

# A Critical Realist Reply to Walters and Young<sup>1</sup>

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Walters and Young (2001, hereafter W&Y), offer a brief sketch of critical realism and three objections to it. This reply starts with three points of clarification to their sketch, before going on to tackle their objections.<sup>2</sup>

## **Critical realism: some points of clarification**

First, W&Y claim that the critical realist understanding of the layered ontology may “be descriptive of scientists’ way of thinking about the world”, and anyway seems “unlikely to have much methodological significance” (5). Whilst the first claim might be held by critical realists and fellow travellers (W&Y themselves perhaps?) it certainly is not held by ‘postmodernists’; ‘poststructuralists’ or (strong) ‘social constructivists’. Neither is it held by those even loosely defined as ‘positivist’ because the latter cannot (consistently) countenance a layered ontology (*cf.* Lawson 1994 259-266). The second claim is incorrect. The presupposition of a layered ontology is a necessary precondition for the switch in the mode of reasoning away from one based upon prediction and causality as event regularity, towards one based upon explanation and causality as powers.

Second, whilst W&Y correctly suggest that for critical realists, society can be “analysed causally”, their rider that it can be “analysed causally *in law-like terms*” (7 emphasis added) is misleading. It is also misleading to suggest that the critical realist method is about “uncovering social laws” (*ibid*) and “law-like explanation”(12). For the critical realist, causal analysis deals with *tendencies* not laws, and the difference between these two conceptions cannot be glossed (*cf.* Fleetwood 2001a, section 5).

Thirdly, it is important not to misunderstand the role of “stylised facts” (8) or, more accurately, “demi-regs” (26). Demi-regs have a primary and secondary role.

Their primary role is “to direct social scientific investigations, through providing evidence that, and where, certain relatively enduring, and potentially identifiable mechanisms have been in play (Lawson 1997: 207). This is largely unproblematic. The secondary role, however, is a little more ambiguous. According to W&Y, empirical support for a hypothesis “takes the form of explanation in terms of stylised facts” or, demi-regs (8). Lawson tells us that “the second role is as an aid in assessing causal hypotheses once they have been formulated”, adding “[t]heories can be assessed according to their abilities to illuminate a wide range of phenomena. And typically this will entail accommodating...demi-regs”(213). One needs to be clear about what this second role for demi-regs *is*, and is *not*. Allow me to elaborate .

One does *not* offer the existence of demi-regs as evidence, in the way one might offer data as evidence confirming (or dis-confirming) some hypothesis. That is, one does not hypothesise that causal mechanisms *m* and *n* produce event *e*, then offer the demi-reg ‘whenever mechanisms *m* and *n* operate, then event *e* often follows’ as evidence of these mechanisms. This would be like trying to use the outcome of the operation of some mechanisms, the event sequence or pattern, to account for these mechanisms. One *does*, however, use demi-regs in support of an explanation. One ‘accommodates the demi-regs’ in the sense that one argues that mechanisms *m* and *n* are the kind of mechanisms that could produce events like *e*. One accounts for these mechanisms, along with the powers they possess, by means of evidence *other than the event sequence or pattern they govern*.

### **Three objections to critical realism**

W& Y argue that critical realism: misleadingly claims that mainstream economics is positivist; it misrepresents the nature and purpose of Hume’s work; and it lacks an elaborated epistemology. The remainder of this reply tackles these objections.

### **Critical realism, positivism and mainstream economics**

There are two responses to W&Y's objection that mainstream economics cannot be labeled 'positivist'. The first, the most common, and the weaker response, is to suspend judgement on the issue of positivism, and argue instead that mainstream economics can be labeled 'deductivist' (cf. Lawson 1999). W&Y appear to have no problem with this response, apparently accepting the "characterisation of economic theory in terms of the axiomatic-deductive method" (16). The second, the stronger response, and the one I defend below, is to argue that mainstream economics can be labeled 'positivist'.

The basis of W&Y's objection to labelling mainstream economics 'positivist' stems from the existence of multiple definitions of both economics and of positivism, (cf. Lawson 1997: 292-3). One must tread carefully here, however, because there are two ways of dealing with this diversity, and they are not equally valid. First, one might claim that positivism or mainstream economics are such diverse activities that any attempts to define them is destined to fail. On this account, there are no activities that can be labelled 'positivist' or 'mainstream economics'. Second, one might redouble one's efforts to cut through the *Gordian Knot* of diversity by identifying the *essential features* of positivism and mainstream economics. I reject the first way of dealing with this diversity because the existence of a thing or activity is independent of our ability to define it - irrespective of how difficult arriving at a definition might be.<sup>3</sup> Unless, therefore, one is prepared to abandon the term 'positivism' altogether, and W&Y say nothing to suggest they are so prepared, then one must accept that 'positivism' refers to some set of activities and set about finding what, in essence, they are.

Whilst positivism has many facets, its essential facet or feature is this: *positivism is a philosophy of science rooted in "knowledge, its nature and limits" and "committed to the claim that human knowledge takes the form of sense-experience"* (Lawson 1997: 19).

Do W&Y agree with this definition? In part they do because, out of the positivists they mention, they identify sense experience as crucial to them all – namely Comte; the logical positivists; Popper; Lakatos; and the instrumentalists. W&Y link “traditional and modern understandings” of the term ‘positivism’, to a “relationship with evidence or indeed experience” (17). Moreover, given that econometrics does require sense experience in the form empirical data, W&Y appear to accept that mainstream economics that is based on econometrics is positivist. Whilst W&Y’s objection to labels has more to do with diversity of the term ‘positivist’, it does not seem unreasonable to conclude that, for them, any economic theory that has a “relationship with evidence or indeed experience” can be labelled positivist, and such economic theory, clearly, exists.

That said, W&Y do not altogether agree with this conclusion because some economic theory *appears* to have no connection with data, observation or sense experience at all. This kind of economic theory is ‘pure’ theory (i.e. purely algebraic models devoid of any data). W&Y seem to have something similar in mind when they refer to analysis couched in terms of “instrumentally rational agents” (13); “constrained optimisation problems”(13); and the lack of a “concern for the evidence of observation”(14). They refer to this kind of economics as “Cartesian rather than Humean” (14). Following Pencavel (1991: 84) I will refer to this kind of economics as “toy” theory. Toy theory *appears* to have no connection to positivism because it *appears* to have no connection to sense experience or observation. Appearances can, however, be misleading as will be shown in two stages.

In stage one the central features of deductivism, as it is used in economic theory are traced backwards, step by step as it were, to the ontology of empirical realism that renders these features intelligible. And empirical realism is an ontology of sense experience. Stage two demonstrates that even toy theory shares these same ontological commitments. If, therefore, toy theory has a connection to positivism because it has a connection to sense experience, then critical realists can declare all mainstream economics to be positivist.

### Tracing the central features of deductivism back to ontology

- Central to the way deductivism is operationalised are functional relations, generalised as  $y = f(x)$ . These can also be expressed as laws and styled '*whenever event x then event y*'.
- If functional relations and laws are to have economic meaning (as opposed to 'mere' mathematical meaning) then they *must* imply causality. Causality renders them intelligible. The clear implication of writing,  $q = f(p)$ , for example, is that the quantity (demanded or supplied) varies with, and is caused to vary by, price.
- Functional relations and laws are not, however, rendered intelligible by just any old account of causality. Rather, they are rendered intelligible by an account of causality based upon constant conjunctions of events – and deriving from the work of Hume – of which more below. Laws as constant conjunctions of events are, thereby, referred to as Humean.
- If, one were to discover a constant conjunction of events in the form of a Humean law or functional relation, one might claim to have *scientific* or general knowledge. What renders this claim intelligible are the constant conjunction of events that makes the deduction or prediction of some event(s) from antecedents possible. *Constant conjunctions of events drive the nomological machinery of the deductivist method.*
- Scientific or general knowledge in the form of constant conjunctions of events is only intelligible on the presumption that *particular* knowledge is derived via experiencing, and subsequently recording events. If this were not the case, the scientific or general and particular knowledge would be rooted in two different sorts of evidence.

- The events that constitute the constant conjunctions cannot be other than unique, individual, or atomistic, since any connection or relation between them would be itself impervious to sense experience. In this case the nature of the connections would require prior explanation, thus undermining the explanatory power of sense experience.
- Because ontology is confined to that which is experienced it is, therefore, confined to the *atomistic* events of sense experience. Because these objects are confined to experience the ontology is *empirical*; and because these objects are thought to exist independently of one's identification of them, it is *realist*. The ontology can, thereby, be labeled *empirical realist* and consists of *the atomistic events of sense experience*.

The deductivist method and the functional relations and laws that operationalise it, are rendered intelligible, therefore, by the consistent presupposition of causality as constant event conjunctions and an empirical realist ontology consisting of *the atomistic events of sense experience*. For the empirical realist, what *is*, is presumed co-existent with what is *perceived*. Causality as constant event conjunctions means that if some event is perceived, one can only seek its cause in terms of another perceived event. *There is nowhere else to seek a cause* because any other domain in this ontological spectrum is ruled out. In sum, then, deductivism *must* give primacy to sense experience. The next stage demonstrates that toy theory is committed to this same ontology.

### ***The ontological commitment of Cartesian, pure or 'toy' theory***

In Fleetwood (2001a, (cf. Lawson 1999) I argued that in toy models, scientific knowledge is generated by *constant conjunctions of atomistic events*, but because the latter are *implicitly* built into the model as an *a priori* premise, they do not manifest themselves *explicitly* as they do in more empirical models. This is not the place to repeat the argument, but the following example of toy theory might help make the point that even toy theory is rooted in sense experience or observation.

Shapiro & Stiglitz (1990) claim that a worker will choose to shirk iff the expected lifetime utility of an employed shirker ( $V_e^s$ ) exceeds that of an employed non-shirker ( $V_e^n$ ). The argument that claims like this have nothing to do with sense experience, or as W&Y (14) put it “need [not] imply any great concern for the evidence of observation” is arguably incorrect. The argument rests on three foundations – which will be stated and then elaborated upon.

1. Claims like this are not derived from any observed shirking and non-shirking behaviour in the workplace, but are spun out of the axiom of rationality and assumptions relating to utility maximisation.
2. Claims like this relate to theoretical possibilities not to behaviour that might be observed.
3. Claims like this are not tested against sense experience.

First, underpinning claims about shirking is a ‘common sense’ idea, notably, that in many circumstances, work is unpleasant and something to be avoided via activity like shirking. How does one know this? Partly because many of us have experienced or observed work, but more likely because one is aware of case studies written by researchers who have themselves observed such behaviour. Arguably, the process wherein certain theoretical claims are formulated requires analogy and metaphor, which would be inconceivable without recourse to (someone’s) sense experience. In other words, sense experience plays a part in the derivation of claims of the kind made above. Models where claims were spun out of axioms and assumptions and yet were *totally* counterintuitive because the postulated behaviour would have no connection whatsoever to any possible observable behaviour, would be *completely and utterly irrelevant* – see below.

Second, the clear *implication* of toy theory is that, under certain conditions, the hypothesised behaviour *could* be observed. If this is *not* an implication, then once again the possibility of irrelevance emerges. What we are dealing with, then, are not actually observed events, but *potentially observable* events, events that could be observed under certain conditions. Whether Shapiro and Stiglitz's claims are right or wrong is beside the point here; they clearly *believe* that under certain conditions, shirking will occur. The problem is *not* that their claim about the existence of shirking is implausible *per se* - such a claim is perfectly plausible. The problem is, rather, that requirements of the deductive method mean that they take this plausible claim and turn it into a total fiction by placing it within a set of conditions that cannot possibly occur. This is similar to the way in which the plausible idea that firms strive to make large and sustainable profits becomes fictionalised when the assumptions in which it gets embedded reduce it to the idea that firms maximise profits.

Third, the fact that Shapiro and Stiglitz do not test their claim against observed events does not imply they think their claim has no connection to reality. Many toy models are confronted with data at some later time when they are 'tested'.

Now, I fully accept that rooting toy theory in sense experience like this may well be taking liberties with intellectual behaviour that even positivists might not wish to condone. *But this is a problem for mainstream economics not critical realism.* I have merely presented a defence of toy theory on behalf of mainstream economics by showing how it might claim justification and intelligibility by rooting it in sense experience and hence positivism. If I chose *not* to present this defence, what would it do to the strong critical realist response that mainstream economics is rooted in positivism? Actually, very little, because if toy theory was totally devoid of any relation to the events of sense experience or potentially observable events, then toy theory would be reduced to an exercise in *complete and utter irrelevance*, thereby, disqualifying itself as economics of any kind. It would not be positivist, but then it would not be (mainstream) economics either. Toy theory is relevant and



part of mainstream economics, only if it is rooted in sense experience, whereupon it can be labelled 'positivist'. If toy theory is not so rooted, then it becomes completely and utterly irrelevant, and ceases to be part of mainstream economics.<sup>4</sup>

In sum, then, despite the fact that positivism is difficult to define, the stronger response that mainstream economics, including toy theory, is rooted in sense experience or observation, and hence rooted in positivism has now been defended.

### **The status of neoclassical economics**

W&Y think critical realism is wrong (a) to label mainstream economics as positivist and (b) to identify mainstream economics with the application of the deductivist method. The problem is, they tend not to keep these two arguments separate so argument (b) slips back into argument (a). I have already dealt with argument (a) and, argument (b) is relatively easy to deal with, for the simple fact that W&Y provide the key argument anyway. They write:

Neoclassical theory and practice are too diverse to be usefully labelled positivist...However, it seems clear that what is meant is not, in fact, the relationship of the generated hypothesis to the evidence but the law-like way in which these hypotheses are expressed. *That is, the objection is the characterisation of economic theory in terms of the axiomatic-deductive method* (16 emphasis added).

They then add:

Most neoclassical economists (and indeed practitioners of other schools including some forms of Marxism) would accept this characterisation of their theoretical structure (16).

Claims to mainstream economics “diversity” notwithstanding, they appear to agree that most economists would accept deductivism as a characterisation of their methodological position. What seems to bother W&Y more than the attempt to find commonality in the diversity, however, is the attempt, by critical realists, to trace the roots of mainstream economics to a theory of causality found in Hume.

The crucial issue seems to be the roots claimed for neoclassical theory of agency in the Humean theory of causation and the ontology which is inferred and transferred to such a structure of explanation (17).

## Hume

A few brief statements of position might help minimise any potential confusion.

- Hume is, clearly, one of the leading scholars of the Enlightenment, and critical realists have no desire to denigrate Hume’s massive contribution to scholarship.
- A good case can be made that on certain issues, Hume has definite realist overtones (*cf.* Dow 2001).
- Whilst W&Y are correct to point out that “Hume’s work is **not** fundamentally about event regularity” (21, emphasis added) he does nevertheless write about event regularity: and this has been one issue where critical realists have raised criticisms.
- Accepting that “Hume’s views on causal relations are not well developed” (W&Y 21) raises problems of interpretation. Whilst W&Y do not offer a Humean version of causality, Dow (2001) does, claiming Hume may be “closer to the critical realist understanding of cause”. Whilst Dow does present a nuanced investigation of Hume on causality, I remain unconvinced.<sup>5</sup> Even if in some places Hume can be said to have a realist understanding of causality, it remains the case that in other places, he has an event regularity understanding. Significantly, this latter interpretation has been used to

underscore contemporary notions of causality and it is this interpretation that is challenged by critical realists.

- One can accept that “the starting point” for the Humean account of causality lies in “the uncertainty surrounding our observations and statements about the world rather than a more questionable claim concerning the certainty of knowledge and its acquisition” (W&Y 19). After all, whilst Hume is neither a rationalist nor a simple minded empiricist, he is a sceptic.
- Relatedly, there is no requirement that observed events must “constitute a correct and complete account of causal connections” (19).

These last two points need a little clarification. For Hume, obtaining knowledge is, generally speaking, fraught with problems<sup>6</sup>, and obtaining knowledge of causes in particular, has its own specific problems. *If, however, there is to be any possibility of gaining knowledge of causes, and Hume seems to think there is such a possibility, then from the Humean perspective, this knowledge cannot be derived from anything other than sense experience.* For anyone of a remotely empiricist bent, and it seems impossible not to locate Hume here, sense experience or observation, provide the ultimate warrant for (fallible) knowledge claims.

This does not mean that Hume thinks unobservable entities do not exist; it does mean that they are inadmissible to an account of causality. Nor does this mean that any knowledge of causes is infallible, or that merely because we have observed event regularities we can be “overconfident” (W&Y; 19) in our causal claims; it does mean that, for Hume causal accounts grounded in things like powers are illegitimate candidates; whereas causal accounts grounded in observation are not. And legitimate candidates can, of course, turn out to be incorrect.

Whilst I think Hume’s account of causality is incorrect, it is consistent. Once the possibility of gaining scientific knowledge via anything other than sense

experience is rejected, a consistent account of causality can only be rooted in, and exhausted by, the raw material delivered by sense experience, in this case, patterns in the events. But not any old patterns will do the job. The patterns must be those of contiguity, succession, regularity, or constant conjunction in the events of sense experience.

### **Critical realism and epistemology**

The crux of W&Y's objection is that the criterion critical realism offers for adjudicating between competing claims (i.e. explanatory power) is weak. In comparison to the bold claims for positivism, this is true. But then again these bold claims are spurious: positivism has not delivered on its promises of adjudicating between competing claims either, so the comparison is moot. This section sketches two competing accounts of labour markets to demonstrate how one might go about adjudicating between them on the basis of explanatory power.<sup>7</sup>

### **The mainstream labour market (MLM) account**

The dominant MLM account conceives of labour markets operating solely via the 'economic' forces of supply and demand. Social structures (i.e. institutions, rules, conventions, habits) are omitted or fictionalised when analysing labour markets. Wages rates and quantities of labour demanded and supplied constitute the *essence* of the model, and determine the field of study. Moreover, it is a clear example of the deductive method, relying heavily upon the alleged existence of event regularities or constant conjunction of events. Consider one aspect of the MLM model, the theory of labour supply, a little more.

In the theory of labour supply a rational agent is presented with a choice between work and leisure subject to an income constraint. S/he chooses to supply more labour hours as the wage rate rises thereby giving rise to the law of labour supply. This law is, typically, augmented by a plethora of assumptions, of which I will mention just two: the individual is free to choose how much labour to supply; work and leisure are the only two uses of time, and are mutually exclusive. Now,

if we ask: what determines the supply of labour to the labour market, the answer is straightforward – the wage rate.

### **The Segmented Labour Market (SLM) account**

In the typical SLM account, labour markets are thought to operate via the ‘economic’ forces of supply and demand, they are also thought to be embedded in a set of social structures which significantly influence the operation of these forces. Social structures cannot, therefore, be omitted when analysing labour markets. In my version of the SLM account, there are no entities called ‘labour markets’ that are then embedded in social structures. Rather, labour markets *just are* the social structures that constitute them and ‘labour markets’ (in the conventional sense) simply don’t exist. Social structures that ‘connect’ firms to a potential workforce form the basis of the model, and determine the field of study.

Moreover, this is a clear example of what I call the *causal-explanatory* method. Because of the openness of socio-economic systems, results, consequences, or outcomes *cannot* be successfully predicted but the social structures that causally govern the flux of events *can* be uncovered and *explained*. To *explain* a phenomenon is to give an account of its *causal* history (*cf.* Lipton 1993; 33). And that means to give an account of the underlying social structures that make this phenomenon possible. The method seeks, primarily, to *explain*, and it does so in terms of underlying *causal* factors, hence causal-explanatory.

On this SLM account, the conditions that make it possible for workers to supply their labour, and for firms to demand it, is explained in terms of a set of underlying social structures, mechanisms, resources, networks and other factors that are drawn upon by workers and firms. The following sketches some of the causal phenomena (the supply side of<sup>8</sup>) a SLM account might include.

- The social structures that effect people’s differential ability to supply labour. For example, most people are dependent upon income from wage labour and

cannot wait until more favourable conditions emerge, or the 'right' job vacancy arises. In this context, the notion of 'choice' becomes highly problematic.

- The social structures upon which various groups are established and the ways in which membership effects their labour supply. For example, the structures that constitute the family generates a gendered labour supply, whereby the supply of female labour often involves finding appropriate hours; or jobs in appropriate geographical locations that give access to schools, bus routes etc.
- The social, political, cultural, ideological and demographic factors that effect the overall supply of labour. For example, the supply of workers leaving school depends upon government policy towards the school leaving age, which in turn depends upon a series of political and ideological factors.
- The state structures available to provide services, for example to the elderly, so that their families are not prevented from participating in the labour market.
- The interaction between structures of accumulation and state revealing, for example, how the domestic labour performed (usually) by women is necessary to the continued employment of paid labour by employers and hence necessary for accumulation of capital.
- The impact of class, race or gender structures on labour supply. Adequate education (or lack of it) impacts upon an individual's ability to supply labour of a particular type, and education has a class base. Female members of certain ethnic communities with an extended family network have, for example, been targeted to as a flexible supply of labour.
- The social structures that effect the way people *motivated/demotivated*. A complex process of socialisation is required to prepare workers for a life-time

of labour, involving the state, education system, media, church and so on. It took a couple of centuries to render the English working class amenable to a work-time orientation, and hence a labour supply regime suited to capitalism.

- Geographical structures. Most people consider their supply of labour within a geographical horizon. Whilst the extent of this spatial area depends upon things like immediate access to appropriate productive systems, and transport costs it also depends upon language barriers, and cultural factors like desiring to reside in certain areas.
- The social networks that effect how people select an employer. Social networks are an important means both of finding information about job vacancies, and actually getting them.

This is by no means an exhaustive list, and each of these causal phenomenon could be expanded upon – although not indefinitely. The point is, the account of these social structures is not ‘added on’ to a basic (deductivist) model of supply and demand. These social structures *just are* the labour market and must, therefore, be built into the causal explanation from first principles.

Now, if we ask: what determines the supply of labour to the labour market, the answer is less straightforward than in the MLM account, but it is far more sophisticated. The answer involves a detailed account of the social structures just outlined and leaves one in a position to explain the conditions necessary to present oneself at the door of an employer.

### ***Adjudicating between accounts on the basis of explanatory power<sup>9</sup>***

To argue that the SLM account has explanatory power is straightforward in the sense that *to explain is what it sets out to do*. The MLM account, by contrast, has no explanatory power for (basically) two reasons.

### ***Explanation is not event regularity***

The causal history of a phenomena is not merely (if at all) one couched in terms of the event that happens to precede the phenomena. One does not, for example, adequately explain (the event of) my office light becoming illuminated simply by pointing to the (event of) flicking of the switch that preceded it. Yet this form of 'explanation' is all that is available to the deductivist. The need to engineer closed systems means that the model has to remove, theoretically of course, all potential causal factors that might violate the closure conditions. This is why the MLM account contains none of the social structures that are included in the SLM account. It is crucially important to grasp that, *once removed from the model, relevant causal factors cannot subsequently be recalled and offered as part of the causal explanation*. Relevant causal factors are either included in the model (as in the SLM account), in which case they contribute to the causal explanation, or they are excluded (as in the case of the MLM model) in which case they cannot make such a contribution.

### ***Explanation does not allow known falsehoods***

If, as part of this causal story, one opts to include a *known* falsehood, or, which amounts to the same thing, leaves out some important causal factors (i.e. falsehood by omission) then the explanation can immediately be objected to simply by pointing to the falsehoods. One only has to reflect upon this for a moment to see this conclusion is self-evidently correct: if known fictions are allowed into explanations, imagine the bizarre explanations that could be advanced. And the MLM account is replete with known falsehoods such as: human beings are instrumentally rational; or the individual is free to choose how much labour to supply. These assumptions are not made because they are believed to be true, but because they are methodologically tractable.

In sum, then, whilst the criterion offered by critical realists for adjudicating between competing claims, (i.e. explanatory power) is not straightforward, especially in



comparison to the bold but spurious claims for positivism, it can, nevertheless, be operationalised.

## **Conclusion**

This paper defends the critical realist project in economics by making three claims. First, despite difficulties in defining both mainstream economics and positivism, mainstream economics (including toy theory) does appear to be rooted in positivism. Second, whatever Hume's own intentions, his work on causality, at the very least, sponsors contemporary notions of causality as event regularity. Third, explanatory power can be used as a means to adjudicate between competing accounts. I make these claims in the spirit of fraternal and scholarly disagreement, and hope they enhance the debate between two groups of heterodox economists (critical realists and W&Y and their fellow travellers) who already have a great deal in common.

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<sup>1</sup> I would like to thank Clive Lawson for his comments on an earlier draft.

<sup>2</sup> W&Y had access neither to the edited collection (Fleetwood 1999); or my own paper (Fleetwood 2001a) when they prepared their paper. I suggest the reader refers to Fleetwood 2001a, because it pre-empts some of W&Y's objections.

<sup>3</sup> Poststructuralists and strong social constructionists would, of course, disagree with this claim but it cannot be pursued here.

<sup>4</sup> None of what I argue here implies that what mainstream economic theory (toy or otherwise) actually claims about the socio-economic world is correct.

<sup>5</sup> Investigating interpretations of Hume's account of causality is, clearly, beyond the scope of this paper. Although Dow has led the interpretation of Hume as a realist, critical realists have not really responded – although I raise the issue in Fleetwood (2001a).

<sup>6</sup> Dow (2001) sensitises us to the fact that Hume was preoccupied with challenging the Cartesian claim that reality can be known merely from our ideas. This may have led him to play up the role of the empirical, and play down the role of powers when attempting to obtain knowledge of causes.

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<sup>7</sup> Whilst the use of explanatory power as an adjudicating criterion has been widely discussed by critical realists, examples of it in practice are (as yet) relatively rare. For this reason I cite my own work. This section is based upon Fleetwood 2001c & 2001d. Another example is Fleetwood 2001e.

<sup>8</sup> For the sake of brevity the social structures, resources, mechanisms and networks that govern the demand for labour are omitted and the supply side is concentrated upon.

<sup>9</sup> On the role of explanatory power as a criterion for evaluating theories see Boylan and O’Gorman (1995), and Fleetwood (2001b).

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