prediction from a system that has been extensively engineered to ensure its closure, then assume this prediction will hold when transferred to the open system of the real world has no valid basis. Deducing statements about the action of agents operating in a closed system, and transferring them to the action of agents in the open system, commits the fallacy called *ignoratio elenchi*. This entails "assuming that one has demonstrated something to be true of X when the argument or evidence really applies to Y which is not the same as X in some respect" (Gordon 1991: 108). What is "not the same" in this context is the existence and ubiquity of constant conjunctions of events.

But is it not the case that some predictions are relatively successful, indicating that some rough and ready constant conjunctions exist? I noted in part 2.1 (ii) that even a (relatively) successful prediction, coupled with an open acknowledgment that numerous explanatory variables known to be influential are omitted, means that many crucial aspects of this differential remain *unexplained* and *unaddressed*. The theory cannot account for its own (alleged) predictive success. CR can, however, account for this dilemma: in order to satisfy the ECC, many explanatory variables have to be omitted, rendering them unavailable for use as part of an in-depth inquiry and explanation.

It appears that economic theories of trade unions cannot satisfy the adequacy criterion of predictive power, because the criterion itself is inappropriate. Failure to recognise this has simply locked economists into attempting to hit an impossible target. One problem arising from this misguided attempt manifests itself in a problem euphemistically referred to as that of *omitted variables*.

Satisfying the ECC entails making assumptions about which phenomena are to constitute the system and which are to remain outside its scope. As Paz Espinosa and Rhee (1989: 572) put matters: "Obviously, not all...factors can be included in this simple model, but we think we have included some important factors". But, what criteria are used to establish what is an important factor? What grounds does one have for thinking one has included the important factors and excluded the non-important factors? If the world is an open system, there can be no such criteria or grounds.

From the CR perspective, the problem of omitted variables is actually a manifestation of the inability to satisfy the ECC. According to Booth and Chatterji:

> It is hard to use...comparative static predictions to shed light on the recent falls in union density in Britain since 1979, since many of the exogenous variables were changing simultaneously over this period (1995: 352).
In a footnote they exemplify these changes: "union power declined...there were demand shocks, costs of union organisation increased following legislative changes and finally changes in ideology" (ibid). This is merely part of an inexhaustive list of phenomena that are isolated from the model with the sole aim of satisfying the ECC. The fact that this list could be extended substantially sets up the problem of ECC in the first place.

Consider the issue of the unions' objective function, as a means to explore the problem more concretely. According to Oswald (1985: 125) "there is at least a measure of agreement about adequate ways to specify a union utility function". One has to disagree. There appear to be a continuously evolving set of objectives and constraints, that can be used in many different combinations. Some of the more common variables include: wage, employment, wage bill, rent, wage relativities, unemployment benefit, expectations or probabilities of various outcomes, uncertainty of things like \textit{ex ante} price of output, membership costs and so on. Compare the union objective function of Dunlop to a contemporary version. For Dunlop (1944):

\begin{equation}
U = wn
\end{equation}

where utility is a function of wage income of employed union members. For Booth and Chaterji (1995):

\begin{equation}
EU = n(w) [u(w-a)+c]+[1-n(w)] u(B)
\end{equation}

where expected utility is a function of \((n)\) number of workers employed, \((w)\) union wage, \((a)\) union subscription cost, \((c)\) workers evaluation of the union private good such as a grievance procedure obtainable only through membership, and \((B)\) expected alternative income.

The point of the example is this: should the model fail to satisfy various econometric tests, and the problem suspected to be a variable omitted from the union objective function, then another variable can easily be added...followed by another, and then another and so on. There are no shortage of variables to choose from and nothing to halt this regress.

4.2 Correspondence with reality, ICC and ECC

Recall that the ICC and ECC are satisfied by making analytical moves (a), (b) and (c). It is clear that these moves (or something like them) are made in virtually all economic theories of unions. This section cites three examples to demonstrate that making the requisite analytical moves severely restricts the choice of what to include in the model, and ultimately prevents correspondence with reality.

In noting that union workers might "care about the welfare of those outside their union" Caruth and Oswald (1987: 441) have stumbled upon what is an absolutely vital factor in understanding what a union is. Their observation that unions are, in part, altruistic institutions is clearly realistic, and if it could be incorporated into the model, this would add a degree of correspondence with reality. How, then, do Caruth and Oswald respond to their own, clearly realistic, observation? They appeal to what everyone else does in order to assume it away, writing:
"However, few economists would want to take an axiom of unselfishness as the foundation stone upon which to construct a theory of trade union actions...As long as one is willing to make the usual assumption that individual workers are rational and selfish* the theory they proffer will go through (ibid emphasis added).

The (emphasised) language is misleading in that it implies choice of axioms and assumptions. It is not a question of "wanting" or being "willing", these are red herrings. Caruth and Oswald would find it extremely difficult to take their own realistic observation seriously. If they ever tried to include their observation into their model, immediately the analytical moves (a), (b) and (c) noted above could not be made. The agent they would then inherit would come ready supplied with an intrinsic structure (i.e. not atomistically specified) and so would not permit them to insert the specific internal state they require, namely that of the constantly rational optimising agent. And without this there is little point in specifying one and only one range of objectives to be optimised over.

Pencavel is acutely aware of the range of factors that have to be left out of theories of unions in order to proceed "analytically". For example, he emphasises "how narrow is the meaning given to trade unions' objectives" and notes the absence of considerations of process and the determinants of union structure (1994: 58). Later he adds a list of possible objectives that arise from workers seeking to improve the quality of their lives such as independence from supervision, establishing of rules to govern crew sizes and types of machines (ibid: 93). The list could, of course, be extended considerably - note the potential for regress here. However, his lament that these factors "have not yet been fully integrated into economic analysis" (ibid: 94) indicates a kind of blind spot. The absence from economic theories of the range of factors he lists, is not simply the result of an ad hoc "penchant for convenient and popular, if uncorroborated, functional expressions" as he puts it (ibid: 66). Rather, it is the result of attempting to satisfy closure conditions. Such factors must be omitted because their inclusion would almost certainly prevent analytical moves (a), (b) and (c) from being made. One likely response would be to formalise and/or quantify these factors in order to bring them into the equation. Yet formalising and/or quantifying an objective such as 'improving the quality of working life' for example, in order to make analytical move (c), would most likely entail doing such violence to reality that it would subsequently fail to correspond with it.

It is worth noting that Pencavel (1994: 21) intuitively grasps the problems surrounding the satisfaction of the ICC, by noting that estimating union wage gaps is difficult because "firms respond to higher wages by upgrading the quality of their employees". In effect, he recognises the difficulty of specifying an unchanging internal state (quality of employees) so that ICC can be satisfied.

According to Booth (1995: 83), "the economic model maker faces a trade-off between tractability and realism". Without explaining her use of the term "realism" she goes on to consider "what appropriate assumptions might be made for a
tractable model. It appears that tractable assumptions are those than ensure a focus on outcomes rather than processes.

While procedural arrangements may well impinge on these economic variables, and may also provide an additional economic rationale for union existence and viability, it seems reasonable to restrict our attention to outcomes, since these are vital to understanding the workings of the economy (Booth 1995: 83).

Again the issue is posed as if economists have a choice about focusing upon either outcomes or processes. Again this is misleading. Booth would find it extremely difficult to (meaningfully) model the kind of procedures she knows are important. If a procedures such as “the role of custom and practice” (ibid: 82) was admitted into the model, analytical moves (a), (b) and (c) could not be made. Custom and practice imply a space for human agency vis-a-vis creativity, of doing otherwise, of subjective interpretation, negotiation and deliberation, none of which is possible since real human agency evaporated when the individual was atomistically specified. Custom and practice imply a changing and perhaps contested set of objectives. The discussion of processes versus outcomes is another red herring. Choice is, once again, severely restricted due to the analytical moves necessary to satisfy closure conditions.

These three examples reveal that choice of what aspects of union behaviour to build into the model is determined by the kind of analytical moves made to satisfy closure conditions. Caruth and Oswald, and Pencavel cannot freely choose which factors to write in the objective function, and Booth cannot freely choose to model processes. They cannot choose those aspects of union behaviour to build into their model on the grounds that they increase the correspondence with reality because the goal of satisfying the closure conditions is more immediate.

### 4.3 Realistic assumptions and the ACC.

The most straightforward way of satisfying the ACC is simply to assume that membership is homogeneous. The union can then be defined as an aggregation of identical individuals with one objective. There is no shortage of models that are quite happy to use this assumption, although they must immediately surrender to the charge of using unrealistic assumptions.

Some economic theories of trade unions, however, embrace the more realistic assumption of heterogeneity, and the existence of diverse objectives being pursued by diverse sub-groups within the union. This makes it difficult to conceive of the union objective and raises issues like the leadership-membership conflict. Allowing the assumption of heterogeneity appears to offer the following advantages.
a) By assuming the union objective function to be that of the union leadership, one can re-establish the conception of the union objective whilst maintaining the assumption of heterogeneity.

b) This assumption also satisfies the ACC, by defining the group appropriately, so that a constant response to influential factors emerges at some aggregate level.

c) By assuming a leadership objective, and a leadership-membership conflict, an important aspect of reality is 'locked on' to, namely, the union as a "political" entity.

As is well known, Ross argued that unions are not maximising agents, but political institutions. The problem for anyone who accepts this as a realistic statement, is how to integrate it within a formal model. One economist that has tried is Pemberton (1988). Whilst Pemberton does not actually state it, it is clear that one strength of his paper is that it appears to accommodate more realistic assumptions. This paper is, therefore, a useful platform from which to investigate the possibility that realistic assumptions can be combined with satisfaction of the ACC.

Realistic assumptions are those that 'lock on' to a feature of reality, make that feature theorisable by bounding or narrowing down the field of inquiry, and do this without invoking fictions. What makes Pembertons' twin assumptions that the union objective is that of the leadership, and that there exists a leadership-membership conflict appear realistic, is that they 'lock on' to well recognised "political" features. These "political" features are then made theorisable, apparently without invoking fictions. But appearances can be deceptive.

The kind of thing the economic literature on unions refers to as "political" is, upon closer inspection, not political at all but some kind of one dimensional, utilitarian pursuit of self-interest. Leaders' objectives are variously described as: "corrupt" (Pencavel 1994: 78); "venal" (Flanagan, 1993: 34) and displaying "malfeasance" or impropriety (Farber, 1986: 1079). The essence of these various sentiments is that union leaders pursue objectives that are narrowly self-interested. One only has to glance at the objectives Pemberton chooses to see this. The "political" conflict he incorporates into his model between membership and leadership reduces to one whereby the former seek higher wages, the latter seek lower wages - and therefore the highest possible employment and membership. This reduction merely empties the dimension of the political of anything really political, and substitutes for it, the thin gruel of a utilitarian squabble.

Pemberton has succeeded in employing assumptions that 'lock on' to "political" features, but not to political ones. Such features, whilst they contain a grain of truth, force the multi-dimensional nature of such politics into the single dimension of utilitarianism, wildly exaggerating the significance of this grain of truth in the process. Moreover, treating the politics of the leadership-membership conflict as nothing more than the "mechanics of self interest" as Jevons once put it, reduces it to a fairy story, and hence to a fiction.
This reduction of the political to "political" is not accidental, but follows from the analytical moves made to satisfy closure conditions. To demonstrate this, compare Pemberton's various moves with the analytical moves necessary to ensure systemic closure, and note the similarities.
* Move (i) is similar to moves (a) and (b).

* Moves (ii) and (iv) are similar to moves (d).

* Moves (iii) and (v) are similar to (c).

It would appear, then, that the necessity of making analytical moves that ensure satisfaction of the closure conditions is always hiding in the shadows. The analytical moves Pemberton makes, and the assumption he subsequently employs are not any old moves or assumptions, nor are they merely *ad hoc*: they follow from his attempts to satisfy closure conditions. Assumptions cannot be selected on the basis of their realisticness, because they must be selected for an alternative purpose. Pemberton has to 'lock on' to "political" features, because if he 'locked on' to political features, he could not make them amenable to deductivism - at least not without fictionalising them. Even by employing the twin assumptions that the union objective is that of the leadership and that a leadership-membership conflict exists, it is still not possible to simultaneously satisfy the closure conditions and employ realistic assumptions.

Pemberton is, however, by no means alone. Most real social, political, psychological and institutional features, stemming from any non-utilitarian treatment of unions could not be incorporated within a deductivist framework. The literature is replete with remarks about the importance of this or that political, social, psychological or institutional fact...
Conclusion

It is difficult enough for one paper to address fundamental issues of methodology and at the same time draw upon examples from substantive theory. It is virtually impossible to conclude by elaborating a fully developed alternative method, adequate to the theorisation of trade union behaviour. I can, therefore, do no more than sketch out some general points, recognising fully that this sketch will be rather misty. Hopefully it will whet the appetite and encourage the reader to delve a little more into critical realism.

This paper has demonstrated that mainstream economic theory has great difficulty in satisfying its own adequacy criteria because the philosophy and methodology underlying these theories are inappropriate. If correct, this is a serious predicament because it strongly suggests that nothing short of a complete abandonment of the deductivist method and the philosophy that grounds it can secure a way out. And most economists recoil from this prospect, in part, because they presume that such complete abandonment leads to a state of theoretical or analytical impotence. This presumption is, however, incorrect. One can adopt CR as alternative mode of theorising.

CR is not merely confined to critique, but actually empowers the formulation of more adequate theories. If one cannot be deduced, one does not have to give up on systematic investigation: the conditions for the possibility of these events can be excavated. The deep structures that act with transfactual necessity to govern the flux of events can be uncovered and their operation illuminated and explained. Hence the domain of the deep is where investigation must focus. As Bhaskar puts matters:

> Looked at in this way...the task of the various social sciences [is] to lay out the structural conditions for various conscious human actions for example, what economic processes must take place for Christmas shopping to be possible but they do not describe the latter (1989, 36).

Metaphorically speaking, the correct *modus operandi* of economics is not to move (horizontally) between events, trying to discover or engineer constant conjunctions, but to move (vertically) from events to the mechanisms that govern them. Economic

Something akin to this alternative has already been adopted by certain schools within labour economics, even if the various writers do not discuss their philosophical commitments. Much of the *Segmented Labour Market* literature (Cf. Rubery 1994; Peck 1996 especially chapter 3), appears to be an example of the kind of thing CR proposes. Another example appears to be Piore’s (1993) recent discussion of the central role played by cognitive structures in underpinning labour market activity. Whilst I might be more tempted to emphasise the role of social structures, his overall approach appears to be compatible with that of CR.
References


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1. Steve Fleetwood, Department of economics, De Montfort University, Milton Keynes, UK. I wish to thank Keith Abbott, Tony Lawson, Ray Petrides, and Gianni Zapalla for their helpful comments on various drafts. This is a condensed version of two longer papers, Fleetwood 1996a&b.

2. Booth (1995) deals with methodology in two pages (83 and 117). The literature of the last twenty years or so contains only one paper Turnbull (1988) critical of the methodology adopted in trade union modelling. Zappala (1993: 210) laments the "lack of discussion of methodological issues in labour economics". I must, therefore, completely disagree with Dertouzos & Pencavel (1981: 1163). Because there are no "methodological debates" in the (recent) literature, there cannot be any "fruitless" ones. Whilst many of the methodological arguments contained in this paper extend beyond the economics of trade unions, relegating them to specialised journals where these economists can avoid engagement with them is an unhealthy state of affairs for methodology and the economics of trade unions.


I use of the term "meaningful" to emphasise the point that anything can be quantified if one is willing to end up with variables that are such a pale reflection of reality that they are meaningless.

7. One often comes across comments such as our model "explains 52 per cent of the variation in wages of semi-skilled manual workers between establishments" (Blanchflower 1984: 325). Comments like this must be problematised and explored.

8. Card and Kreuger's recent work is an example of replication. As they put it: "When the same econometric specifications...are re-estimated with data from more recent years, the historical relationship between minimum wages and teenage employment is weaker and no longer statistically significant" (1995: 2 emphasis added).

9. This is Forslund (1994: acknowledges that there have been no studies using his approach, so he casts around for "similar" models to compare his findings with. Examining one allegedly "similar" model (i.e. Farber 1978) reveals similar predictions generated using different explanatory variables. They tell different stories as it were. It is difficult to know, therefore, if theses models are in fact "similar".


11. Blanchflower and Oswald (1995) claim to be "driven by a hunt for empirical regularities" (11). They attempt to
"document the existence of an empirical 'law' of economics. The law features two of the variables that most interest policy makers, namely, the level of unemployment and the level of wages" (1). They even liken their modus operandi to medical statistics where constant conjunctions between variables like smoking and lung cancer allegedly exist. These authors are engaging in pure empirical realism.

- The point, ignoring the possibility that they may occur accidentally and/or over some restricted spatio-temporal region, and/or be trivial.
- Stewart (1995: 143) grasps this point implicitly by acknowledging that the impact of legislation on wage differentials "may simply have been offset by other factors". He does not, however, have a methodological perspective that allows him to grasp the significance of this point.

It is extremely important to understand that I use the term 'analytical move' generically, and in full recognition that there will, typically, be several ways of making any move, depending upon context. For example, maximising might be replaced with satisficing or some other formally specifiable objective. The moves I cite here are by way of example, even if they are commonly used. Whatever form a particular analytical move takes, it is crucial that it must not violate the closed nature of the system.

- Interestingly, Dunlop's (1944: 4) observation that an "economic theory of a trade union requires that the organisation be assumed to maximise...something" appears to be an implicit recognition of something like analytical move (b). Farber (1978: 924) and Pemberton (1988: 756) both cite the same quote, whilst Flanagan et al (1993: 72) paraphrase it.

Virtually all economists feel obliged to mention the limitations of their models (Cf. Farber 1986: 1069, Fedeli 1994: 102), although none are prepared to investigate the extent to which such limitations impair any correspondence with reality.


I use the term political to denote that which really is political; and the term "political" to denote that which many economists refer to as political. Booth (1995: 89) does not avoid the following criticism simply by defining "political" as having nothing to do with "broader external political aims" pursued by unions.

It is instructive to compare the (narrowly economic) approach of Clark and Oswald (1993) vis-a-vis the union leadership-membership conflict, to that of Kelly and Heery (1994). The former, unlike the latter, reduce the political content of the conflict to the extent that it fails to capture the reality of this rather complex issue. Moreover, this reduction appears to be a result of adherence to the deductive method.

- If one...Cf. Fleetwood's (1995, 1997) work on Hayek.

Economically, therefore, both to agree and disagree with Pencavel (1991:83-4). I agree that occupying the wings of much economics are attempts to discover correlations and "toy models". In my terminology these are attempts to discover and engineer constant conjunctions of events via closed system modelling. I disagree that the way forward lies in finding a "middle ground" between the two wings, because both of these wings share the same, untenable, philosophy, ontology, and method. Seeing the problem in terms of a deductivist versus inductivist (false) dichotomy misses the point: both are preoccupied with obtaining constancy at the level of events. Moreover, if the problem is not located at this meta-theoretical level, then any middle ground, will run into the same problems as the wings.