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CHAPTER 3

Ad hocness in economics and the Popperian tradition

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This chapter discusses the literature on ad hoc theory adjustment both within economics and within the Popperian philosophical tradition. It will be shown that there are two fundamentally different concepts of ad hocness[1] within the Popperian tradition, one associated with Popper himself and one with Lakatos, and that these two different concepts are mirrored in the way the term is used by economic methodologists and economic theorists, respectively. In Section I, Popper's use of the term "ad hoc" and its fundamental importance to the Popperian program will be discussed. In Section II, Lakatos's three different notions of ad hocness will be reduced to two (one the same as Popper's and one uniquely Lakatosian), and the importance of each of these to the methodology of scientific research programs will be examined. These first two sections, although simply surveys, are necessary because this particular aspect of both the Popperian and Lakatosian philosophy of science has been badly neglected in the recent literature on economic methodology. Section III discusses the use of "ad hoc" by both economic methodologists and economic theorists, and compares these uses with those in the philosophical literature. In the conclusion (Section IV) the methodological implications of the discussion in the first three sections will be examined. It will be argued that proper emphasis on the different notions of ad hocness, and the different groups who tend to emphasize each use, have significant implications for economic methodology, particularly Lakatosian economic methodology.

I. Ad hocness: Popper

There is a long tradition, within both scientific philosophy and scientific practice, that argues that a new theory is less than satisfactory if it is *designed solely to deal with a previously anomalous observation.* Such theories do not go beyond their predecessors and are considered to be

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[1] In the interest of clarity, the convention of italicizing or underlining ad hoc and its derivatives will be abandoned throughout (except where it occurs in quotations).

ad hoc. Although the term "ad hoc" has no universally accepted definition within the philosophy of science, it is usually reserved for a particular type of face-saving/falsification-avoiding theoretical adjustment. For instance, Laudan's (1977, p. 115) popular definition states, "a theory is ad hoc if it is believed to figure essentially in the solution of all and only those empirical problems which were solved by, or refuting instances for, an earlier theory."

As an example of the ad hocness problem, consider the following (hypothetical) case from economic theory. Suppose a macroeconomic theory T1 predicts that for any country whose money supply changes by the amount *m,* the price level in the following period will change by the amount *p*. Suppose further that after this result has been successfully corroborated for several countries, a contrary result is obtained for country A, that is, T1 is refuted by the evidence from country A. How can T1 be saved? How can T1 be adjusted so that the potential refutation is avoided? One approach is to simply modify T1 in an ad hoc way to avoid the refutation. For instance, we could modify T1 so that it now says, "for all countries except country A, *m* implies *p*." This new theory – call it T2 – is confirmed by all the countries that confirmed T1, and in addition, it is confirmed in country A, where T1 failed. Thus, it could be argued that since T1 was refuted and T2 is more confirmed (or, in Popper's terms, more "corroborated"), the move from T1 and T2 constituted *scientific progress.*

It seems obvious that this type of theory modification should not count as progress. T1 was refuted, but T2 is not really any better. The change from T1 to T2 was patently ad hoc; it was contrived solely to protect the theory from an observed anomaly, and served no other purpose. Not only does this amount to "cheap success" (Worrall, 1978, p. 49), it also makes "progress" trivially easy.[2] By this technique a successful theory can always be constructed out of one with a dismal track record. The history of human speculation abounds with such (ultimately unsuccessful) face-saving moves.[3,4]

Within the Popperian tradition, the ad hocness problem is even more

[2] For this reason, Watkins (1984, p. 166) refers to the non-ad hocness requirement as an "antitrivialisation principle." The principle states that "any philosophical account of scientific progress must be inadequate if it has the (no doubt unintended) implication that it is always *trivially easy* to make theoretical progress in science."

[3] Grunbaum (1976, pp. 329–30) cites historical examples.

[4] It should be noted that although the undesirability of ad hoc theories is being presented as obvious, not all philosophers of science would agree. Laudan (1977, pp. 114–18), for instance, finds no difficulty with this type of ad hoc modification, claiming that to condemn such adjustments puts "an epistemic premium on theories which work the first time around" (p. 117). Grunbaum (1976, pp. 358–61) advocates a similar position.

important than in other philosophies of science. As a matter of intellectual history, Popper's reaction to precisely this type of ad hoc defensive stratagem contributed to his initial interest in the demarcation problem.[5] Philosophically, the entire Popperian approach to science is characterized by *bold* conjecture and *severe* tests – precisely the opposite of such ad hoc/defensive behavior. For Popper "the aim of science is to get explanatory theories which are as little *ad hoc* as possible: a 'good' theory is not *ad hoc,* while a 'bad' theory is" (1963, p. 16).

Now although ad hoc adjustments are undesirable in general, one should not be too reckless in eliminating ways that a theory can be adjusted in response to contrary evidence. As Popper explained in his autobiography, he "also realized that we must not exclude all immunizations, not even all which introduced *ad hoc* auxiliary hypotheses" (1976, p. 42). Popper cites the modification of Newton's theory in response to the observed motion of Uranus as an example of an ad hoc adjustment that eventually contributed to the development of the Newtonian program. What is needed is a methodological rule that is strict, but not too strict, in the way it bars ad hoc adjustments. Popper's solution is the requirement of *independent testability.*

By "independent testability" Popper simply means that a new theory should make testable predictions independently of the predictions made by its predecessor.[6] Or to put it more in the Popperian vernacular, the new theory should have *excess empirical* content; in addition to inheriting and correcting the potential falsifiers of the early theory, the new theory must have additional falsifiers of its own.[7] A new theory that meets this requirement will be more bold and "better testable than the previous theory: the fact that it explains all the explicanda of the previous theory, and that, in addition, it gives rise to new tests, suffices to ensure this" (Popper, 1963, p. 242). An independently testable theory cannot be ad hoc in the way discussed previously.[8]

[5] Popper (1963, pp. 33–7; 1976, pp. 41–44).

[6] Popper (1963, pp. 217–20, 240–8; 1972, pp. 191–205).

[7] Recall that Popper defines the *empirical content* of a statement as the set of all its potential falsifiers (1963, pp. 332, 385; 1965, pp. 113, 119–211; 1976, p. 26; and Lakatos 1970, p. 111).

Although Popper's definition of empirical content is sufficient for present purposes, it should be noted that some neo-Popperians find it to be problematic. Watkins (1984, pp. 167–83) discusses the difficulties of the Popperian definition (actually, definitions) and offers his own modification called the "comparative testability" criterion.

[8] Although independent testability is sufficient to rule out cases like T2 in the preceding economic example, it is not sufficient to rule out every type of ad hoc adjustment. To really guarantee that the new theory is non-ad hoc, Popper also requires that some of the excess content actually *be confirmed.* As he says, "This becomes clear if we consider that it is always possible, by a trivial stratagem, to made an *ad hoc* theory independently testable, *if we do not also require that it should pass the independent tests in question:* we

Let us see how the independent testability requirement might be applied to the previous economic example. Suppose that further examination of the anomalous country A reveals that the government of this country was running an extremely large deficit during the years when theory T1 failed to predict inflation accurately. Suppose further that a reexamination of the countries where T1 had been successful reveals that the governments of these countries had budgets close to balanced for the years in question. These observations might suggest a new, much bolder theory, T′2. T′2 would predict the initial result (same as T1) for countries with budgets close to balanced and a different result, the one obtained in country A, for countries with large deficits. Notice that T′2 predicts the same results as T1 where T1 was successful, corrects T1 for the case of country A, and provides independently testable predictions. For example, T′2 tells us that for any as yet unexamined country, we should find different results during years when the country was at war (and thus running a large deficit) than during years when the country was at peace. T′2 has excess empirical content; it leads to "new testable consequences, suggested by the new theory and never thought of before" (Popper, 1963, p. 243).

The last few words of the preceding quotation reveal another aspect of the Popperian response to the ad hocness question, that is, the importance of *novel facts*. In the paper considered to be his most important on the topic of scientific progress (1963, pp. 215–50)[9] *Popper identified independent testability exclusively with the prediction of novel facts.* He says (1963, p. 241), "we require that the new theory should be *independently testable.* That is to say, apart from explaining all the explicanda which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a new kind); it must lead to the prediction of phenomena which have not so far been observed." Popper admits that requiring the new theory to predict "what had never been thought of before" (p. 243) may "sound strange" (p. 247) because it means that whether or not evidence counts in favor of a theory depends critically on "whether the theory is temporally prior to the evidence" (1963, p. 247).[10]

merely have to connect it (conjunctively) in some way or other with any testable but not yet tested fantastic ad hoc prediction which may occur to us (or to some science fiction writer)" (1963, p. 244).

[9] And to a lesser extent in "The Aim of Science" (1972, pp. 191–205).

[10] Not everyone writing on Popper's transition from non-ad hocness to novel facts documents the transition in exactly the same way (see Musgrave, 1974, pp. 3–12; Watkins, 1978, pp. 33–6; 1984, pp. 288–300; and Worrall, 1978, pp. 45–51). Lakatos seems to argue (1978a, p. 172) that there was no transition at all, that Popper always subscribed to the view that non-ad hocness was equivalent to the prediction of novel facts.

Now *if* Popper's novelty requirement for non-ad hocness is strictly interpreted, it has the "unsatisfactory consequence" (Watkins, 1978, p. 34) that "known" evidence does not contribute to the confirmation of a theory, that is, that *only* novel facts matter.[11] It has been pointed out by a number of authors that such a novelty requirement is sufficient to rule out ad hoc maneuvers, but it is not necessary to do so.[12] For instance, in the previous example, if the evidence regarding inflation and governmental deficits were known before T′2 was proposed, those facts would not be novel and would not count as independent evidence for the new theory. Such implications have led several authors to propose new, less restrictive definitions of novel.[13]

In summary, then, for Popper good science is science that makes bold empirical conjectures and exposes these conjectures to severe tests. The ad hoc adjustment of a theory for the sole purpose of protecting it from such a refutation is precisely the opposite of what should occur in good scientific practice. Popper proposed independent testability as a requirement that would prevent such ad hoc adjustments. Because independent testability was associated with the prediction of novel facts, the two concepts, novelty and non-ad hocness, came to be regarded as synonymous.

II. Ad hocness: Lakatos

In his presentation of the "Methodology of Scientific Research Programs" (hereafter, MSRP)[14] Lakatos defines a particular step in the development of a research program (a series of theories with shared hard-core metaphysical presuppositions and heuristic recommendations) as *theoretically progressive* if it "has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact" (1970, p. 118), and *empirically progressive* if some of these novel facts are actually confirmed. Lakatos defines a research program as *progressive* (nondegenerating) if it is theoretically progressive at each step in its development, and empirically progressive at least "intermittently" (1970, p. 134; 1968, p. 170). Notice two things about these definitions. First,

[11] Although this is not the only way that Popper's discussion of novelty can be interpreted, it is a quite common interpretation. For instance, Worrall (1978, p. 46) says, "the Popperian account of empirical support says that a theory is supported by any fact which it describes correctly and which was first discovered as a result of testing this theory; and that a fact which was already known before the theory's proposal does not support it."

[12] Musgrave (1974, p. 12) and Worrall (1978, p. 49).

[13] This changing view of novelty is discussed briefly in Hands (1985, pp. 6–7). Alternative definitions have been provided in Gardner (1982), Musgrave (1974), Watkins (1984), Worrall (1978), and Zahar (1973). Lakatos's emphasis on novelty (discussed in the next section) has been an important factor in the development of this literature.

[14] Lakatos (1968, 1970).

even theoretical progress requires the prediction of novel facts.[15] And second, Lakatos uses the expressions "predicts novel facts," "has excess empirical content," and "is independently testable" as perfect substitutes for one another.[16]

Although Lakatos's notion of progress is tied to the same testability/novelty requirement that Popper used to eliminate ad hoc theory modifications, his actual use of the term differs significantly from Popper's. In fact, Lakatos uses the term "ad hoc" in at least three separate ways.[17] The first of these, which he calls "ad hoc_1," corresponds precisely to the way the term was used by Popper (and previously). A theory is ad hoc_1 if "there is no *independent test* possible for it" (1970, p. 175, note 2). Thus Popper's requirement for non-ad hocness is the same as Lakatos's requirement for nondegeneracy,[18] and the following equivalency holds: "non-Popperian ad hoc" = "non-ad hoc_1" = "theoretically progressive."[19] Lakatos, like Popper, makes non-ad hocness an essential component of his notion of progress in science.[20]

Lakatos's second definition of ad hocness, "ad hoc_2," is only a slight modification of ad hoc_1. He calls a theory (or problem shift) ad hoc_2 if it has excess content but *none* of this "excess content got corroborated" (1970, p. 175, note 2). This definition makes "non-ad hoc_2" = "empirically progressive."[21] Though requiring a theory to be non-ad hoc_2 is technically more stringent than requiring it to be non-ad hoc_1, the difference is only one of degree; ad hoc_2, like ad hoc_1, is fundamentally related to Popper's notion of ad hocness. Both requirements, theoretical and empirical progressiveness, represent ways to prevent the type of face-saving/

[15] Based on what Lakatos actually wrote, this is *not* literally correct. He says that a change is theoretically progressive *if* it predicts novel facts – he does *not* say *only if* it predicts novel facts. Thus the door is (formally) left open for other things that would be sufficient for theoretical progress. Although this *is* an interpretation that can be defended on the basis of what Lakatos actually wrote, it is certainly not the standard interpretation. The standard interpretation is (as above) that progress *requires* novel facts, that is, "if and only if" is read in rather than "if." The standard interpretation is probably more consistent with the overall spirit of the MSRP.

[16] For example, he says "produces novel facts (that is, it is 'independently testable')" (1970, p. 126) and "theories which had no excess content over their predecessors (or competitors) that is, which did not predict any *novel* facts" (1970, p. 175, note 2).

[17] This follows his presentation in (1978a), a paper originally published in 1968.

[18] Actually, of course, Popper requires some confirmation of this excess content (see note 8) and Lakatos requires at least intermittent confirmation.

[19] This equivalence is implicit in Lakatos (1968, 1970); it becomes explicit in later "Lakatosian" works such as Zahar (1973).

[20] "The problem of when a hypothesis-replacement is 'ad hoc', i.e., irrational, degenerating, bad, has never been discussed with more attention and detail than by Popper and myself" (Lakatos, 1978b, pp. 221–2).

[21] Actually, this identity requires a slight modification of Lakatos's definition; see Zahar (1973, p. 101, note 1).

content-decreasing/defensive behavior that Popper sought to exclude. This is not the case, though, for Lakatos's third notion of ad hocness.

Lakatos's third notion of ad hocness, "ad hoc_3," is related to the concepts of a research program's *positive heuristic*. According to MSRP, the hard core of the research program is the set of metaphysical (irrefutable) propositions that define the program and remain essentially unchanged throughout its evolution. The positive heuristic of the program is a set of rules that specify the research plan of the program and its relationship to the hard core.[22] The positive heuristic tells scientists working in the program what types of projects to work on, what empirical tests to perform, and how to interpret the results of those tests.[23]

Given this definition of the positive heuristic, ad hoc_3 is characterized as follows: "The theory is said to be *ad hoc*$_3$ if it is obtained from its predecessor through a modification of the auxiliary hypotheses which does not accord with the spirit of the heuristic of the programme" (Zahar, 1973, p. 101).[24] The absence of such ad hoc_3 stratagems provides a dividing line "between 'mature science,' consisting of research programmes, and 'immature science,' consisting of a mere patched up pattern of trial and error" (Lakatos, 1970, p. 175). It also defines an *alternative type of progress;* "non-ad hoc_3 = heuristically progressive" (Zahar, 1973, p. 101, note 1). The motiviation for defining (and avoiding) this third type of ad hocness is that scientific progress should entail increasingly more unified and cohesive theories; it should not be achieved (though it could be if only ad hoc_1 and ad hoc_2 adjustments are excluded) "with a patched up, arbitrary series of disconnected theories" (Lakatos 1970, p. 175). Banning ad hoc_3 adjustments guarantees the continuity of science and preserves its "organic unity" (Zahar, 1973, p. 105). Thus, when scientific research *programs* are being considered, the previously discussed requirements for *progress* must be revised to include *heuristic progress*. "Within a given programme a theory represents progress if it satisfies three conditions: relatively to its predecessors, it should entail novel predictions; some of these predictions ought eventually to be confirmed; finally, the theory should be structured in accordance with the heuristic" (Zahar, 1983, p. 170).

[22] Lakatos (1968, pp. 170–3; 1970, pp. 134–8) and Worrall (1978, p. 59). Worrall (1978, p. 69, note 35) provides a partial list of the things that the positive heuristic might include.

[23] As an economic example, in Weintraub's (1985a) specification of the neo-Walrasian research program, the positive heuristic consists of propositions like "go forth and construct theories in which economic agents optimize" and "construct theories that make predictions about changes in equilibrium states."

[24] Zahar is quoted here simply because his statement is more straightforward than Lakatos's, which must be extracted from his discussion on pp. 174–7 and 182–7 of his 1970 paper.

This third type of ad hocness is clearly different from the type that concerned Popper (and was discussed previously).[25] It is apparent that even an empirically progressive (non-ad hoc_2) adjustment might still be ad hoc_3. For instance, it may be that T′2, proposed in the previous economic example, is ad hoc_3. Unless we know the macro research program the theory is embedded in, and can show that something in the hard core or positive heuristic of that program suggests that deficits should matter, T′2 could be accused of being ad hoc_3. Lakatos clearly considered program continuity to be extremely important and something that was not guaranteed by Popper's attempts to exclude ad hoc adjustments. For Lakatos, real progress occurs when a research program achieves theoretical/empirical progress "while sticking to its hard core" (1970, p. 187).

Thus there are two separate notions of ad hocness within the Popperian tradition. Ad hoc_{1-2}, or Popperian ad hocness, is the traditional notion of modifying a theory for the sole purpose of avoiding falsification. It can be prevented by requiring independent testability (or testing), that is, the prediction (or confirmation) of novel facts. The second notion, due to Lakatos, is ad hoc_3, and it pertains to the continuity of the program in which the theory is embedded. Ad hoc_3 adjustments can be avoided by requiring that any change be consistent with the core and positive heuristic of the program. Armed with these two concepts of ad hocness, let us now turn to the way the term is used in economics.

III. Ad hocness in economics

The purpose of this section is to compare economists' use of the term "ad hoc" with its use in the philosophical literature: ad hoc_{1-2} and ad hoc_3. In determining the way ad hocness is used in economics, two separate cases will be considered. The first is the way that *economic methodologists* use the term when appraising economic theories, and the second is the way that *economic theorists* (particularly recent theorists) use the term in theoretical discourse. In both cases, what is desired is a generalization regarding "the" representative or paradigmatic use of the term. Given the variety of ways the term is used, particularly among

[25] Though Popper did require a new theory to be "deeper," to have "a certain coherence or compactness" (1972, p. 197) or "simplicity" (1963, p. 241). Although these concepts are related to Lakatos's non-ad hoc_3, Popper was pessimistic about formalizing such requirements and never provided more than an intuitive discussion. Nor did Popper ever directly relate this coherence and simplicity to his notion of ad hocness. Recently, though, such concepts have been analyzed more rigorously (and related to ad hocness) by neo-Popperians, especially Watkins (1984).

theorists,[26] obtaining such a generalization is quite an empirical problem in itself. All that can be done in the current context is to posit a representative use and offer a few quotations/references in its defense. Hopefully this posit will prove to be an "empirically progressive generalization," that is, it will "anticipate" the way the term is used by (as yet) unexamined economic theorists.

There should be no controversy regarding the first group; the evidence clearly suggests that economic methodologists (and philosophers writing about economics) use the term in the traditional Popperian manner. For these authors, a theory is ad hoc if it is deliberately patched up to avoid falsification. For instance, Blaug (1980a) criticizes both human capital theory (p. 238) and the new economics of the family (pp. 242–3) as ad hoc in this traditional sense (despite Gary Becker's insistence to the contrary), and Hutchison accuses Marxists of making "*ad hoc* adjustments and qualifications" (1981, p. 18) to protect the Marxian theory of capitalist development from its predictive failures.[27,28] As yet another example, Rosenberg argues that most of the theoretical developments in the history of neoclassical rational choice theory amount of "largely *ad hoc* qualifications and restrictions that have preserved the theory against a series of failures to empirically substantiate it" (1980, p. 79).

Like methodologists and economic philosophers, economic theorists also use ad hocness in a defamatory way. It is not unusual in recent theoretical literature to find one economist condemning another economist or theoretical approach as ad hoc. This tendency is particularly pronounced in the rational expectations (or New Classical Macro) literature. In this controversial area it seems that "everyone calls the theory of the other ad hoc" (Klamer, 1983, p. 111). For example, in defending their use of the Lucas model, Sargent and Wallace state, "The advantage of Lucas's model is that ad hockeries are given much less of a role."[29] It is quite

[26] Not only economic theorists but natural scientists as well "use the term *ad hoc* to cover a multitude of sins. A theory may be called *ad hoc* because it is unaesthetic and clumsy, because it is arbitrary and uninteresting, or because it is wildly implausible" (Koertge, 1978, p. 267). Grunbaum (1976, p. 361) cites a quite extensive list of scientific uses of ad hocness (compiled by Holton).

[27] Similar criticisms of Marxian economics are made by Blaug (1980b).

[28] Although the ad hocness that concerns economic methodologists always seems to be of the Popperian type, it is interesting that Popperian methodologists such as Blaug, Hutchison, and Klant explicitly discuss the "disease" itself, that is, ad hocness, whereas Lakatosian methodologists are more likely to focus exclusively on the "cure," that is, "excess content" and "novel facts." In either case, there is seldom any recognition that the fundamental issue is the same.

[29] From T. Sargent and N. Wallace, "Rational Expectations and the Theory of Economic Policy," *Journal of Monetary Economics* 2, April 1976; quoted by Maddock (1984, p. 295).

common in surveys of this literature to hear that the assumption of rational expectations is compelling "in comparison with *ad hoc* alternatives" (Perry, 1984, p. 404) or that adaptive expectations, the principal alternative to rationally formed expectations, are simply "*ad hoc* rules" (Begg, 1982, p. 29). Keynesian alternatives to the New Classical Macroeconomics are said to rely on wage and price stickiness, which is "simply an *ad hoc* assumption" (Olson, 1984, p. 299), or they are based on "plausible – but you certainly could say ad hoc – disequilibrium dynamics" (Solow in Klamer, 1983, p. 139). In a few cases the accusatory arrow is even reversed; for instance, Hahn argues that it is the rational expectations theorists who "have chosen just that *ad hoc* model that delivers the goods" (1983, p. 60).

Although accusations of ad hocness seem to fly more freely in the rational expectations literature than in other theoretical areas, they are not exclusive to it. One of the other areas where it occurs is in traditional (Arrow–Debreu) general equilibrium theory. It has been known since the 1960s that in order to guarantee the stability or determinant comparative statics of a standard Walrasian general equilibrium system, it is necessary to impose quite restrictive mathematical properties on aggregate excess demand functions. Such restrictions are often called "*ad hoc* specializations of excess demand functions" (Fisher, 1983, p. 13) or "ad hoc assumptions, imposed directly on the excess demand system" (Hildenbrand's introduction to Debreu, 1983, p. 26).

How then are these economic theorists using the term "ad hoc"? Are they using it as methodologists do, as a claim that the accused theory has been deliberately modified solely to avoid an empirical refutation? The answer seems to be "no." Consider what immediately follows the Sargent and Wallace quotation cited previously. They say, "ad hockeries are given much less of a role and, consequently the neutrality proposition he obtains is seen to be a consequence of individual agents optimizing behavior."[30] Similar statements are made regarding the ad hocness of adaptive expectations. For instance, Begg states, "I argued that *ad hoc* rules such as Adaptive Expectations have the disturbing implication that they allow individuals to make systematic forecasting errors period after period" (1982, p. 29), and this "suboptimal use of available information is hard to reconcile with the idea of optimization" (1982, p. 26).[31] The previously mentioned Keynesian rigidities are not questioned empirically, but because the "assumption [of rigid wages or prices] is

[30] Ibid.

[31] In Begg's introduction to his own antirational expectations model he states, "we shall work with an ad-hoc specification of a macroeconomic model, rather than attempt to derive such a model from explicit microeconomic foundations" (1980, p. 294).

needed to generate the main Keynesian results, yet it is not derived from or supported by any analysis grounded in the motivational assumptions economists have found to be generally applicable" (Olson, 1984, p. 299). Even Solow's disequilibrium dynamics are potentially ad hoc because they are "certainly not the solution of some vast intertemporal optimization problem" (Klamer, 1983, p. 139). These statements do not indicate that nonrational expectations models are ad hoc because they lack empirical novelty or independent testability (though they might); rather, they are ad hoc because they are not derived from individual optimizing behavior. Nonrational expectations models are accused of being *ad hoc*$_3$, not *ad hoc*$_{1-2}$; according to their critics, the assumptions they require *do not follow from the core or positive heuristic of the neoclassical/individual optimization research program,* and that is the source of their ad hocness.

What about criticism going the other way, that is, what about theorists who accuse the rational expectations models of being ad hoc? Are they concerned with ad hoc$_3$ as well? Certainly this is the case for Hahn. The basis of Hahn's ad hocness charge is that Lucas-type models assume equilibrium but fail to explain the presence of that equilibrium on the basis of rational optimizing behavior. According to Hahn, "much importance is attached to rationality until it comes to price changes: then anything goes. For the Lucasians, prices change to keep Walrasian markets cleared by a mechanism that is entirely secret in the Lucasian mind" (1983, p. 54).[32] The accusation here, as in the case of nonrational expectations models, is that the theory in question fails to follow from the positive heuristic of the neoclassical research program, not that it is ad hoc$_{1-2}$.[33]

What about the situation outside of the rational expectations literature? Is the concern over ad hocness only a rational expectations phenomenon, or is it an economic theory phenomenon? Certainly if the

[32] Similarly, "I am in Lucas's methodological spirit when, instead, I propose that prices are flexible when there are no obstacles to price change when it is to someone's advantage to do so. More formally, prices in a given theory are flexible when their formation is endogenous to the theory. . . . Now as a matter of fact, prices in the Lucasian world are not properly endogenous to the fundamental theory, because there is no theory of the actions of agents that explains how prices come to be such as to clear Walrasian markets" (Hahn, 1983, p. 49).

[33] Following a time-honored Lakatosian tradition (Lakatos, 1971, p. 107), theorists who have "misbehaved" relative to the preceding reconstruction are confined to footnotes. One such case is Tobin (1980). In his discussion of Lucas's model, Tobin accuses (p. 42) it of being ad hoc in the traditional (ad hoc$_{1-2}$) sense. In response, Lucas fully admits that it is. "If ever there was a model rigged frankly and unapologetically, to fit a limited set of facts, it is this one" (1981, p. 563). The reader is reminded that the preceding argument tries to offer only an empirically progressive generalization about the way economic theorists use the term "ad hoc," not a claim that it is not used in any other way.

general equilibrium case cited previously is any indication, the concern is discipline-wide. Excess demand restrictions that guarantee stability and comparative statics (gross substitutes being the most common) are ad hoc because they do not follow from the standard assumptions on utility-maximizing agents, that is, they are not implied by the maximization of a strictly quasi-concave differentiable utility function subject to parametric prices. Since it is empirically obvious that not all goods actually exhibit properties like gross substitutability, it hardly seems possible that such assumptions were proposed to absorb a potential falsification. In addition, conditions like zero degree homogeneity and Walras's law are *not* considered ad hoc (though empirically questionable) simply because they *do* follow from the standard assumptions of consumer choice theory (and are therefore non-ad hoc_3 with respect to the neoclassical program).

Thus, for economic theorists, the sin of ad hocness seems to be infidelity to the metaphysical presuppositions of the neoclassical program rather than face-saving adjustments in response to recalcitrant data. Of course, this is not to say that economic theorists do not actually adjust their theories in an ad $\text{hoc}_{1\text{-}2}$ manner,[34] only that recent theorists seem to consider ad hoc_3ness to be a more damning criticism. The methodological implications of this result will now be examined.

IV. Implications and conclusion

One response to the realization that economic theorists and economic philosophers use the term "ad hoc" in different ways is the so-called rhetorical response.[35] According to this view, these differences merely reflect the fact that methodologists and theorists are engaged in different types of discourse; they attempt to persuade different audiences and use different means to do so. Philosophers and methodologists are trying to persuade an audience convinced of the cognitive superiority of "science" that certain areas within economics do not measure up to those exacting standards. Economic theorists, on the other hand, are trying to persuade an audience convinced of the methodological superiority of the neoclassical approach that some particular economic theories do not measure up to its standards. Based on such a rhetorical view, the interesting things about ad hocness pertain to why certain individuals use it in certain ways, what it is about the conversational context that requires one use

[34] Thus, it may be that the preceding criticisms by economic methodologists are correct.

[35] Klamer (1984) provides such a rhetorical view. For him the rational expectations theorists' use of ad hoc_3 is an "epistemological argument" that serves "the new classical economists well to ward off the criticism that their assumptions are unrealistic" (p. 283).

over another, and what political/sociological/psychological factors influence the discourse of the two groups.

Although this rhetorical view seems fine as far as it goes, it does not exhaust the implications of the ad hocness issue. The differences among the various ways ad hocness is used have important methodological implications that go beyond these rhetorical concerns. For one thing, the preceding discussion can help economic methodologists provide a better rationalization of theoretical developments, particularly from a Lakatosian perspective. A good example of this is provided in Maddock's (1984) Lakatosian reconstruction of the rational expectations literature. At one point, Maddock notes an apparent "methodological inconsistency" in the work of Sargent and Lucas: "they rejected ad hoc adjustments to their models which could overturn the basic propositions, but made their own ad hoc adjustments in their endeavor to corroborate statistically those same propositions" (p. 301). This methodological inconsistency seems to dissolve when it is realized that different types of ad hocness are involved in the two cases. In the first case, the "ad hoc adjustments" rejected by rational expectations theorists are ad hoc_3 adjustments (discussed previously) that would introduce parameters not motivated by individual optimization. In the second case, where the rational expectations theorists "make their own ad hoc adjustments," the ad hocness is of the more traditional ad $\text{hoc}_{1\text{-}2}$ type. In this second case, as Maddock clearly documents, there was a rather blatant attempt to account for the empirical fact of "persistence" by adding a (thoroughly ad $\text{hoc}_{1\text{-}2}$) lagged unemployment (or income) term to the right-hand side of the Lucas supply function.

The two uses of ad hocness and the relative importance that theorists attach to each can also help explain Lucas's response to this apparent "inconsistency." Rather than provide a new theory that fits the data without being ad $\text{hoc}_{1\text{-}2}$, Lucas's response was to provide a formal general equilibrium model in which persistence could be derived from informational imperfections. As Maddock explains, "the introduction of a lagged income term into the aggregate-supply equation which had appeared ad hoc could now be justified as arising from within a consistent general equilibrium model with information differentials. As this paper stood it made no new empirical prediction, but it did lessen the impact of a criticism" (1984, p. 302). What it did, of course, was to lessen the criticism from other theorists that the supply function was ad hoc_3. Maddock claims that from a Lakatosian perspective Lucas's move was "defensive" (p. 302); but maybe not. The theory certainly didn't predict any novel facts, but maybe it was still "going forward" in a Lakatosian sense. What we have is a case where the lagged income term was both ad

hoc_{1-2} and ad hoc_3. According to Lakatos's MSRP, *both types* of ad hocness *must be eliminated* to avoid degeneracy.[36] Nothing Lakatos ever wrote specified which type of ad hocness should be addressed first.

This brings up a second but related point about the use of ad hoc_3ness in economics. Although heuristic progress (non-ad hoc_3ness) seems to be precisely what concerns economic theorists the most, it is almost never mentioned in the extensive literature that applies the MSRP to economics.[37] If heuristic progress were seriously considered, it might substantially change the nature of Lakatosian reconstructions in economics. On the positive side, as shown previously in the Lucas case, it may be possible to reconstruct certain theoretical developments as rational (or nondegenerating) by Lakatosian standards, where they would not be if only ad hoc_{1-2} were considered. Lakatos *does* require heuristic progress, and it plays an important role in Lakatosian reconstructions of physical science.[38] If the profession's well-known infatuation with maximization can be reinterpreted as heuristic progress within a neoclassical research program that has individual optimization as a hard-core proposition, the entire history of economics may appear more progressive in Lakatosian terms. Also on the positive side, it seems that *economic theorists* have something like a general neoclassical research program clearly in mind. They seem to have already formed a hard core and positive heuristic, and to know when it is violated. This supports those economists attempting to reconstruct most of neoclassical economics as one big research program.[39] On the negative side, it seems that theorists' revealed preference for heuristic over theoretical/empirical progress (as Lakatos defines these terms) will make it hard to reconcile the history of economic thought with Lakatos's requirement that novel facts be predicted (though they need not be confirmed) at each stage in the program's development. This preference for heuristic progress may explain the paucity of novel facts in the history of economic thought.[40] It may be necessary to modify the MSRP to suit the

[36] "I define a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic" (Lakatos, 1971, p. 125, note 36).

[37] A partial list is provided in Hands (1985), but many more works have appeared since that paper was written (for instance, Maddock, 1984, and Weintraub, 1985a).

[38] For instance, Zahar (1973) argues that Einstein's program superseded Lorentz's program because it was heuristically more progressive.

[39] Clearly, the best example is Weintraub (1985a).

[40] Although the absence of novel facts is a theme in Hands (1985), in all fairness it should be noted that "paucity" does not imply "nonexistence." Maddock (1984) and Weintraub (1985b), for instance, both do a very good job of finding novel facts in the economic programs they discuss.

It may be just a humorous coincidence, but whenever philosophers in the Popperian tradition discuss authors who are critical of novel facts, that is, those who think that facts

needs of economics by "weighting" these two types of progress and finding an optimal way of making trade-offs between them. If this is to be done in a way consistent with the actual practice of theorists, it seems obvious that heuristic progress should be given a relatively large weight. In any case, it appears that philosophers and economists taking a Lakatosian approach to economics (as well as those criticizing it) should seriously reconsider the question of heuristic progress (non-ad hoc_3ness) and its role in the evaluation of economic theory.[41]

Finally, at the end of this discussion of implications (most of which concern the Lakatosian interpretation of economics), it is important to recall the discussion of Popper from Section I. Popper was concerned with ad hoc_{1-2}, with the deliberate modification of a theory for the sole purpose of avoiding a refutation. The concern with independent testability and novel facts only came about as a possible way of avoiding ad hoc_{1-2} theory adjustment. In other words, novel facts are *not* fundamentally interesting to Popper – they are only derivative; they are interesting because they help solve the problem of ad hoc_{1-2} theory adjustment, a problem that *is* fundamentally interesting. Thus, even if we accept Popper's concern over ad hoc_{1-2} adjustments, novelty need not be as significant as recent work would indicate.[42] Of course, the elevation of novelty to an independently interesting concept is due to Lakatos. Although Lakatos's interest in novel facts initiated from the same basic concern as Popper's, it seems to have turned into a novel fact fetishism that lost sight of the original problem. It may be time, particularly in economic methodology, to reexamine Popper's problem: the issue of ad hoc theory adjustment in the traditional sense and what might prevent it. Do traditionally ad hoc adjustments often occur in economics, and if so, how are they problematic? Are there sufficient (or even necessary) conditions that would prevent such adjustments as they occur in economics? Such questions would bring us back to the traditional issue of ad hocness and Popper's problem, something that seems to have been lost in the shuffle of recent methodological discussion.

are facts, regardless of whether they are "known" or "unknown," the two "philosophers" most often cited are J.S. Mill and J.M. Keynes (Lakatos, 1970, pp. 123–4; 1978a, p. 183; Musgrave 1974, p. 2; Popper, 1963, p. 247).

[41] It is interesting to note that in Lakatos's few offhand comments about social science (for instance, 1970, p. 176, note 1), he considers its immaturity, that is, its lack of heuristic progress, to be its primary deficiency. It may be that economists' seemingly irrational concern with "sticking with the program" (even at the expense of novel facts) is precisely what makes economics the most mature of the social sciences.

[42] Recall (from note 4) that not all philosophers think that ad hoc_{1-2}ness is a real problem. It can also be added that within the Popperian tradition, not everyone who thinks that ad hoc_{1-2}ness is a problem believes that predicting novel facts is the way to solve it (Koertge, 1978, p. 269).

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