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Homo Economicus: Alive and Diversified in the Economic Research Program

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No Economics Research Program: The Approach of Gary Becker, Economics Imperialism, and a Call for Pluralism

A Capstone Project Submitted in Partial Fulfillment of the Requirements of the
Renée Crown University Honors Program at Syracuse University

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ABSTRACT

This paper offers a conceptual overview of the methodology of science research programs (MSRP) of Imre Lakatos and germane philosophy of science (Part 1). It applies the MSRP and the lessons learned from the philosophy of science to the neo-classical economics theory and approach of Gary Becker (Part 2). The MSRP framework is used in the pursuit of an elucidating historical analysis of economics, not as a meta-methodology. The pros and cons of Becker's approach are presented in the tradition of instrumentalism and falsificationism and in light of discussion of Milton Friedman, economics imperialism, feminist economics, and the philosophy of science. It is argued that Becker is overly committed to the explanatory power of the economic approach, to the detriment of heterodox alternatives and social sciences pluralism.

KEY WORDS

Philosophy of Science, Karl Popper, Thomas Kuhn, Imre Lakatos, The Methodology of Scientific Research Programmes, Gary Becker, Milton Friedman, Instrumentalism, Falsificationism, Economics Imperialism, Explanatory Unification

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INTRODUCTION

This paper will present and critique a theoretical “nucleus” or “hard core” of the discipline of economics. In Part 1, the philosophy of science work of Imre Lakatos, specifically his methodology of scientific research programs (MSRPs), is introduced in order to frame the analysis of economics. Lakatos’ work in the philosophy of mathematics, as well as the work of Karl Popper, Thomas Kuhn, and other major contributions to 20th century philosophy of science are presented to flesh out and enrich the analysis. It is argued that the MSRP is an inadequate meta-methodology or demarcating framework between “good/scientific economics” and “bad/nonscientific economics” (Hands, 2001, p. 296; Mäki, 2008b). Nevertheless, Lakatos is used because he represents an improvement on Popper and Kuhn and because the MSRP is an effective critical and historical tool.

In Part 2, the MSRP is applied to the methodological and theoretical work of Gary Becker. Becker is presented as representative of neo-classical economic theory and shown to be amenable to analysis by the MSRP. He writes, “The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it,” and equates rational behavior or “rational choice” with maximizing behavior (Becker, 1976, pp. 5, 11-12; Swedberg, 1990, p. 40). Given this theoretical core – consisting of maximizing/rational behavior, market equilibrium, stable preferences – “the economic approach provides a valuable

unified framework for understanding *all* human behavior” (Becker, 1976, p. 14, italics in original).

The plausibility of this statement is appraised in the context of a discussion of economics imperialism. The claim is made that Becker is a dogmatic imperialist, despite minimal concessions to the value of the other social sciences and to epistemic uncertainty (Becker, 1976, p. 14; Becker in Swedberg, 1990, p. 41; Mäki, 2008a, pp. 22-23). Criteria for appraising economics imperialism found in Uskali Mäki (2008) – including metrics of fallibilism and pluralism – are used to determine that Becker’s approach is rigid and exclusionary, and not philosophically or methodologically justifiable, given the cogency of various critiques (Blaug, 1992; Nelson, 1996; Hands, 2001) and the philosophy of science of Part 1.

Throughout the paper, the range and depth of the theory and approach of economics are investigated, with nods to the literature on economic imperialism, instrumentalism, and feminist economics. An assumption of this work, which is not self-evident or uncontroversial, is that economics should be a theory-driven science. Given that there is no consensus-determined theoretical core of economics, support for academic pluralism emerges as the defining normative prescription of this paper.

1.0

The objective of this section is to present a framework for analysis of the theory of economics. That framework is derived from the methodology of scientific research programs (MSRPs), of the late philosopher of mathematics and the sciences, Imre Lakatos. The use of Lakatos is based on precedent. Historians, philosophers, and methodologists of economics, as well as economists, have produced a vast literature on Lakatos and economics which consists of numerous general discussions as well as case studies on “almost every conceivable area of economic thought,” according to D. Wade Hands (2001, p. 287).¹ Instead of focusing on the range of applications, this paper explores the development of Lakatos and the MSRP. To this end, Lakatos’ work on the MSRP is shown in conjunction with his earliest work in mathematics theory as well as influential contributions from Karl Popper and Thomas Kuhn.

Despite the precedent, usage of the MSRP for economics is controversial, and has been severely criticized on methodological grounds, especially recently, by scholars such as Hands and Uskali Mäki (Hands, 2001; Mäki, 2008b). The key criticisms are discussed, and it will be argued that the brunt of the damage is deflected by use of the theory in an avowedly critical and historical manner. The primary concern here is not to establish fit of the Lakatosian MSRP to economics or argue that Lakatosian prescriptions should guide economic research. Instead, the objective is to provide a useful elucidation of the theory and methodology of economics. Hands observes that much of the literature on Lakatos and economics

¹ See Hands 2001, p. 287 for a nearly exhaustive list.

“has provided valuable and independently interesting history of economic thought, but has shed little light on the methodological adequacy of the MSRP” (Hands, 1993, p. 69). Likewise, Mark Blaug points out that Lakatos’ philosophy of science can be seen as “a critical tool of historical research” (Blaug, 1976, pp 349). In that vein, the methodological shortcomings of Lakatos applied to economics do not preclude that application’s capacity to provide valuable insight into economics.

1.1 Finding Lakatos

Just as some scholars deem it requisite to define terms at the beginning of a work, it is similarly necessary to pin down a ‘definition’ of Lakatos. This paper will focus on the work, not the man. ‘Lakatos,’ therefore, signifies the man’s corpus of theoretical and methodological exposition – in papers, seminars, and through direct contact and collaboration with other scholars – and its impact. Elucidating this work is not an easy task, according to Blaug, due to Lakatos’ tendency to “make vital points in footnotes, to proliferate labels for different intellectual positions, and to refer back and forth to his own writings – as if it were impossible to understand any part of them without understanding the whole” (1976, p. 349). In fact, the task is yet more complicated. Rather than merely having to nail down Lakatos, it is first necessary to decide which Lakatos to nail down.

Lakatos is a complicated enough theoretician that scholars have referred to him as if he had multiple personality disorder (Berkson, 1976; Toulmin, 1976).

Berkson writes of iterations of Lakatos in, ‘Lakatos One and Lakatos Two: An Appreciation.’ Toulmin identifies three historical phases of work, the third being an evolved and more nuanced version of the second. Both Berkson and Toulmin refer to a triple Lakatos with the notation, $Lakatos_n$ where $n=1,2,3$ (Berkson, 1976, p. 40). For the purposes of this paper, $Lakatos_2$ will be considered to also include $Lakatos_3$, because they have so much in common. The multiple-personality formulation is not exclusively theoretical, but also influences the university-level teaching of Lakatos.² Two other theorists – Karl Popper and Thomas Kuhn – will also be discussed, for their influence on Lakatos and because they are cornerstones of 20th century philosophy of science.

1.2 $Lakatos_1$

$Lakatos_1$ focuses on the evolution of mathematical theory and spans the years 1961-64 (Toulmin, 1976, p. 658). The 1964 ‘Proofs and Refutations’ is the definitive work of this phase. It is both inheritor and more mature successor of Lakatos’ 1961 Cambridge Ph.D. thesis, ‘Essays in the Logic of Mathematical Discovery.’ That thesis depicts “an imaginary discussion between a teacher and a group of his students which reconstructs the history of the attempts to prove the Descartes-Euler conjecture about polyhedral” (Worrall, 1976, p. 2). Worrall describes it as having great “philosophical and historical value” (1976, p. 2). This is accurate because in the course of the imagined dialogues, Lakatos informally

² The author can attest to this first-hand; he was introduced to Lakatos in this manner in a Spring 2009 class of Economics and Philosophy at the Pontificia Universidad Católica of Chile (Professor F. E. Saffie Kattan, EAE 231A, May 5 & 7, 2009).

sketches a number of the principles which later became central to his work in mathematics theory.

‘Proofs and Refutations’ is a treatise on the development of mathematics, which rejects ‘the deductivist approach,’ characterized by ‘Euclidean methodology’ (Lakatos, 1976, p. 142). In deductivist style, or ‘the conventional view,’ according to Lakatos, “mathematics is presented as an ever-increasing set of eternal, immutable truths” (p. 142). Instead, Lakatos argues, mathematics advances through a sequence of bold conjecture, attempts to ‘prove’ the conjecture (i.e., to reduce it to other conjectures), and criticisms via counter-examples which constitute refutations (Worrall, 1976, p. 2). In this way, postulates which start out as relatively intuitive often evolve into recondite formal structures. Writing of the Euclidian methodology’s deductivist style Lakatos says, “The axioms and definitions frequently look artificial and mystifyingly complicated” (Lakatos, 1976, p. 142). Of theorems, he writes, “It seems impossible that anyone should ever have guessed them” (p. 142). Furthermore, he adds, “The student...is obliged...to attend this conjuring act without asking questions either about the background or about how this sleight-of-hand is performed...In deductivist style, all propositions are true and all inferences valid” (p. 142).

Lakatos is in concord with the view that complicated axioms, theorems, and proofs are indeed necessary for progress in formal mathematics. However, mathematics of this type should be accompanied by the teaching of the history, with emphasis on the ‘problem situation,’ ‘counter-examples,’ and the ‘logic’ which guide theoretical growth and alteration (Lakatos, 1976, p. 144). Theories

are proposed, proven, challenged, refuted, altered, and accepted, and through all of this, they are profoundly changed. The mechanisms and methods whereby these changes are enacted by mathematicians are collectively called a ‘heuristic’ (Lakatos, 1976, pp. 3, 74). The success or failure of theoretical transformations – the evolution or degeneration of theory – depends on the proper functioning of the heuristic.

By asserting that a heuristic exists, is important, and can be studied from a methodological, philosophical, and historical perspective, Lakatos expanded the realm in which philosophy was thought relevant to mathematics. His view challenged that of Karl Popper and ‘the logical positivists,’ who held that philosophy had traction only on the question of justification of theory. To those thinkers, the nature of the discovery of theory and the psychology and internal method of the theoretician were beyond the ken of philosophers of science. Lakatos also made inroads on the formulation of a “new philosophy of mathematics” which “transcended” the three cornerstones of mathematics in the 20th century: logicism, formalism, and intuitionism (Worrall, 1976, p. 3). Berkson praises ‘Proofs and Refutations’ for having “fundamental importance for [the] history...the teaching...the social community...[and the] philosophy of mathematics and perhaps even directly to mathematics itself (Berkson, 1976, p.45).

Lakatos₁ is pertinent to the purposes of this paper in two principal ways: (1) mathematics – hence the theory of mathematics – is fundamental to modern economics; and (2) many elements from Lakatos₁ cross over into Lakatos₂. There

are also lessons to be extracted from Lakatos₁ that are indirectly relevant to academic scholarship and work in the sciences and social sciences, in general.

For instance, by boldly pushing the bounds of the philosophy of science, Lakatos₁ helped make the use of a ‘meta-methodology’ (method for choosing among methods) more palatable and prevalent (Hands, 2001, p. 289).

Toulmin writes that while there “are genuine parallels between Lakatos’ views on these two subjects [mathematics and the natural sciences]...there are also notable divergencies [*sic*] between them” (Toulmin 1976, pp. 657-658).

Berkson goes further: He quips that the shift from Lakatos₁ to Lakatos₂ represents a change in research program³. Though meant to be a criticism, and despite being an exaggeration, the view that Lakatos should be subjected to his own framework – the MSRP – is proper and not uncommon; for instance, economist Mark Blaug does this in his application of the MSRP to economics (1976, p. 367).

Lakatos₂ is best understood as consisting of evolved forms of ideas from Lakatos₁ applied to a new problem. The transition to Lakatos₂ is marked by a change of focus from mathematics to, primarily, the natural sciences.⁴

Thematically, continuity is maintained in that both explore change in scientific knowledge by way of a meta-historical process (Hands, 2001, p. 289). Lakatos₂ is analogous to the methodology of scientific research programs (MSRP) – a theoretical tool which has roots in Lakatos₁ but is usually and rightfully seen as a stand-alone framework.

³ the critique goes even further, by asserting that Lakatos has been “identified with fourteen research programs at least” (Berkson, 1976, p. 40).

⁴ His framework was later extended, by him and others, to the social sciences, including an early and famous application to economics by Spiro Latsis (Latsis, 1972; Latsis, 1976)

The quotation from Blaug which introduced this section, about how Lakatos seems to present his writings – “as if it were impossible to understand any part of them without understanding the whole” – undergirds the holistic approach to Lakatos in this paper (Blaug, 1976, p. 349). In fact, the analysis goes beyond Lakatos, to examine philosopher, Karl Popper, and the American historian of science, Thomas Kuhn. The three scholars conducted a fruitful debate starting in 1965, when the three, but specifically Kuhn and Popper, clashed at a famous conference at Bedford College (Toulmin, 1976, p. 658). A major component of Lakatos₂ consists of critiques of Kuhn and Popper as well as expansions on their work. Toulmin rightly claims that not only is Lakatos influenced by these figures, but that Lakatos’ engagement with their ideas is what generated his shift into a novel realm – the philosophy of science – and sparked the MSRP (Toulmin, 1976, p. 658).

1.3 Catalyzing Lakatos₂: The Influence of Karl Popper and Thomas Kuhn

Lakatos was a proponent of teaching the history and heuristic of mathematics to reveal the method of theory evolution and the totality of propositions. This is the wise and widely-applicable vision of early Lakatos, and it has been an influential pedagogical insight (Agassi, 1976). Lakatos₂ is presented here, according to this prescription. All three – Popper, Kuhn, and Lakatos – have proven influential to economics.

Popper was a mentor of Lakatos at the London School of Economics, and is best known for his doctrine of falsificationism and his work on the ‘problem of

demarcation' between science and pseudoscience (Popper, 1953, p. 39). Popper is remembered for upsetting the prevailing view in the early 20th century on science, which held that scientific hypotheses could be verified. Verificationism, or the verifiability of knowledge, is described as 'conventional' or 'classical,' and often associated with Ludwig Wittgenstein, 'The Vienna Circle,' and 'inductive' or 'logical' positivism (Popper, 1953, pp. 40-41; Blaug, 1976, p. 350). Popper argued against verifiability on the grounds that it is not rigorous enough to serve as a demarcation criterion: Practically any already-formulated theory can be readily verified or confirmed, given a certain narrowness of vision and assiduousness of search (1963, p. 36). Milton Friedman argues against verificationism in "The Methodology of Positive Economics," stating "Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it" (Friedman, 1953, p. 184). Furthermore, according to Popper, science is not only fallible, but objectivity is unobtainable because we "make" observations, we do not "have" them (Hands, 2001, p. 93; Popper, 1972, p. 342).⁵ Therefore, science is open to revision and falsification, and our observation is a result of our interpretation of the world in light of theories. Popper also popularized the problem of induction which undermines verification – and its offshoots, such as probalism (which attempted to assign numerical probabilities to events) – by asserting that "there can be no valid derivation of a law of nature from any finite number of facts" (Lakatos, 1973, p. 2).

⁵ Kuhn is most credited with popularizing the notion of theory-ladenness, however Hands writes that "the basic recognition of the problem...was in Popper's work from the very beginning" (2001, p. 93). See Popper's attack on objectivity, for instance (1963, pp. 46-57).

These critiques were aimed at what is called the Received View, logical positivism, or the logical empiricism program in the philosophy of science (Hands, 2001, p. 72). This vision is often associated with the Vienna Circle and the logical positivists, who “were strictly interested in *a priori* knowledge...sought to combine aspects of logicism and positivism,” and claimed that “two categories exhausted knowledge: the synthetic factual truth of empirical science, and the purely formal analytic truth of logic and mathematics” (Hands, 2001, 73-74). The rest was pseudoscience, superstition, and balderdash. Drakopoulos and Karayiannis write that Popper “gave the initial momentum to the gradual undermining of the positivist approach” and write explicitly of Popper, Kuhn, and Lakatos as post-positivist philosophers of science (2005, p. 52).

Popper’s proposition evades the fallacy of induction by focusing not on verification but on falsification. According to him, a theory is ‘logically falsifiable’ – and therefore scientific – if its theoreticians are willing to specify at least one statement in opposition to their predictions, the occurrence of which, would cause them to abandon their theory (Popper, 1953, p. 36-37). This was a stunning proposition in that it traced the stuff of science to a behavioral origin rather than to a subject matter. Popper also makes the claim that the falsificationist criterion is applicable to the human and social sciences, in addition to the physical sciences (1953).

To illustrate this point, Popper famously juxtaposes the socio-economic theories of Marx and Marxians and the psychological personality theories of

Alfred Adler and Sigmund Freud with the physics theory of Einstein. The former theories – all of which garnered flocks of devotees – are derided as ‘pseudoscientific,’ while Einstein’s expansion on Newtonian mechanics is lionized (Popper, 1953, pp. 34-39). Why? Because Einstein specified possible and feasible experimental conditions which, he conceded, would be capable of refuting his theory. According to Popper, the devotees of Adler, Freud, and Marx – and the theorists themselves – saw their theories as omnipotent in explaining human behavior, and specified no such test condition that would contravene or refute their theories. Popper writes that it was the attribute of Freud, Adler, and Marx’s theories that “they always fitted, that they were always confirmed – which in the eyes of their admirers constituted the strongest argument in [their] favor – ...[that] was in fact their weakness” (Popper, 1953, p. 35).

Lamentably, a great amount of time and effort has been wasted on knocking down a Popperian straw man – what Lakatos calls ‘naïve falsificationism’ (Lakatos, 1970). Blaug describes naïve falsificationism as the view that “a single refutation is sufficient to overthrow a theory” – a golden gun of falsification (1976, p. 350). Naïve falsificationism is a simplistic caricature of the Popperian position.⁶ Popper is better characterized as a ‘sophisticated falsificationist,’ because he implicitly acknowledges what Lakatos calls the ‘tenacity principle’ of scientists (Lakatos, 1973, p. 4). Instead of willingly abandoning theories, scientists are liable to concoct *ad hoc* or ‘rescue hypotheses’

⁶ Lakatos employs this rhetorical fallacy in a radio broadcast in 1973, in which he summarily dismisses naïve versions of both Kuhn and Popper before proceeding to promote his own theory (Lakatos, 1973). However, elsewhere, Lakatos identifies three Poppers – Popper₀, Popper₁, and Popper₂ – which he equates with positions of dogmatic falsification, naïve falsification, and sophisticated falsification, respectively (Lakatos, 1970).

subsequent to an apparent ‘refutation’ (Lakatos, 1973, p 4). Scientists invest human capital in a theoretical framework and method, and are rationally adverse to abandoning that sunk capital and making concessions to competing frameworks. Economists, for instance, are often wedded to formalism in mathematics and quantitative methods, which do not constitute theory, per se, but in which they are similarly invested (Katzner, 2003, p. 564).⁷

Popper condemns the tendency of scientists to ‘immunize’ their theories (Blaug, 1976, p. 354). He differentiates between ‘*ad hoc*’ and ‘*non ad hoc*’ auxiliary hypotheses – a distinction between the rescue hypothesis which band-aids weakness, and the prescient change in hypothesis which adds empirical content to the theory by augmenting its observational consequences. Popper argued that the former – which he called the ‘conventionalist twist’ or ‘conventionalist strategem’ – reduced the scientific status of a theory, even while it obviated refutation in the short-term (Popper, 1953, p. 37). The more risk and audacity theorists evince in establishing grounds for falsification, the more scientific their behavior and theory. If scientists prove willing to abandon their theory, post-refutation, they are meritorious indeed.

Popper’s logic of demarcation is theoretical and historically descriptive, therefore falsifiable. However, Popper also anchored his work to the practical and methodological realm of science by aiming to guide scientists’ choice between and among theories (Hands, 1993, p. 62). It follows that, as a normative methodology, Popper’s falsificationism “cannot be refuted by showing that most,

⁷ Of course, quantitative methods are valuable in economics and the social sciences at large. A question to be answered is whether their hegemony in mainstream economics has hedged out theory, qualitative work, or alternative approaches. This issue is addressed in Section 2.5.

and indeed even all, scientists have failed to obey its precepts” (Blaug, 1976, p. 350). Popper thereby eludes the scope of his own theory.

Popper’s view proved immensely influential, to natural scientists and social scientists alike. According to Hands, Popper was first applied to economics in 1938 by Hutchinson (Hands, 1993, p. 61). Popper eventually lost ground due to the accumulation of severe criticisms against descriptive (i.e., naïve) falsificationism, the inheritance and expansion of his theory by Lakatos, and due to the work and intellectual competition of Thomas Kuhn.

Kuhn’s work is encapsulated in the first edition (1962) and second edition (1970) of his flagship book, *The Structure of Scientific Revolutions*. Edition two of the work includes a postscript, which addresses principal criticisms of the original and introduces substantive revisions and concessions to critics. Kuhn’s work achieved widespread academic acclaim as well as public and commercial success. It generated a new language for the discussion and classification of science. However, that language permeated popular culture and the social sciences more successfully than the natural scientific, philosophical, and methodological academic community.

Kuhn’s work is best known for its stadial theory of science and its mainstream introduction (though not invention) of the terms ‘paradigm’ and ‘paradigm shift’ into the academic and mainstream discourse (Drakopoulos & Karayiannis, 2005). The theory asserts that scientific practice can be broken down into three not-necessarily-consecutive stages: (1) pre-paradigmatic science, (2) normal science, and (3) scientific revolution (Kuhn, 1970). Normal science

resembles ‘puzzle solving’ in which the behavior and methods of scientists in a particular community are governed by established regulations and expectations about methodology, object of study, and theoretical precepts (Kuhn, 1970, pp 35-42). When normal science is absent, there can either be a state of competition, in which numerous theoretical paradigms vie for supremacy, or a period of revolutionary flux, in which an inadequate paradigm is replaced by a more powerful one.

Normal science occurs when a scientific community achieves consensus on a theoretical framework, known as a ‘paradigm’ (Kuhn, 1970). In the second-addition, Kuhn replaced this concept of paradigm with ‘disciplinary matrix’ for reasons of clarity (Kuhn, 1970 p. 182; Drakopoulos & Karayiannis, 2005, p. 54). Either way, the paradigm/disciplinary matrix composes “the entire constellation of beliefs, values, and techniques” of a scientific community/discipline (Kuhn, 1970, p. 175). This provides the lens, or interpretative framework, through which the world is observed (Hands, 2001, p. 103), and this entire framework weighs on and determines observation, thereby supporting the ‘theory-ladenness’ of observation thesis and distorting objectivity. As Philip Kitcher so cleverly phrased it, there are no “out-of-theory” experiences (Kitcher in Hands, 2001, p. 103).

Kuhn’s innovation was to use theory-ladenness in conjunction with his view of paradigmatic science. According to Kuhn a paradigm/disciplinary matrix falters, fails, and is supplanted due to its inability to solve or seriously tackle ‘anomalies,’ which are regularly turned up by normal science (1970, pp. 52-76,

pp.153-154). When those anomalies are more adequately explained by a new theoretical framework, a revolution is in order.

In the original 1962 text, Kuhn proposed a radical idea – the incommensurability or incommunicability of theories (Hands, 2001, p. 103). He claimed that “the normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often incommensurable with that which has gone before” (1970, p. 103). He explains this with the theory-ladenness of observation, insisting that a paradigm shift implies a ‘gestalt-switch’ or radical change in worldview (Kuhn, 1970, pp. 111-114). Therefore, according to Kuhn, “What were ducks in the scientist’s world before the revolution are rabbits afterward” (1970, p. 111). In edition two, he modifies the thesis, and claims that paradigm shifts imply a sweeping change in technical language which results in a communications breakdown (Kuhn, 1970, p 200). This concession softened the incommensurability principle, but left it standing (Hands, 2001, p. 103).

Kuhn’s argument therefore is that that individuals belonging to different scientific communities, although capable of observing the same natural phenomena, would observe them differently (because of theory-ladenness) and describe their different observations with different sets of technical language. Kuhn followed W. V. O. Quine in supporting the ‘radical indeterminacy of language’ thesis – which argued that no ‘perfect translation’ is feasible (Hands, 2001, p. 95; Kuhn, 1970, pp. 198-204). There is no neutral language in which to define troublesome or contested words, however translation between scientists cognizant of the problem is possible, albeit difficult and irregular (Kuhn, 1970, p.

202). Because of this incommensurability or incommunicability, paradigm shifts produce scientific discontinuities (Kuhn, 1970, p. 209). These rank as Kuhn's most controversial assertions.

It follows that, because of these discontinuities, science in Kuhn is not necessarily progressive, where progress is measured on ontological grounds by how accurately or truthfully theory matches "what nature is really like," or "what is really there" (Kuhn, 1970, pp. 206-207). The measure of how 'close' a theory is to the truth is called verisimilitude or truthlikeness (Hands, 2001, p. 282). Verisimilitude is used as a normative criterion for the evaluation of science by those that concede that knowledge of Truth with a capital 'T' is infeasible (Zamorra Bonilla, 2002, p. 350). Popper tries to use the concept in conjunction with his falsificationism, arguing that a theory's resistance to falsification is somehow positively correlated with verisimilitude, and that science was inclined to increase in truthlikeness in an asymptotic way, getting closer and closer to an unreachable Truth or "what is really there" (Hands, 2001, p. 282). Other philosopher's of science never thought much of this (Hands 2001, p. 282), and neither did Kuhn, who said he was "impressed with the implausibility" of the "illusory" notion (Kuhn, 1970, p. 206). Instead, Kuhn uses the measure of a theory's puzzle-solving-ability as a criterion for progress and paradigm-shifting (1970, pp. 205-206). Considering a scientific theory to be an instrument of puzzle-solving, Kuhn insists that later scientific paradigms, more often than not, do a better job at solving puzzles than earlier ones (1970, p. 205-206). He measures this by using first and foremost, "accuracy of prediction; particularly of

quantitative prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved”; and, second tier criteria, “simplicity, scope, and compatibility with other specialties” (206). In this way, Kuhn declares that he is “a convinced believer in scientific progress” (206).

Additionally, much of Kuhn’s insight belongs to what he calls the “sociology” or “social psychology” of science (Kuhn, 1970, p. 8). Hence, while paradigm shifts may be caused at root by epistemological factors (i.e., by an expansion of epistemic content – puzzle-solving capacity), the shift may be slowed or accelerated by psycho-sociological factors (e.g., because “retooling is an extravagance” or thanks to the vim and vigor of a young proselytizing cohort of scientists) (Kuhn, 1970, pp. 76, p. 199). By incorporating these elements into his analysis, Kuhn inserts a human element into the theory of science that is largely absent from the bold and mechanical prescriptive science of Popper. Kuhn uses these arguments to defend himself from the frequent allegation that paradigm shifts are, in his account, akin to “religious conversions” (Lakatos, 1973, p. 4).

Instead, a macroscopic paradigm shift in a scientific community comes about as a sum of individual actions. Scientists choices are ultimately “personal and subjective,” and yet, Kuhn claims, “it is the community of specialists rather than its individual members that makes the effective decision” of paradigm (1970, pp.199-200). This is a bit muddled, but somewhat equivalent to the idea of collective action or “corporate individualism” as expressed by some economic sociologists, such as James Coleman (Swedberg, 1990, p. 52). The distinction

between individual and collective action, of course, is familiar to economists. Some have even argued that Kuhn “is groping toward a cognitive theory,” pointing out that he “picked Gestalt psychology because that was the best thing around in the late 1950s...he was writing before cognitive science had been invented” (Giere in Callebout, 1993, p. 352; Hands, 2001, p. 147).

In the augmented 1970 text, Kuhn admits to gross rhetorical exaggeration and terminological imprecision in the original and dilutes the grandiosity of the theory (Blaug, 1976, p. 351). For instance, he concedes that multiple meanings (at least two important ones) of the word paradigm are present in his initial draft, and he makes drastic language adjustments to update his theory (Kuhn, 1970, 181). More importantly, he concedes the weakness of his stadial theory because “any period of scientific development is marked by a large number of overlapping and interpenetrating ‘paradigms’; some of these may be incommensurable but certainly not all of them are;” and a the paradigm shift is not revolutionary and alacritous, but generational and gradual (Blaug, 1976, p. 352). Although science may usually be ‘normal,’ features from the pre-paradigmatic and revolutionary stages are omnipresent. These concessions reveal incommensurability to be unforgivably exaggerated in the initial text, and sufficiently diluted in the second edition be discounted. Science is not, therefore, subject to utter discontinuities, as Kuhn had initially claimed (Blaug, 1976, p. 351).

Any economist, Blaug argues, would be remiss to take the idea of incommensurability, incommunicability, or discontinuity seriously, given the history of Keynesian economics and *General Theory* as a theoretical adjustment

in economics (Blaug, 1976, p. 360-361). Instead of offering an incommensurate, incommunicable and stunningly novel theory which had the power of inducing a rapid, generational religious conversion, Keynes presented a theory which contained superior justifications and explanations for some policies which economists were already then endorsing (Blaug, 1976, p. 360). Many economists and methodologists who have applied Kuhn to economics, have argued that Keynes constitutes a new paradigm (Coats, 1969; Ward, 1972; Bronfenbrenner, 1971; in Blaug, 1976, p. 357). If this is true, Kuhn's theory, at least in its first formulation, fails to capture the reality and subtlety of the situation. However, whether this example delegitimizes Kuhn, the Keynesian revolution as a revolution, or the usefulness of the application of Kuhn to economics, or all three, can be debated further and must be decided by the interested reader. The uncontroversial point is that radical incommensurability or incommunicability do not fit into this period of change and challenge to orthodox economics.

Historical perspectives from economics and sociology demonstrate similarly how incommensurability or incommunicability is a modifier which operates to degree. Sociologist James Coleman writes about the rift between Gary Becker's economics perspective on crime and punishment and the "traditional" view or "dominant view among sociologists" (Becker, 1968 in Becker 1976; Coleman, 1993, p. 172). While criminologists and sociologists tackled the question from a normative perspective, Becker approached it from the "perspective of positive social science." The normative view was that "no offenders should in principle be permitted, and all offenders should in principle be

punished,” which became the positive question, “how many offenses *should* be permitted and how many offenses *should* go unpunished?” (Becker, 1976, p. 40). Becker transformed the normative declaration into a question of efficiency, scarcity of resources in law enforcement and minimization of the social losses resulting from crime. While the dominant sociologists “saw crime as the simple product of external causes, the criminal as passive clay, and the goal of criminological policy as rehabilitation, Beckerians have used a “rational choice orientation” to show “the deterrent effect of punishment and...the effect of incentives on crime” (Coleman, 1993, p. 172). Becker theorizes on the assumption that “individuals become criminals because of the financial and other rewards from crime,” and bluntly dismisses the alternate view, prevalent during the 1950s and 1960s, “that criminal behavior was caused by mental illness and social oppression, and that criminals were helpless ‘victims’” (Becker, 1993, p. 390).

Coleman asserts that “the previously accepted theory and practice” – which focused on causation, personality, and rehabilitation – has declined while “research which examines the effects on crime of changes in incentives has blossomed, and a rational choice orientation is evident in theoretical work by criminologists” (1993, p. 173). To some substantial degree, therefore, Coleman argues that Becker has achieved his goal – to “dispense with special theories of anomie, psychological inadequacies, or inheritance of special traits” by simply extending the “economist’s usual analysis of choice” (Becker, 1968, p. 40). In short, according to Coleman, Becker has ignited a paradigm shift.

Admittedly, Coleman's argument is not wholly satisfying: he oversimplifies both the economic and sociological positions (as does Becker) and offers only two articles (one in sociology, one in economics) as evidence of the alleged paradigm-shift (1993, p. 172-173). Becker is similarly economical when it comes to substantiating his characterizations of alternative approaches. He cites, for instance, just one source, albeit an allegedly popular one, for the 'victim' theory of criminal behavior (Becker, 1993, p. 390). Coleman also admits that Becker's work has had "little impact on most areas of 'mainstream sociology'" – but Kuhn's theory (in its second iteration) pertains because it is applicable to the large macroscopic and micro sub-sections of disciplines (p. 175). Nevertheless, the example demonstrates Kuhn's influence (be it direct or indirect) on Coleman and the field of sociology, and that Kuhn's postulation of incommensurability or incommunicability is viable if one talks about degree. Kuhn's stadial theory is also challenged, because, in both cases, overlapping, interpenetrating, and cross-pollinating 'paradigms' are evident. Furthermore, these paradigms continue to compete, overlap, and cross-pollinate. No victors have appeared in economics, in the case of the Keynesian 'revolution,' nor in the social sciences in the case of explaining crime and punishment, and they are not likely to appear any time soon.

It is no surprise therefore that Kuhn adulterates the critical points of *The Structure of Scientific Revolutions* in the second edition. His first edition was inconsistent with the histories of social science and science, and is refuted by observation. Blaug writes that the "final version is difficult to distinguish from the average historian's account of the history of science" (Blaug, 1976, p. 352).

Nevertheless, Kuhn was and is influential and responsible for major developments in the philosophy and historiography of science. That which Kuhn pioneered, however, Lakatos perfected. Indeed, Kuhn and Popper are well-understood as greenhorn pioneers, whose work found its culmination and more level-headed, mature realization in the MSRP.

1.4 Lakatos₂

Lakatos developed his ideas strictly in the philosophy of science, and on the methodology of scientific research programs (MSRP) – a meta-methodology (for appraising and choosing among methodologies) – for approximately a decade before he died at the age of 51 in 1974 (Worrall, 1976, p. 11; Hands, 2001, p. 288). Lakatos₂ is cohesive in form, but complex and drawn from numerous articles (Lakatos, 1968-69; 1970; 1971; 1973; 1978). A concise, albeit simplified, sketch of his views on the philosophy of science can be found in a public radio broadcast from 30 June 1973 (Lakatos, 1973). The transcript is a fine introduction to the major elements of the MSRP, and also includes references to Popper (p. 3), Kuhn (p. 4), the problem of induction (p. 1-2), the demarcation criterion between science and pseudoscience, and the ‘tenacity principle’ of scientists (p. 4).⁸

Lakatos’ theoretical framework is positive and historically descriptive, à la Kuhn, as well as normative – a “logic of appraisal” – and prescriptive, à la Popper

⁸ It is worth noting, again, that, in this broadcast, Lakatos unfaithfully depicts Popper as a naïve falsificationist and Kuhn as an advocate of the view that a scientific revolutions is equivalent to “an irrational change in commitment, that it is a religious conversion” (1978, p. 4). These are preposterous caricatures, but not representative of Lakatos’ comprehension and treatment of the two competing scholars elsewhere (Lakatos, 1970; Lakatos, 1974).

(Blaug, 1976, p. 353). Lakatos claims that “the history of science refutes both Popper and Kuhn” (Lakatos, 1973, p. 6), however his work retains an incredible amount of direct influence from both of their positions.

From Kuhn, Lakatos incorporates the insight that the basic unit of science is not the singular hypothesis or theory, but a more complex theoretical framework, which he calls the research program (RP) (Lakatos, 1973, p. 4). Lakatos and Kuhn are responsible for popularizing the notion of a larger unit of science, and guiding the discourse away from “the fallacy of trying to appraise particular theories without invoking the wider, metaphysical framework in which they are embedded” (Blaug, 1976, p. 349). However, as opposed to Kuhn’s vague, all-inclusive paradigm/disciplinary matrix, Lakatos SRP is a more tangible and detailed model.

Skepticism towards the stand-alone theory had a long-established tradition in the philosophy of science prior to Kuhn and Lakatos. Rejection of that position is attributed variably to Pierre Duhem, W. V. O. Quine, and/or Henri Poincaré. Their main argument has come to be known, generally, as the Duhem-Quine underdetermination thesis (Hands, 2001, p. 96; Blaug, 1976, p. 354). The thesis is that “no individual scientific hypothesis is conclusively verifiable or falsifiable, because it is always tested in conjunction with auxiliary statements” (i.e., assumptions or hypotheses) and not in isolation (Blaug, 1976, p. 154).

Lakatos follows Kuhn, who associated a paradigm with a particular scientific community (Kuhn, 1970, pp 3-4), by identifying the RP as a “certain continuity which connects... members” (Lakatos 1970, p. 47). However, Lakatos

avoids rhetorical exaggeration, by making it clear that multiple research programs can and do operate even within a particular field of science at any one time. The SRP, according to Lakatos, has three structural components: (1) a ‘hard core’ of theory, (2) a ‘protective belt’ of auxiliary hypotheses, and (3) a two-part heuristic, or ‘problem-solving machinery’ (Lakatos, 1973, p. 4). Auxiliary hypotheses function as defensive modifications or specifications of hard core principles that inure those principles to refutation. The heuristic – which can be positive or negative – constitutes a set of accepted methodological rules. The positive heuristic “tells [scientists] what paths of research to pursue,” and the negative, “what paths of research to avoid” (Lakatos, 1970, p. 47). More specifically, the negative heuristic redirects assertions of falsity or inadequacy to the protective belt and therefore makes the hard core “‘irrefutable’ by the methodological decision of its proponents” (Lakatos, 1970, pp. 48-50). The positive heuristic is a “set of suggestions or hints” for how to amend, append, and amputate sections of the ‘protective belt’ (p.50). Together, according to Lakatos, these heuristics guide the actions of scientists, so as to make them more purposive and productive in regular actions such as the digestion of anomalies (1970, p. 50). It is important to keep in mind that the hard core of theory is irrefutable or ‘metaphysical’ by the consensus decision-making of a scientific community, as well as the proper functioning of the heuristic.⁹

⁹ For instance, the hard core in Newton’s program consists of the three laws of dynamics and the law of gravitation. Methodological consensus by Newtonians and the negative heuristic make these irrefutable, and the digestion of “anomalies” by the positive heuristic” must lead to changes only in the ‘protective’ belt of auxiliary, observational hypotheses and initial conditions” (Lakatos, 1970, p. 48).

This structural part of Lakatos' MSRP is positive and descriptive therefore open to refutation on whether or not it squares with reality. Lakatos builds on Kuhn and in so doing produces a more sophisticated and unambiguous – yet flexible – framework. However, Kuhn is partially correct in asserting that the differences between the two approaches are minimal, and that the “hard core, work in the protective belt, and degenerating phase are close parallels for my paradigms, normal science, and crisis” (Kuhn in Blaug, 1976, p. 354).

When it comes to the normative – and therefore empirically irrefutable – side of his theory, Lakatos follows Popper, but resolves some of the weaknesses of the Popperian position. For instance, the Duhem-Quine thesis poses a grave problem for Popper, because his falsificationism presumes isolated theories as the units of science (Popper, 1953). Lakatos criticizes Popper on the methodological grounds that falsificationism is not practicable, asserting that refutation by crucial experiment is myth, and that scientists talk about “anomalies [and] recalcitrant instances, not refutations” (Lakatos, 1973, p. 4). Lakatos claims that, because all theories have “unresolved problems and undigested anomalies, all theories are “born refuted and die refuted” (Lakatos, 1973, p. 5).

Lakatos follows Popper, by asserting that there is a normative criterion for evaluating of SRPs as ‘better’ or ‘worse’ (p. 5). Popper identifies audacity, specificity, falsifiability, and prescience as positive attributes of scientific theories, while sweeping generality, ad hoc hypotheses, and unwillingness to specify grounds for refutation are pseudoscientific (Popper, 1953). Lakatos upholds these principles, but makes them secondary to a new criterion for

distinguishing between progressive and degenerating science – the prediction and corroboration of novel facts (Lakatos, 1973, p. 5; Hands, 2001, p. 288).

A particular SRP is theoretically progressive if it makes “dramatic, unexpected, stunning predictions,” and empirically progressive if those predictions are corroborated and lead to a “progressive problem shift” (Lakatos, 1973, pp. 5-6; Lakatos, 1970, pp. 48-49). Degenerating SRPs are characterized by stagnation or “the endless addition of *ad hoc* adjustments that merely accommodate whatever new facts become available,” and can also generate a “degenerating problem shift” (Blaug, 1976, p. 354; Lakatos, 1970, pp. 48-49). This is a cornerstone of Lakatos’ normative methodology of science, and often described as a major departure from the work of Popper. That said, it is worth noting that Popper specified a similar objective – “the search for truth...more than mere truth: what we look for is *interesting truth* – truth which is hard to come by” (Popper, 1960, p. 229, italics in original). Hence, even on this distinctly Lakatosian principle, there is substantial impact from Popper.

Lakatos draws on Kuhn again in his incorporation of socio-psychological factors as catalysts of scientific change. He illustrates a distinction between objective reasons versus 'socio-psychological' reasons for the displacement and succession of SRPs (Blaug, 1976, p. 355). Objective change is characterized by a process in which epistemically richer and heuristically more powerful SRPs explain the content of their predecessors and supersede them (p. 355). In a 1971 article, *History of Science and its Rational Reconstructions*, Lakatos defines this objective change as ‘internal history,’ whereas ‘external history’ describes the

alternative process by which SRPs rise or fall due to some reason other than epistemic heuristic expansion (Lakatos, 1971, p. 102). These include the aforementioned socio-psychological criteria (e.g., elegance, parsimony, accessibility), which are accorded a modicum of value relative to the factors of internal history. Lakatos argued that internal history should be prioritized in explanations of scientific progress, and that external history be employed to demonstrate how science (or scientists) “misbehaves,” in contravention of the precepts of internal history (Blaug, 1976, p. 355).

On an epistemological level, Lakatos makes an appeal for a “whiff of inductivism” to establish the verisimilitude of theories (Lakatos, 1978b, p. 159). He attempts to escape Popper’s pseudoscientific hope that falsificationism is correlated with verisimilitude as well as Popper’s conviction that theory cannot be justified as true or near the truth (Popper, 1960, pp. 230-231; Hands, 2001, p. 282). This principle, however weak, turns “methodological theories of demarcation...from arbitrary conventions into rational metaphysics” (Lakatos, 1978b, p. 165), and is seen as an improvement on the radical anti-inductivism of Popper.

Lakatos₁ taught that knowledge formulations are most accessible when seen in context and as fully evolved and accumulated structures. This analysis has attempted to stay true to that principle. However, Lakatos also advocated grounding theory in reality, through application, and Lakatos liberally applied his own framework to the analysis of scientific research programs. Examples include the discussions of Newton, William Prout, and Neil Bohr in his 1970

“Falsificationism and the Methodology of Scientific Research Programmes,” and his article with Elie Zahar on the Copernican revolution in astronomy (Lakatos and Zahar, 1976). The successive sections apply Lakatos to economics, in order to further comprehend the Lakatosian framework as well as the science of economics.

1.5 Lakatos and Economics

Truth be told, some economists, philosophers of science, and methodologists have recently proven generally skeptical about the methodological adequacy of Lakatos in economics (Hands, 1993; Hands, 2001; Mäki, 2008b). However, it may be that the Lakatosian perspective casts the methodological adequacy of economics into doubt. The philosophical inquiries in this paper inform the appraisal and execution of scientific economics, but they also give good reasons to moderate our confidence in the methods and products of positive economics.

On theoretical grounds, various problems for Popper and Lakatos in economics have to do with the aforementioned Duhem-Quine thesis and the dysfunction of falsification (Hands, 2001, p. 96; Hands, 1993). Falsificationism has been influential in economics so much so that Blaug argues that “Not many [economists] read Popper. Instead they read Friedman... simply Popper-with-a-twist applied to economics” (1976, p. 348).

As has been shown, the Duhem-Quine underdetermination thesis makes the verification or falsification of individual theories impossible, because they

must always be tested in conjunction with auxiliary statements (Blaug, 1976, p. 154). This applies readily to economics, because “the complexity of human behavior requires the use of numerous initial conditions and simplifying assumptions,” which may be outright false, logically unfalsifiable, or logically falsifiable but practically unfalsifiable (Hands, 1993, p. 63). There is also a “giant measure of uncontrolled complexity, leading to striking epistemic uncertainty” in economics calculations, principally because of underdetermination (Mäki, 2008a, p. 22). Underdetermination likewise implies a fatal flaw in the logic of tracing falsity to its source, a process which is called *modus tollens* (Boland, 1979, p. 505). Through *modus tollens*, falsity is passed backwards, such that if a conclusion is found false, it must be that at least one of the underlying assumptions is false. Ignorance about “where to aim the arrow of *modus tollens*,” because of underdetermination and the requisite large number of underlying assumptions, retards scientific falsification in economics (Hands, 2001, p. 96; Mäki, 2008a, p. 22).

Blaug identifies Friedman with Popper because of the claims that economists are blasé about ‘unrealistic assumptions’ and inclined to take the “purely instrumentalist view,” which sanctions such assumptions given “the theory deduced from them culminates in falsifiable predictions” (Blaug, 1976, p. 348; Friedman, 1953, p 183-184). And yet Blaug reduces this, like Popper before him, to a normative contention – that the central weakness in economics lies in the reluctance to produce theories which yield unambiguously refutable implications (1976, p. 363). Friedman counters this view in a Lakatosian manner,

by pointing out the absence of the “‘crucial’ experiment” in economics and the fact that “the weeding-out of unsuccessful hypotheses [is] slow and difficult” and that “they are seldom down for good and are always cropping up again” (1953, p. 186). Not only do scientists have ‘thick skins’ but, as Blaug himself points out, social processes are as opposed to deterministic, such that, given an initial condition, they can lead to multiple possible positions, some of which are more probable than others (Lakatos, 1973, p. 4; Blaug, 1976, p. 350). A dogmatic application of Popper, it has been argued would decimate the field of economics (Blaug, 1976, p. 356).

The falsificationist agenda is therefore a chimera. However, scholars relatively sympathetic to Popper’s falsificationism often find comfort in Lakatos’ less intransigent MSRP. Hands indicates that such scholars hope that Lakatos can “provide a set of relatively strict rules that will constrain, and hopefully cleanse, the empirical practices of economists” (Hands, 2001, p. 294). However, Hands claims that attempts to impose “tougher empirical standards” and extirpate ‘the principle of tenacity’ – what Blaug calls “the fear of an intellectual vacuum” – have categorically failed (2001, p. 295; Blaug, 1976, p. 366).

Furthermore, the Duhem-Quine thesis also implies difficulty in evaluating between different hypotheses as predictors or explanations for certain phenomenon. In his seminal article, “The Methodology of Positive Economics,” Friedman points out that a discreet amount of evidence can be explained by an infinite number of distinct hypotheses (Friedman, 1953, p. 209-210). Hands makes note of the same problem: “There are always a large (perhaps infinite)

number of ways of modifying the test system to make it consistent with the relevant evidence” to the extent that “observation will always support a large (perhaps infinite) number of empirically equivalent theoretical hypotheses” (Hands, 2001, p. 96). This is called the problem of identification, and it implies a certain amount of sloppiness in theory-construction and evaluation.

Of course, falsificationism is not critical to Lakatos. Instead, the crux of science for both Lakatos and Friedman is the production of “meaningful (i.e., not truistic) predictions” and “the prediction of novel facts” (Friedman, 1953, p. 183; Lakatos, 1973, p. 5). Friedman refers to this as “the ultimate goal of a positive science,” whereas Lakatos names it the criterion of progress or degeneration. This crucial component is the vulnerable underbelly of the Lakatosian framework in economics. Hands, for instance, argues that where Lakatos has been applied with fidelity to the study of economics (especially the history of economics), it has been shown that ‘progress’ and ‘novel facts’ are rare if not absent from the history of the economics discipline (Hands, 1993, p. 69). Economist and philosopher Mäki takes a similar position (2008, p. 8). However, both authors point out that the notion of ‘novelty’ is highly contested (Hands, 2001, p. 295; Mäki, 2008b, p. 8). Hands writes,

There are multiple definitions of novel facts in the philosophical (even Lakatosian) literature; the idea of novelty has a long and controversial history in philosophy that seems to raise more questions than answers about the significance of the concept to the

truth, usefulness, meaningfulness, or reliability of a scientific theory; and many contemporary philosophers doubt that novelty (under any of the available definitions) is relevant to the appraisal of scientific theories or research programs (Hands, 2001, p. 295)

Hands sums up by tracing the fetish with novelty from Popper (1963b) through Lakatos, and concludes that understanding of “the current mess regarding novelty...should tell us *to stay away from novelty-based standards*, not make it our sole criterion for scientific progress”(2001, p. 296, italics in original).

Mäki is more sympathetic to the potential value of novelty. He argues that sequences of economic models might provide a “skillfully invented novel way of deriving familiar phenomena from the premises of a standard theory,” and that “this is generally viewed as an important achievement in economics, and it surely qualifies as scientific progress of some sort” (2008, p. 8). He suggests that more work should be done on the issue.

While Mäki hedges on novel facts in Lakatos, he provides other criticisms against a marriage between economics and Lakatos – (1) No clear economic hard core and positive heuristics, (2) no regular production of novel facts, (3) no place for straightforward inductive support by empirical evidence, (4) no role in the MSRP for social institutions and their history, and (5) no account in the MSRP of how progress in terms of novel predictions connects with the goal of verisimilitude (2008, p. 9). This paper does not agree with all of these conclusions completely, nevertheless, it will be assumed that these criticisms are sufficiently weighty to sink the application of Lakatos as “a tool for *the appraisal*

of various research programs within economics” (Hands, 2001, p. 296, italics in original).

Indeed, it has been argued from the beginning that Lakatos is inadequate as a meta-methodology or demarcating framework between “good/scientific economics” and “bad/nonscientific economics” (Hands, 2001, p. 296). This paper has heretofore presented three demarcating criteria – (1) falsifiability, (2) the prediction of novel facts, and (3) the solving of puzzles, pioneered by Popper, Lakatos, and Kuhn, respectively. The Kuhnian ‘puzzle-solving’ approach is most consistent with the instrumentalist outlook of Becker and, especially, Friedman (Kuhn, 1970, 206). And yet, Kuhn’s criterion is not entirely satisfactory (and neither is pure instrumentalism, as will be shown – see Section 2.6) because science is considered to be interested in substantially more than just puzzle solving. Nevertheless, demarcation is nothing to get hung up on. Although Lakatos, Kuhn, and Popper prove not wholly effective, Hands argues that “nothing else in contemporary science does the job [of demarcation] either” (2001, p. 296). Hence, while it may feel tenuous clinging to an inner tube on open waters, one receives some consolation from the knowledge that everybody else must make do with floaties.

Nevertheless, on positive-descriptive grounds, Lakatos is deserving of some acclaim in that “economic theories do seem to have hard cores, protective belts, positive and negative heuristics, and so on (Hands, 2001, p. 296). There are also notions of theoretical and empirical progress in economics, which, although devoid of any novel fact criterion, can be usefully looked at with the help of

Lakatos. It follows that Lakatos is adequate as a “useful analytical tool” for “understanding the *structure* of economics...for *comparing* research programs,” and for accomplishing “more doable jobs, that are more local in character, primarily historical, less arrogant, and perhaps more interesting” (Hands, 2001, p. 296, italics in original; Hands, 1993, p. 69; Blaug, 1976, p. 349).

In this way, Lakatos is used here in a principally historical and critical manner. This application appears also to be inspired by precepts of instrumentalism. Boland states, in the manner of explaining the instrumentalist method, that “so long as a theory does its intended job, there is no apparent need to argue in its favor” (Boland, 1979, p. 508). According to this argument, adequacy is a function of function. The intended job here is indeed modest: to elucidate something about the nature of theory in economic analysis. Friedman would likely assert that such a project belongs to the “art of economics,” an allegation that would be accepted by this author (1953, p. 183). In the next section, the economic approach of Gary S. Becker is presented as a research program and subjected to the Lakatosian framework, in the hope that it will accomplish the intended elucidation.

2.0

Having examined Lakatos and entourage, the methodology of scientific research programs (MSRP) will now be brought to bear on economics. Gary Becker is presented as representative of a neo-classical research program. Attention is turned to justifying the adoption of Becker – an old hand of economic theory and methodology – as an appropriate object of study and representative of that program. Becker’s profound impact on the discipline is demonstrated to justify this role. Subsequently, his methodology (Becker’s preferred term is ‘approach’) and theoretical hard core are examined. In order to examine Becker in an integrated and moderately holistic way, he is discussed in conjunction with his teacher and one of his principal influences, Milton Friedman. He is also discussed with regard to methodological instrumentalism, falsificationism, feminist economics, and the philosophy of science that composes Part 1.

2.1 Justifying Becker

Gary S. Becker earned his Bachelor of Arts (A.B.) at Princeton in 1951, and his Ph.D. in Economics at the University of Chicago from 1954-1957 (University of Chicago News Office, 2007). He subsequently taught at the University of Chicago and then Colombia, before returning to the University of Chicago in 1970 as a Professor of Economics. Becker has since been awarded joint faculty appointments in the University’s Graduate School of Business (called the Booth School of Business since 2008) and in the Department of Sociology.

Among his numerous accolades are the Nobel Memorial Prize in Economic Sciences and the Presidential Medal of Freedom.

Becker is a founding member of the National Academy of Education; a fellow in the American Statistical Association, the Econometric Society and the American Academy of Arts and Sciences; a member of the National Academy of Sciences, the American Philosophical Society and the International Union for the Scientific Study of Population; and a member of the American Economic Association, of which he was president in 1987. All of this goes to illustrate Becker's considerable influence in economics and the other social sciences. Indeed, economist Victor Fuchs asserts that Becker is "one of the most influential social scientists of the second half of the 20th century... [whose] influence on the other social sciences exceeds that of any economist of his generation" (Fuchs, 1994, p.183-184).

Despite his influence, Becker is given short shrift in Hands' comprehensive survey of economic methodology and contemporary science theory, *Reflection Without Rules* (2001). Becker appears only in an indirectly laudatory introductory quotation by Larry Boland (p. 1) and then in a scathingly critical case study in a section about feminist economics (pp. 270-271). In *The Methodology of Economics: How Economists Explain* (1992), Blaug expends more ink surveying Becker's theoretical and methodological contributions – to marginal productivity theory (p. 175), human capital theory (p. 207), methodological individualism (pp. 209-11), and the new economics of the family (pp. 220-228). Although Becker is better known as a working economist than a

methodologist, his influential writing on the “economic approach” (Becker, 1976) does constitute methodology, and is central to his work and influence. The difference between the two presentations of Becker in *Hands* is striking and helpful in showing the controversy around Becker. First, the Boland quotation:

If we were to believe many economic methodologists, particularly those attempting to impress philosophers of science, you would think that all methodologists sit around “appraising” the work of economists. I have a vision of these guys sitting around in priestly robes...passing judgment on people such as Becker, Arrow, Samuelson, Friedman, Keynes, etc. On what basis do they criticize such economists? Do they accuse economists of being unscientific? Who cares? (Boland, 1997, p. 152).

Boland places Becker amidst the most illustrious economists of the 20th century. With the exception of Keynes (which could refer to John Neville or John Maynard or both), these gentleman belong to the 64-strong group of Nobel Laureates in Economics. They are the architects of contemporary, mainstream economics, influential in both the positive-quantitative and normative-methodological discourses.

Paul Samuelson, for instance, produced the two pedagogical texts, *Economics* (1948) and *Foundations of Economic Analysis* (1947), which defined economics education at the undergraduate and graduate levels, respectively, for

the middle decades of the 20th century (Hands, 2001, p. 60). Hands writes of Samuelson and Friedman as the “sum total” of what graduate students in economics learned, in his time, about economic methodology (2001, p. 48). Friedman’s 1953 article, “The Methodology of Positive Economics” is read by economics students in lieu of Popper, according to Blaug (1976, p. 348). Kenneth Arrow made fundamental contributions – i.e., the Arrow-Debreu-McKenzie (ADM) model – to the leading theoretical framework in economics, the Neo-Walrasian program (Hands, 2001, p. 292). J. N. Keynes popularized the notion of a positive-normative bifurcation of the science of economics, while his son, J. N. Keynes, popularized the micro-macro bifurcation (Hands, 2001, pp. 29-30; Hausman, 1994, pp. 33-34). J. N. Keynes is the principal inspirational source for Friedman’s 1953 article on methodology (p 180).

Elsewhere, Becker appears on a list of “the most important works in economics” compiled by the Wharton School economist, Justin Wolfers, in the popular *Freakonomics Blog*. Becker’s book-length titles, *Economics of Discrimination*, *Human Capital* and, *Treatise on the Family*, are listed with Adam Smith’s *Wealth of Nations*, David Ricardo’s *Political Economy*, Alfred Marshall’s *Principles*, Samuelson’s *Foundations*, J. N. Keynes’s *General Theory*, John Von Neumann and Oskar Morgenstern’s *Games and Economic Behavior*, and Milton Friedman and Anna Schwartz’s *Monetary History* (Wolfers, 2010). Wolfers was recently named on the short-list of the most peer-respected and popularly relevant young (untenured) economists (Leonhardt, 2007), therefore his testimony to the modern relevance and influence of Becker is meaningful.

Besides the Boland quotation, the other place that Becker appears in Hands is in a section which explores the work of feminist economics (2001, p. 270-271). Becker, not being a feminist economist, appears as the object of critique. He is used to represent a formalist, neo-classical position, and is subjected to a one-sided and vehement critique of his “economics of the family” or “home economics” by Julie Nelson. Nelson is critical of the economic mainstream (Becker) on the grounds “that the current (androcentric) view is too narrow and overfocused on things that (while important) are not the whole story about economic life” (p. 270). Nelson believes that feminist economists, by bearing a different “socially situated objectivity” can augment and diversify economics theorizing (p. 270). Ironically, it is Becker’s attempt to do just this – augment and diversify economics, by discussing aspects of the family and the home – which Nelson lambasts so thoroughly.

The logic and legitimacy of this attack will be discussed later (Section 2.6). For now, it is important to emphasize the way in which Hands deploys Becker. On the one hand, in the Boland quotation, Becker is shown amongst the economic deities of contemporary neo-classical (orthodox) economics. On the other hand, Becker is opened to derisive criticism by feminist (heterodox) economics and offered no defense. The glaring absence of a fleshed-out Beckerian position (he is given $\frac{3}{4}$ of a page) speaks volumes. Obviously, Hands does not think much of Becker’s methodological or theoretical work. Hands’ quotation from Boland and his unleashing of Nelson offer insight into the role Becker plays in the modern field of economics. Becker is influential but also

controversial, and notably absent from methodological literatures despite his work in methodology. This is a problem-situation worthy of scrutiny.

Has Becker been discounted because he is seen as a radical proponent of a degenerating neo-orthodoxy? Has he been made obsolete by resounding criticism or alternative and progressive programs? If so, why are young, ambitious, and promising economists like Justin Wolfers still putting Becker on a pedestal? Does Becker continue to inspire and influence the attitudes and actions of working economists? Are Becker's theory and methodology consistent with his own work?

In the upcoming sections, the objective will be to shine some light on these questions by examining Becker's theoretical and methodological positions, how he is influenced by Milton Friedman, and his role in economics imperialism.

2.2 Classifying Becker

Becker represents most accurately what is called 'neo-classical economics.' Classification of Becker as neo-classical is supported by Becker himself (Becker, 1976) as well as other writers (Coleman, 1993, p. 169; Hands, 2001, p. 273). In the introduction to the second chapter of *The Economic Approach to Human Behavior*, for instance, Becker describes how his 1957 book, *The Economics of Discrimination*, was initially received by economists and social scientists (1976, p. 15). He writes about how the nay-sayers and skeptics were confused over why or how a "neo-classical type" economist might be studying discrimination. He explained that his work was "an application of neo-classical

economics” to “Discrimination in the Marketplace” (the name of his 1955 Ph.D. dissertation).¹⁰ Other writers refer to Becker as part of the Chicago school (Blaug, 1993, p. 221) or “the standard,” “mainstream,” or “neo-classical mainstream” of economics (Hands, 271-273). These classifications are taken to be analogous.

Naturally, there is some trouble with words like mainstream, standard, orthodox, or neo-classical in economics. They are general stereotypes often used as caricatures or straw men by critics and opponents. And yet the examples cited above confirm the unanimous recognition of their coherence by those who wear these labels. However, there are some limits to their delineation because of the inevitable diversity and controversy inherent in any umbrella category. Such things are best understood as nebulous formations. Therefore, insofar as a neo-classical mainstream exists, Becker is taken to represent it in the 20th and 21st centuries. The question of “What constitutes neo-classical economics?” is thus reduced to “What constitutes the Beckerian theory and methodology?”

Unlike Lakatos, whose texts are labyrinthine, Becker’s work is relatively clear-cut – his writing in “scholarly publications [is] straightforward, almost solemn prose, [which] reflects his intense seriousness of purpose” (Fuchs, 1994, p. 186).¹¹ As such, Becker is open about his theory, methodology, and purpose. However, there are some caveats which should be taken into account. For one, much of the language in Becker’s seminal work is dated, and must be considered in context to be fully comprehended. For instance, the use of the terms “whites”

¹⁰ According to Becker, *Economics of Discrimination* is the seminal text for what has become the field of “minority economics” (Becker, 1976, p. 15).

¹¹ Blaug, conversely, describes Becker’s prose as “humorless” (1992, p. 224).

and “Negroes” in the book, *The Economics of Discrimination*, may appear bizarre or be offensive to contemporary readers. It appears much less so when it is recalled that the book was written in 1957. A more serious criticism which will be explored is that many of Becker’s prescriptions on methodology and theory have been judged inconsistent with his own work (Blaug, 1992, pp. 222-228). And, to complicate matters even further, Becker himself seems to have recanted some of his early and most radical statements (Becker in Swedberg, 1990). These considerations should be kept in mind when discussing Beckerian economics.

2.3 The Economic Approach and the Beckerian Hard Core

Becker most openly details his theoretical and methodological position in the eponymous essay to *The Economic Approach to Human Behavior* (1976). Invoking his teacher Milton Friedman and J. N. Keynes, Becker’s essay is on the scope and method (approach) of positive economics (Friedman, 1953; Keynes, 1891). Becker opens with a discussion of three distinct definitions of economics – (1) the allocation of material goods to satisfy material wants, (2) the market sector, and (3) the allocation of scarce means to satisfy competing ends.¹² This discussion of definitions is employed to eliminate these options as effective tools for distinguishing “the economic approach from, sociological, psychological, political, or even genetical approaches” (Becker, 1976, p. 3). Becker condemns definitions of the social sciences’ subjects on the grounds that they are overly broad, and points out that many kinds of behavior fall within the subject matter of

¹² Becker attributes (1) to Albert Rees (1968) and others (see L. Robbins (1962), (2) to A.C. Pigou (1962 p. 11) and (3) to Lionel Robbins (1962 p. 16) and Albert Rees (1968).

several disciplines (p. 5). Instead, he asserts “that what most distinguishes economics as a discipline from other disciplines in the social sciences is not its subject matter but its approach” (p. 5).

In search of the economic approach, Becker seeks to identify the theoretical givens that guide that approach. These axioms are taken to form the ‘hard core,’ in the Lakatosian sense – a small number of irrefutable, metaphysical beliefs which are defended by the negative heuristic. Becker states that “the combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it” (Becker, 1976, p. 5). These assumptions are highly generalized, and are herein explored in more detail.

“Everyone recognizes,” Becker argues, “that the economic approach assumes maximizing behavior more explicitly and extensively than any other discipline, be it the utility or wealth function of the household, firm, union, or government bureau that is maximized” (Becker, 1976, p. 5). This sentence is all Becker says about this fundamental concept. Enough said, according to Becker. It is hardcore, and it is what one can do with this assumption that matters.

Maximizing behavior is not, however, immediately obvious or accessible. Consider, for a moment, the following apparently simple refutation of this assumption – A person choosing between two discreet commodities of consumption – “Star Wars: The Empire Strikes Back” and “Star Wars: Clone Wars,” say, at the movie rental store – can make a suboptimal choice. In

retrospective contemplation, she might bemoan the decision, and lament, “If only I had maximized my utility properly! How could I have been so stupid?”

This straw man of Becker’s position is shown here to demonstrate how Becker and the economic approach operate – First, a simple assumption is formulated (e.g., individuals maximize their utility). Second, that assumption is applied to an empirical case, and the results are evaluated (e.g., movie renter chooses film that, when consumed, causes boredom and aggravation. Conclusion: utility would appear not to be maximized). Third, more complex concepts consistent with the core assumptions are introduced to adequately explain the behavior (e.g., movie renter chooses to rent movie, watch movie, keep watching movie, etc., because he is ignorant of other options for pleasurable recreation. Therefore he maximizes his utility given a constraint of incomplete information). Indeed, Becker writes that the “assumption that information is often seriously incomplete...[and] costly to acquire” can be used to explain the same kind of behavior that is ‘explained’ as irrational, volatile, traditional, or ‘nonrational’ by other theorists (1976, p. 7). Elsewhere, he notes that “the quantity of information is a complex notion: its degree of accuracy, its multidimensional properties, its variable obsolescence with time are all qualities that make direct measurement of information extremely difficult” (Stigler & Becker, 1977, p. 84). Incomplete information is not the only important constraint which can explain ostensibly non-maximizing behavior, says Becker: “Actions are constrained by income, time, imperfect memory and calculating capacities, and other limited resources, and also by the opportunities available” (1993, p. 386).

Incompleteness of information and these other constraints are taken to be auxiliary assumptions of the Beckerian research program. These are appropriate protective belt assumptions because they do not require metaphysical leaps of faith, but are rather universally accepted (or fundamentally uncontroversial) realities that play a useful role in the protective belt. In this way, Becker plays right into the framework of the methodology of scientific research programs (MSRP). His core assumption is general enough to be applicable to all behavior, but it is augmented by protective belt assumptions which are rich enough to encompass the complexity of human preference, choice, and motivation.

Becker's second assumption at "the heart of the economic approach" is that "markets...with varying degrees of efficiency coordinate the action of different participants – individuals, firms, even nations – so that their behavior becomes mutually consistent" (Becker, 1976, p. 5). This appears almost banal when considering only material and monetary commerce. However, Becker's markets are not limited to material and monetary commerce. Becker extends the application of market analysis, traditionally applied to goods and services, into every dimension of human choice (Becker, 1976, p. 5). Therefore, just as choices with respect to hamburgers, hot dogs, and liposuction are determined in markets, so are choices with respect to spouses (Becker, 1973-1974), discrimination (Becker, 1957), and euphoria (Stigler & Becker, 1977, p. 80). These markets tend towards "equilibrium" (p. 5). The key to the analysis lies in understanding the term 'commodity,' as it is used by Stigler and Becker, and how it is distinct from material goods and services (1977). In "De Gustibus Non Est Disputandum" they

remark that, “people consume commodities, and only indirectly do they consume market goods” (1977, p. 87).

Commodities are “underlying objects of choice...defined over fundamental aspects of life, such as health, prestige, sensual pleasure, benevolence, or envy, that do not always bear a stable relation to market goods and services” (Becker, 1976, p. 5). According to Becker, these are the objects of choice to which preferences refer – as opposed to things like oranges, automobiles, or medical care (p. 5). Of course, oranges, automobiles, or medical care are instrumental in the production of commodities.

This ‘commodity’ is a squishy concept in Becker, but it means that the objects of choice/consumption are sometimes (but not always) more complex than they seem. Commodities are abstractions such as “appreciation for music” and “euphoria” or “social distinction,” in the cases of addiction and fads and fashion, respectively (Stigler & Becker, 1977). The example of fads and fashion illustrates the point (1977, pp. 87-89). Becker and Stigler argue that the choice of some fashionable good, say, Timberland boots, is instrumental in achieving what is truly valued, e.g., “social distinction...[which] might be termed *style*” (1977, p. 88). Style is an underlying commodity that confers value through the utilitarian function-value of the footwear (e.g., foot protection). In Becker’s view, utilitarian value is but one dimension of the value of the good, which is supplemented by less-palpable layers of commodity value.

While the value of a commodity can change, the major argument of “De Gustibus Non Est Disputandum,” as the title would suggest, is that the preferences

or tastes which guide choice cannot change (Stigler & Becker, p. 76). This is the third of Becker's hard core assumptions. It is also identified by Blaug as the "negative heuristic" of Becker's research program (1992, p. 221). For the purpose of justifying this assumption, Becker examines the opposite assumption – that tastes are variable and subject to change. Stigler and Becker describe the assumption of unstable tastes as an impotent theoretical instrument – because of its lack of predictive power – and a cop-out – because it can 'explain away' anomaly (1977, p. 76). In the case of anomalous behavior, postulating a shift in taste would appear to 'resolve' the issue, leaving no other eventuality to be explored and no more to be done. However, this constitutes a capitulation to ignorance. Just as Popper criticized the applied theories of Adler, Freud, and Marx, the fatal flaw of the assumption of changing tastes is that it 'explains' too much, and therefore explains nothing (Popper, 1953, p. 35). Becker and Stigler argue that "no significant behavior has been illuminated by assumptions of differences in tastes" (1977, p. 89).

Alternatively, Becker argues that "the assumption of stable preferences provides a foundation for predicting the responses to various changes" (Becker, 1976, p. 7). He would have economists commit to the belief that individual tastes are "not to change substantially over time," and that tastes are not "very different" between persons, even persons from different classes, societies, and cultures (1976, p. 5). Just as was demonstrated earlier with the movie rental example, given commitment to the stability of tastes foundation, anomalous behavior leads to an exhaustive but potentially fruitful search for explanation.

Becker applies the same argument to a broader spectrum of what he would call other pseudoscientific practices. He writes that “irrational behavior, unnecessary ignorance, folly, *ad hoc* shifts in values, and the like” cover a lack of understanding (Becker, 1976, pp. 11-12, italics in original). Besides values (tastes), the other big-ticket item on that list is ‘irrational behavior.’ Becker and Stigler refer to “stable behavior” as preferable to any theory of behavior that contradicts “personal interest,” and Becker condemns any irrationality assumption on the same grounds as the instability of tastes assumption – that it is an *ad hoc* explanation, not a thoughtful examination of causation (Becker and Stigler, 1977, 82; Becker, 1976, p. 6). Elsewhere, Becker refers to his economic approach as “rational choice” (Swedberg, 1990, p. 40). This open critique of irrationality, and endorsement of ‘rational choice’ would appear to constitute another core assumption, but Becker equates rational behavior with maximizing behavior. He says, “by ‘behaving rationally,’ I mean ‘maximizing’ consistent behavior that looks forward and tries to anticipate as far as possible what the future will bring. This is common to all versions of rational choice that I know of” (Becker in Swedberg, 1990, p. 40). This is effectively a combination of maximizing behavior and the incompleteness of information assumption.

Therefore, the Beckerian hard core remains composed of assumptions of (1) maximizing/rational behavior, (2) market equilibrium, and (3) stable preferences. The incompleteness of information, along with other constraints, is retained in the protective belt, because it instrumentalizes the hard core for explanation and prediction (Becker, 1976, p. 7). This hard core, Becker argues,

functions as an engine for the most formidable and fruitful economics theories and hypotheses: supply and demand theory, the view that competitive markets satisfy consumer preferences more effectively than monopolistic markets, and tax theory (pp. 5-6).

Becker neatly wraps up his economic approach with the assertion that prices and other market instruments “allocate scarce resources, constrain desires, and coordinate actions of participants – in short, they “perform most, if not all, of the functions assigned to ‘structure’ in sociological theories” (Becker, 1976, p. 7). Becker admits that postulating the existence of prices (including “shadow” or non-monetary prices) or costs and incomes “closes or ‘completes’ the economic approach in” an “almost tautological way” (Becker, 1976, p. 7). Indeed, he claims that solutions and predictions to problems of *all* human behavior can be found in the subtle manifestations of prices and incomes (Stigler & Becker, 1977, p. 76). However, the existence of these mechanisms implies the existence of explanations to behavioral questions and anomalies, not that those explanations are easy to obtain or understand. For Becker, the process by which those explanations are teased out of complex social realities by constrained utility maximization, subject to stable preferences constitutes the a “positive heuristic” (1992, p. 227). That heuristic describes the rules of the game, and how economists should go about turning anomalous or under-analyzed behaviors into positively reinforcing parts of the economics research program.

2.4 Methodological Instrumentalism and Milton Friedman

The precepts of Becker's hard core can, upon initial examination, seem tenuous. A typical argument against the assumption of stable preferences, for instance, from Nobel laureate Friedrich Hayek, is that, "if through their interactions in markets and other social institutions humans are constantly gaining new information, it seems hardly likely that their tastes and preferences would remain unchanged" (Caldwell, 2002, p. 290). Similarly, Amartya Sen challenges the feasibility of maximization, and claims that individuals are much more likely to act like "mixed utilitarians" (Sen, 1977, p. 318). Both of these critiques are allegations that the Beckerian or neo-classical assumptions are unrealistic. Sen also points out that "it is possible to define a person's interests in such a way that no matter what he does he can be seen to be furthering his own interest in every isolated act of choice" (p. 322). This describes Becker's maximizing behavior/rationality assumption. For instance, in his Nobel Prize address, he says "individuals maximize welfare *as they conceive it*, whether they be selfish, altruistic, loyal, spiteful, or masochistic" (Becker, 1993, p. 386). Further along, he declares that "The economic approach to the family assumes that even intimate decisions such as marriage, divorce, and family size are reached through weighing the advantages and disadvantages of alternate actions" (Becker, 1993, p. 402). These are clearly tautological (necessarily true, or true by definition) – a potential critique of which Becker is aware (Becker, 1976, p. 7).¹³ However, neither of these allegations – of unrealistic or tautological assumptions – constitutes a

¹³ Becker argues, for example, that the Achilles heel of Jeremy Bentham, who advocated the universally applicable pleasure-pain-calculus (Utilitarianism), was that "he often became bogged down in tautologies...and was more concerned about making his calculus consistent with all behavior than about deriving the restrictions it imposed on behavior" (Becker, 1976, pp. 8-9).

devastating critique of Becker's economic approach. Both methodological instrumentalism and the Lakatosian framework nullify those critiques.

Recall from the discussion of Lakatos that all theories are "born refuted and die refuted" (Lakatos, 1973, p. 5). For this reason, hard core assumptions are assumed to be 'irrefutable' not because they stalwartly defeat all attempted falsifications, but because of methodologically reasoned scientific consensus (Lakatos, 1970, p. 48). By the Lakatosian logic, if core assumptions were not, to some extent, "metaphysical" in nature – taken on faith – there would be no research programs (Worrall, 1976, p. 5). Similarly, dogmatic application of Popper's falsificationism would decimate the field of economics theory (Blaug, 1976, p. 356). By this logic, economics uses unrealistic and tautological assumptions as a mechanism for continued survival.

Instrumentalism provides a different rationale. In Section 1.5, it was argued that economists tend to be blasé about 'unrealistic assumptions' and inclined to sanction such assumptions given "the theory deduced from them culminates in falsifiable predictions" or "sufficiently accurate predictions" (Blaug, 1976, p. 348; Friedman, 1953, p. 188). Therefore, the hard core of theory is taken on faith, but faith conditioned on functionality. This conclusion is famously expressed by Friedman, who postulated that "in general, the more significant the theory, the more unrealistic the assumptions," and asserted that

"A hypothesis is important if it 'explains' much by little, that is, if it abstracts the common and crucial elements from the mass of

complex and detailed circumstances surrounding the phenomenon to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained” (Friedman, 1953, p. 188).

Friedman explains this with his famous “as if” thesis (Friedman, 1953, p. 189-194). He writes, “A meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena,” and the prerogative of the scientist is to predict the behavior of actors “as if they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important” (p. 206).

Passed through Friedman’s “as if” thesis, Becker’s assumption that “participants...maximize their utility” transforms into the assumption that economic actors act “as if” they maximize their utility (Becker, 1976, p. 14).¹⁴ The degree to which the assumption is an accurate description of real motive is not important so long as it serves in producing testable and sufficiently accurate predictions. Indeed, for Friedman, the ultimate goal of a positive science is the

¹⁴ Becker uses this locution in his 1974 article, “A Theory of Social Interaction,” in which he writes, “Not only the head [of the family] but other members too act ‘as if’ they ‘loved’ all members, even when they are really selfish, in the sense that they maximize not their own income alone but the family income” (p. 1063).

production of “meaningful (i.e., not truistic) predictions” (Friedman, 1953, p. 183). And, “the only relevant test of the validity of a hypothesis is comparison of its predictions with experience” (Friedman, 1953, p. 184). The use of explicitly unrealistic assumptions (or any other type of assumptions) in the pursuit of that goal is known as methodological instrumentalism (Boland, 1979, pp. 508-509).

In the same vein, Mäki describes the process of using ‘false’ or ‘unrealistic’ assumptions for the sake of “isolation by idealization” (Mäki, 2009, p. 30). This involves the use of “strategic falsehoods” to “control for noise so as to isolate some important fact, dependency relation, causal factor, or mechanism” (p. 30). Mäki, like Friedman, asserts that this process is used unanimously in theorizing, modeling, and material experimentation by positive economists, social scientists, and natural scientists equally (Mäki, 2009, p. 30; Friedman, 1953, p. 189-194). According to Friedman’s instrumentalism and Mäki’s description of conventional scientific practice, assumptions are not assailable for being unrealistic or tautological. They must be appraised first and foremost on the grounds of prediction production.

Becker is taken to be an instrumentalist because of his statements about the economic approach’s predictive power, and because he is highly influenced by Friedman, who was nothing if not a methodological instrumentalist (Boland, 1979; Caldwell, 1980; Frazer & Boland, 1983; Caldwell, 1992).

On the Friedman connection, not only does Becker dedicate his book to Friedman (“from whom,” he says, “I learned the economic approach”),¹⁵ he also

¹⁵ “To Milton Friedman, H. Gregg Lewis, T. W. Schultz, and George J. Stigler, from whom I learned the economic approach” (Becker, 1976, dedication).

expresses his thanks for comments given by Friedman (Becker, 1976, p. 3). Elsewhere, with regard to influence, Becker writes “If I had to mention one person, it would certainly have to be Milton Friedman...He made me see that you can attack social problems with economics” (Becker in Swedberg, 1990, p. 29). Becker follows Friedman in separating positive (seeking “what is”) and normative (seeking “what ought to be”) economics, and championing the former at the expense of the latter (Friedman, 1953, p. 180).¹⁶ He shows this preference in disparaging remarks aimed at Benthamites and Marxists, who, he claims, “concentrated on what ought to be” and emptied their research of “predictive content in the effort to make it consistent with all events” (Becker, 1976, p. 9). By aiming at what ought to be, he claims, they sacrifice their aim at what is. It is positive economics and the generation of “meaningful predictions” to which Becker dedicates himself (Friedman, 1953, p. 183).

Becker’s writing is in line with the predictive mission of instrumentalism. He repeatedly praises the economic approach’s “foundation for predicting” (Becker, 1976, pp. 5, 7). Stigler and Becker make the claim that their approach “yielded more useful predictions about observable behavior” than alternative approaches, which relied on opposite assumptions (1977, p. 89). Given the prominence of prediction as an objective in Becker, it follows that there must be some method for appraising the results of that prediction (Lakatos, 1976, pp. 3, 74; Lakatos, 1973, p. 4). That logic of appraisal is inspired by precepts of falsificationism.

¹⁶ James Buchanan identifies Friedman’s influential “Essay in Positive Economics” as “the clearest statement of the positivist position” (Buchanan, 1964).

2.5 Falsificationism in Friedman and Becker

Rhetorical evidence of Friedman and Becker's falsificationism is abundant. Friedman argues that "factual evidence can never 'prove' a hypothesis; it can only fail to disprove it" (Friedman, 1953, p. 184). He asserts that when writing of 'confirmation' or 'verification' of a theory, he is signifying the resilience of a particular theory to attempted refutation (p. 184). Therefore, instead of classifying theories as 'right' or 'wrong' or 'true' or 'false,' they are considered to be tentatively 'accepted' or 'rejected.' (p. 184). This is consistent with Popper, who writes about theories which resist all critical efforts at overthrowing them, and therefore "may quite possibly be false" but are "not unworthy of being seriously considered and perhaps even of being believed – though only tentatively" (Popper, 1960, p. 228). Becker writes that "the economic approach has numerous implications about behavior that could be falsified" (1976, p. 10) and identifies the importance of deriving restrictions on behavior from a theory and approach (p. 9). Therefore, Becker endorses falsificationism as a logic of appraisal. As such, it is consistent with his instrumentalism, which provides no explicit method of appraisal of the "testability" or "usefulness" of assumptions.

Becker as a falsificationist instrumentalist is not the end of the story. He also states that "implications" of the economic approach "all appear to be consistent with the available evidence" (1976, p. 10). One could argue that the assertion of "consistence" with facts is, like Friedman's use of the word

‘confirmation’ or Popper’s use of the word ‘corroboration,’ merely short hand for “not yet refuted” (Friedman, 1953, p. 184; Popper, 1953, pp. 57-58). However, it might also be construed as a moderate form of verificationism, based on induction. This is most likely to be the preferred approach of an economist and methodological instrumentalist, who, although cognizant of the philosophical literature, is unlikely to take its most radical positions. Popper is radical in his condemnation of induction, rejection of verificationism as a “program that cannot be carried out,” and faith in falsificationism as an alternative method for “distinguishing rational science from various forms of superstition” (Popper, 1960, p. 228). Becker evidently respects the tenets of falsificationism, and his writing is consistent with the normative criteria of Popper’s doctrine – he writes approvingly of audacity, specificity, and falsifiability of predictions and denounces sweeping generality, ad hoc hypotheses, and unwillingness to specify grounds for refutation (Popper, 1953; Becker, 1976, p. 6). However, Becker is more consistent with Lakatos’ moderate philosophical stance, which approves a “whiff of inductivism” (Lakatos, 1978b, p. 159).

Indeed, falsificationism of the Popperian tradition – even sophisticated falsificationism – has been shown in this paper to be problematic in general, and especially problematic for economics. It was argued in Section 1.4 that it is positively refuted by observation and that it is too ‘hard’ and intractable in its normative prescriptions. Problems revolve around the Duhem-Quine underdetermination thesis, the ‘tenacity principle’ of scientists, the view that all theories are born refuted and die refuted, and the problem of identification. These

problems were shown in Section 1.5 to be especially relevant to economics, because social processes are stochastic and economic calculations require numerous underlying assumptions. Friedman himself provides the Lakatosian argument against falsificationism, arguing that “the weeding-out of unsuccessful hypotheses [is] slow and difficult” and that “they are seldom down for good and are always cropping up again” (1953, p. 186).

In *The Methodology of Economics*, Blaug provides an exquisite exhibition of these ideas in action (1992, pp. 220-228). Blaug is a severe Popperian falsificationist, and he applies that viewpoint to a critical analysis of Becker. His exposition is useful because it demonstrates the foibles of immoderate falsificationism and some of the weaknesses of Becker’s work.

At the beginning of Blaug’s analysis, his tone is hopeful as he points out that the Beckerian core is methodologically oriented towards production of “unambiguously falsifiable predictions” – a trait he traces to Friedman’s ‘as if’ thesis (Blaug, 1976, p. 356) – and determined to eschew “immunizing strategems, as Popper calls them” – the ad hoc reliance on shifting or idiosyncratic preferences that have been discussed many times in this paper (pp. 221, 222). Blaug writes, “It would appear...that the Chicago research program [Becker’s program] is firmly committed, as are few other research programs in modern economics, to the methodological norms laid down by Karl Popper. For that reason, if for no other, the program deserves our attention.” (p. 220).

Blaug then proceeds to provide “provocative comments” on the idea of Becker as a Popperian falsificationist (p. 222). He starts off by alleging that

Becker, who derides other economists for “considerable ad hocery” is almost equally inclined to introduce arbitrary and ad hoc elements into his approach. Becker does so in order to help solve his maximization problems and “produce testable implications”(p. 222-223). Blaug targets three integral areas of his work – his “new economics of the family,” “economics of altruism,” and his “theory of crime,” all from essays in his 1976 book, *The Economics Approach to Human Behavior* (1992, p. 222-223; Becker, 1976, chapters 4, 10, 11 & 13).

Ad hocery can be seen, for example, in Becker’s new home economics or “new theory of the household” (Becker, 1976, p. 169). This approach, pioneered by Becker and others at the University of Chicago, is called ‘new’ because it offers a distinct conception of the household than the ‘traditional’ or ‘conventional’ theory. Becker’s approach describes the home as a multi-person production unit, whose members have interdependent utility functions and coordinate with regard to economic decisions (e.g. about children, marriage, the division of labor, etc.) (Becker, 1976, p. 169). The ‘traditional’ theory, which Becker labels “almost sterile,” assumes “a one person household, maximizing a utility function that is defined on goods and services bought in the marketplace” (Becker, 1976, p. 169; Blaug, 1992, p. 220). In one of his trademark works, “A Theory of Marriage,” Becker uses this theoretical framework to analyze different marriage arrangements (1973-1974). He begins with the positive-descriptive case that monogamous unions predominate over polygamous ones (polygyny, polyandry, and group marriage) in contemporary times (1973-1974, p. 211).

Becker seeks to explain why this is the case and why polygynous marriages – which used to be “common” – have declined over past centuries (p. 238).

To explain contemporary and historical dominance of monogamy, Becker introduces the “rather plausible assumption of ‘diminishing returns’ from adding persons to a household having one man and one woman” (Becker, 1973-1974, p. 211). This assumption and the manner of its presentation reveal much about Becker’s approach in action.

First, by feeling it necessary to justify the assumption as “rather plausible,” Becker demonstrates that, although he is an instrumentalist falsificationist, he is likewise concerned with some verisimilitude or truth condition. Plausibility implies concern for resemblance of the theory to ‘the real world’ or “what is really there” (Hands, 2001, p. 282). Plausibility fits into the spectrum of resemblance criteria, which range from feasibility or conceivability, to plausibility, to persuasiveness, and to credit-worthiness (Mäki, 2009, p. 39-40). If nothing else, this is a pragmatic consideration, employed because Becker is trying to sell his approach to other academics as plausible (in terms of verisimilitude), as well as powerful (in terms of predictions and testability).

Second, such theory adjustments – no matter their plausibility or cogency – are, as Blaug alleges, rather ad hoc and, to some degree, arbitrary. Agnar Sandmo echoes this complaint, with regard to Becker’s 1957 *Economics of Discrimination* (Sandmo, 1993, p. 9). Another ad hoc assumption identified by Blaug is in Becker’s theory of crime and punishment (1968). Blaug argues that the central results of Becker’s theory (e.g. “that offenders are more deterred by

probability of conviction than by the severity of punishment when convicted”) are dependent on “arbitrary assumptions about the preference for risk among offenders” (Blaug, 1992, p. 223). Assumptions of this type, for Blaug, represent a miscarriage of the Popperian legacy, and reveal Becker as a false falsificationist.

From a Lakatosian perspective, Becker’s assumptions (e.g., diminishing returns to scale for husband-and-wife households and certain risk preferences amongst criminal offenders) take on a different light. Instead of arbitrary, they are seen as auxiliary – resident in the protective belt. As such, they are necessary augmentations to the hard core, whose purpose is to protect that core from refutation and specialize the theoretical framework for application to particular problems. Whereas the hard-core assumptions are metaphysical on the grounds of methodological consensus, the auxiliary assumptions are intended to be probed and tested, tacitly approved, rejected, or modified and re-deployed. An assumption of risk acceptance, for instance, is permissible and reasonable in this function.

This understanding of Becker by Lakatos is consistent with the argument presented earlier, from Hands, that Lakatos provides an alternative form of falsificationism, which is less stringent than Popper’s. Lakatos allows Becker to escape from the obdurate regulations of radical falsificationism by “providing a set of relatively strict rules that will constrain, and hopefully cleanse, the empirical practices of economists” (Hands, 2001, p. 294). Furthermore, all attempts to impose tougher standards, of the Blaug and Popper variety, have failed (2001, p. 295). Therefore, not only is Lakatos the logical and inevitable

choice over Popper, but his methodology of scientific research programs (MSRP) is more helpful in describing and analyzing Becker. That said, if one is to apply Lakatos, one must explore the ramifications of that application to their logical end.

The logical end for Lakatos is not the production of testable predictions, per se, but the prediction of logical facts (Lakatos, 1973, p. 5). It is obvious that Friedman and Becker are both concerned not only with predicting, but with predicting novel facts. Friedman writes, for example, about the production of “meaningful (i.e., not truistic) predictions” as “the ultimate goal of a positive science” (Friedman, 1953, p. 187) and identifies the objective of predicting “new facts capable of being observed, but not previously known” (p. 183). Becker is known an economics maverick whose supporters constantly refer to his “originality,” “creativity,” “eagerness to open new areas [of research],” and the like (Sandmo, 1993; MacRae, 1978; Fuchs, 1994; Coleman, 1993). Becker and his followers consider his work to be teeming with novel fact, observation, and application. Sociologist James Coleman writes about how Becker’s work “has involved the use of the paradigm of ‘economic man’ to generate non-obvious deductions” (Coleman, 1993, p. 170). If this were true it would evidence the progressive nature of the Beckerian research program.

Again, Blaug provides the dissenting viewpoint. Not only does he suggest that novel facts are hard to find or non-existent in Becker, he points out that Becker’s writings “lend themselves all too easily to caricature because they employ a cumbersome apparatus to produce implications that are sometimes

obvious, if not banal” (1992, p. 223). According to Blaug, Becker’s approach consists largely of dressing up intuitive or well-known conjectures about human behavior with fancy and unwieldy formal mathematics, and calling them “predictions.” Though these predictions are described as testable and falsifiable, the economic approach is often guilty of being so general that it is “compatible with almost any finding” (Blaug, 1992, p. 224) – the same critique Becker has of “allegations of irrational behavior, unnecessary ignorance, folly, ad hoc shifts in values, and the like” (1976, p. 12), and the same critique Popper levels at Adler, Freud, and Marx (Popper, 1953, p. 35)

The abovementioned case of monogamy illustrates the critique. While it might be ‘rather plausible’ to postulate diminishing returns from joining numerous men and women together in one household, it might be equally reasonable to postulate the opposite if polygamous households were the norm (Becker, 1973-1974, p. 211). Assuming the former, therefore, depends on “cultural constraints on gender-role behavior” (p. 224). If the world were different, perhaps the assumption would be different.

In this sense, Becker’s assumptions are seen by Blaug as arbitrary, derived from an inductive perspective on social relations, and pseudoscientific. Not only are Becker’s observations theory-laden, but his text and formal mathematical models bear the imprint of those theory-laden observations (Kuhn, 1970). The source of the arbitrariness of assumptions in Becker is his own worldview. Hence, statements such as “monogamous unions – one man married to one woman – predominate because it is the most efficient marital form” and “the

physical and emotional involvement called ‘love’ is also primarily between persons of the opposite sex” (Becker, 1976, p. 210). Although these assumptions are plausible, they are not necessarily persuasive or credit-worthy, given their origin in Becker’s worldview (Mäki, 2009, pp. 39-40). Blaug writes, “Becker’s writings are positively infected by...verificationism: we begin with the available evidence about human behavior in areas traditionally neglected by economists and then congratulate ourselves that we have accounted for it by nothing more than the application of standard economics logic” (1992, p. 226). Becker also makes the claim that economic approach outperforms any alternative (1976, p. 5; 1993, p. 402), but neglects substantive documentation of those alternatives (1992, p. 226).

Blaug provides many crucial insights into the work of Becker, but his critique is not fully robust because it lacks fair substantiation. He cherry-picks Becker’s work for conceited, simplistic or tautological, and ridiculous statements, such as, “a person decides to marry ‘when the utility expected from marriage exceeds that from remaining single’” and “caring can strikingly modify the market allocation between married persons” (Blaug, 1992, pp. 223, 226). Blaug’s analysis also suffers from overconfidence in falsificationism. Blaug puts all of his eggs in the falsificationist basket, decreeing that “the central weakness of modern economics is in fact the reluctance to produce theories which yield unambiguously refutable implications” (1976, p. 363). He makes the claim that there is a major inconsistency between the economic approach in theory and the economic approach in action (Blaug, 1992, pp. 220-221). However, because

falsificationism has been shown to fall short for economics, Blaug's 'provocative comments' on Becker are illuminating, but not totally convincing (Hands, 2001, p. 295). There is no refutation without a superior solution.

Alternatively, the Lakatosian MSRP turns Becker's ad hoc assumptions into positive contributions to the protective belt. Yet, Blaug's critique can be extended to Lakatosian falsificationism, however, and Blaug alleges that Becker fails to predict novel facts (Blaug, 1992, p. 223). If one adheres to the rules of the Lakatosian MSRP for methodological adequacy, one is forced to revert to a softening of the 'novel fact' criterion to validate Becker's approach. Recall from Section 1.5 that Mäki identifies value in a "skillfully invented novel way of deriving familiar phenomena from the premises of a standard theory," and suggests that "this is generally viewed as an important achievement in economics, and it surely qualifies as scientific progress of some sort" (2008, p. 8). Becker does at least do this.

A softening of the novel fact criterion sets the stage for ever more expansion of the economic approach to new ambits of analysis, so that the standard economic theory and approach can skillfully be applied in order to derive familiar phenomena in a novel way (Mäki, 2008b, p. 8). Economics of this type, outside of the ambit of traditional economics, has been rampant and controversial, and has been referred to as economics imperialism.

2.6 Becker's Imperialistic Economics

This section introduces the phenomenon of economics imperialism, Becker as an economics imperialist, and a feminist critique of Becker and neoclassical economics (mentioned in Section 2.1). The objective is to provide a two-way window between economics imperialism and Becker, for purposes of further elucidating Becker. The first two parts are intended to flesh out the analysis of Becker and trace his methodological propositions, given their considerable influence, to their logical end. The feminist critique is introduced to demonstrate some of the weaknesses of mainstream/Beckerian economics and an alternative approach.

The case has been made that much of Becker can be traced to Friedman, including his penchant for instrumentalism, falsificationist rhetoric, and his focus on positive economics over normative economics. Another central component of Friedman in Becker is the objective to “‘explain’ much by little” (Friedman, 1953, p. 188). Becker advocates the economics approach over and over again as “comprehensive” (1976, p. 8) and a “unified framework” (p. 14) for “understanding *all* human behavior” (p. 14). This could be considered a sweeping extension of the search for “novel facts” (Lakatos, 1973, p. 5), but is more accurately viewed as an attempt at a larger, more philosophically acceptable, and more interesting objective in science – the unification of explanation (Mäki, 2008a; Sen in Swedberg, p. 264). Becker and neo-classical microeconomics are would-be unifiers of explanation. Becker writes, “The rational choice model provides the most promising basis presently available for a unified approach to

the analysis of the social world by scholars from different social sciences” (1993, p. 403).

Prediction is the first-order evaluative criterion for Friedman, but secondary and tertiary criteria are also important (1953, p. 85). Principal among these are simplicity – which describes theories that rely on a minimum of assumptions – and fecundity – which denotes precision, magnitude, and the maximization of lines of investigation which are produced by a particular hypothesis. Indeed, the considerations of power and fecundity are the metrics of the economic approach’s ability to make predictions. This gets to the root of what Becker and Stigler mean when they trace the unequalled “power” of the economic approach, and what Becker means when he advocates a theoretical framework of three basic assumptions that is “comprehensive” and “applicable to all human behavior” (Stigler & Becker, 1977, p. 89; Becker, 1976, p. 8).

Becker points out that he is not alone even among economists in attempting to formulate a unifying approach and theoretical framework. He claims that Adam Smith, Jeremy Bentham, and Karl Marx had similar intentions (1976, p. 8-9). Mäki extends the list, arguing that “the search for a parsimonious and explanatory powerful set of core principles has been a dominant line of theoretical research in economics from Nassau Senior to a series of Nobel Laureates as diverse as Paul Samuelson, James Buchanan, Gary Becker, and Robert Lucas” (Mäki, 2008a, p. 7). He adds that “most scientists and most philosophers of science believe that one respectable, if not the most respectable, species of scientific achievement amounts to expanding the domain of phenomena

explained by a given theory, or, even better, by an increasingly parsimonious theory” (2008, p. 3).¹⁷ This is consistent with statements by economists such as Sandmo, who writes that Becker’s core of assumptions as defensible and valuable by virtue of its “simplicity and its power to generate fruitful hypotheses” (Sandmo, 1993, p. 10). Even a severe critic of Becker, Amartya Sen, writes approvingly of Becker’s “project of trying to unify the analysis in the social sciences,” adding that “the project of unification is right; the particular method of unification Becker has chosen is not” (Sen in Swedberg, 1990, p. 264). While unifying explanation is relatively uncontroversial, extending the economic approach into the traditional terrain of other social scientists is highly contested.

Economics imperialism is the tendency of economists to “deal with domains of phenomena that previously were not generally perceived as ‘economic’ but are now analyzed in economic terms” (Mäki, 2008a, p. 2). Gordon Tullock (1972) might have been the first to identify the growing imperialistic tendency, but many authors have documented and reacted to the phenomenon since then. Economist Justin Wolfers writes:

There’s no doubt in my mind that economics is currently the queen of the social sciences. Economists have invaded intellectual territory that was previously the exclusive domain of our friends in sociology, political science, criminology, geography and, to a

¹⁷ Parsimony is important because of underdetermination, the problem of identification, and the task of tracing a conclusion’s falsity to its source, all of which apply in especially high degree to economics because it is a social science (Hands, 2001, p. 96; Boland, 1979, p. 505; Mäki, 2008, p. 22).

degree, history. While the President regularly consults his Council of Economic Advisers, I'm not aware of him seeking the counsel of parallel councils of sociological, historical, or psychological advisers...Popular discussion of economics trumps that of the other social sciences. *Newsweek* even declared economics "the sexiest trade alive"(Wolfers, 2010).¹⁸

If economics is taken to be an imperial science, Becker is part of the vanguard and one of its intellectual leaders (Wolfers, 2010; Mäki, 2008a; Sandmo, 1993). Mäki describes him as "the arch imperialist" (Mäki, 2008a, p. 22). Agnar Sandmo writes, "Gary Becker has consistently strived [*sic*] to extend the domain of economic analysis...[and] contributed to the movement which some have characterized as 'economic imperialism'...Becker is one of the most outstanding of the imperialists" (1993, p. 7). Fuchs argues that Becker has expended "efforts to make economic theory the central paradigm of all social science" (1994, p. 191).

Fundamental to Becker's unification is the argument that all behavior is economic and economic denotes the potential for explanation by the economic approach. As he says, "human behavior is not compartmentalized...all human behavior can be viewed as involving participants who maximize their utility from a stable set of preferences and accumulate an optimal amount of information and

¹⁸ Note the distinct deployments of imperialism associated with economics in this passage. The first is academic imperialism, the other is political and popular prominence and power of economics. Mäki calls the former "economics imperialism" and the latter "economic imperialism" (Mäki, 2008, p. 2).

other inputs in a variety of markets” (Becker, 1976, p. 14). Becker would replace “special theories of anomie, psychological inadequacies, or inheritance of special traits” with “economist’s usual analysis of choice” (Becker, 1968, p. 40) – what Becker calls “rational choice” (Becker in Swedberg, 1990, p. 40) or what Coleman calls “the explanatory logic of neo-classical economics” (Coleman, 1993, p. 169). This is explicitly imperialistic, as opposed to merely expansionist, in that it seeks to better explain social phenomena historically associated with another social science, and in the process, “dispense with” the inferior theories of sociology, psychology, and anthropology (Mäki, 2008a, p. 11).

Becker has done expansionist-imperialist work in the economics of various social problems – discrimination, fertility, addiction, human capital, marriage and divorce, the family, crime and punishment (Swedberg 1990, p. 72).¹⁹ These forays into “new areas” involving “allegedly ‘noneconomic subjects’” have often been met with “skepticism, outrage, and scorn,” says Fuchs, and “some criticisms [of Becker] have been constructive, but many have been mean-spirited and often contradictory” (1994, p. 187).

One area where Becker has received vigorous opposition is in the “economics of the family” and “home economics” (Becker, 1976, Ch. 6; Becker, 1981). In the comparative statics results of his household utility model, Becker argues that “one sex naturally has more household capital than the other and, thus, is naturally better suited for specialization in household production” (Becker, 1981 in Hands, 2001, p. 271). Due to comparative advantage and a “reasonable

¹⁹ To see the distinction between the economic and ‘traditional’ sociological approaches, examine the crime and punishment case in section 1.3, where the juxtaposition was presented in light of Kuhn’s notions of incommensurability and incommunicability (Coleman, 1993).

assumption about the efficiency of specialization,” it follows that women are more likely to specialize fully in childbearing – in which they have a natural aptitude – and associated household tasks, leaving men more likely to specialize fully in labor market participation (Coleman, 1993, p. 173-174). Coleman points out that this and other conclusions of Becker’s socio-economics of the family “were so directly at variance with the position of recently emerged feminist theory in sociology that his work was taken as a challenge to be defended against” (1993, p. 173). This is the work which is lambasted by the feminist economist Julie Nelson in *Hands’ Reflection Without Rules* (see Section 2.1) (Hands, 2001, pp. 270-271). Nelson is severely critical, but emblematic of the more sophisticated, constructive, and reasonable side of the criticism against Becker and neo-classical economics.

As an imperialist, Becker is wont to criticize narrowness of vision and scholarly conservatism, opining that “most economists stick to traditional problems partly because they don’t want to look ridiculous” (Becker in Swedberg, 1990, p. 42).²⁰ He points out that his work in the “new theory of the household” has helped transform the household “from a sterile field in economics into one of the most exciting and promising areas” (Becker, 1976, p. 168). Nelson turns Becker’s statements around and asserts that Becker, despite his liberal application of the economic approach, is the one propagating the narrowness of vision.

²⁰Although Becker was correct in making such statements during the time of his seminal and pioneering work in economics (between 1954 and 1976) (such as, “many economists are openly hostile to all but the traditional applications” (Becker, 1976, p. 11)), he was almost surely exaggerating at the time of the Swedberg interview (prior to 1990), and this view has certainly become an outdated, if we are to take accounts of economics imperialism seriously.

She takes aim at Becker on two levels – the epistemological and the methodological. She criticizes Becker as a purveyor of “epistemological machismo,” and argues “that the current (androcentric) view is too narrow and overfocused on things that (while important) are not the whole story about economic life” (Hands, 2001, p. 270). It is worth emphasizing that the Nelson’s argument is not that there is a “particular feminist way of knowing, or even that feminist economics would necessarily produce something totally different” (Hands, 2001, p. 270). Instead, she makes the claim that economics is limited by having a unitary and hegemonic worldview and system of value patterns – one that puts “undue emphasis on masculine-associated traits and experiences” (p. 150). Feminist economists, by bearing a different “socially situated objectivity,” and by deploying a distinct methodology, can broaden, diversify, and thus improve, economics theory (Hands, 2001, p. 270). In this way, Nelson is an expansionist, but not exclusionary, over-critical, or a radical revisionist.

This critique is consistent with Blaug’s argument that Becker uses ‘theory laden’ observation and inductive verificationism to fashion his theory, test that theory, and then assert that it is “consistent with the available evidence” (Kuhn, 1970, pp. 111-114; Becker, 1976, p. 10). Theory ladenness is another way of asserting that scientific objectivity is unobtainable, and is in line with Nelson’s notion of “science as socially constructed, and scientists as social beings” (Popper, 1972, p. 342; Nelson, 1996, p. 39).²¹ These concepts constitute a critique of positivism and scientism (and Becker) – but not science, per se.

²¹ This idea, and Nelson’s exposition, is inspired by the sociology of knowledge. For example, see Berger & Luckmann (1966).

Despite adhering to the scientific method and striving for objectivity, all scientists bear an inherent and personal worldview, which permeates their work. This is what Nelson and other feminist theorists describe as a socially situated objectivity (Hands, 2001, p. 270).

This argument implies that the positive-normative divide in economics is a myth, and all positive economists are also, to some degree, normatively influenced. To this end, economist Joseph J. Spengler suggests that “to postulate an opinionless economist is to postulate an empirically inexistent creature” (Spengler 1934, pp. 330-331). Of course, just as incommensurability and incommunicability were shown to apply more strongly or weakly (Section 1.4), so do the normative and positive designations.

Nelson argues that economists do not take their limited objectivity seriously into account. When it comes to methodology, Nelson alleges that “a concern for objectivity has been allowed to degenerate into a rigid objectivism, and a concern for reliable explanations of human behavior has been allowed to collapse into a dogmatic focus on constrained maximization” (Nelson, 1996, p. 150). The “standard” or mainstream (i.e., Beckerian/neo-classical) approach is “obsessed with formalism” and “exhibiting the power of microeconomic theory [but] provides no real understanding of the social forces (even economic forces) that undergird the institution of the family” (Nelson, 1996, p. 66; Hands, 2001, p. 271). Nelson’s analysis would “start with a much more general economic concept – the notion of ‘provisioning’ – and then proceed to an analysis that recognizes that families consist of ‘persons-in-relation’ and that family decision making

involves a ‘process’ that is not amenable to choice-theoretic analysis” (Hands, 2001, p. 271). Hands seems to agree that the feminist analysis can “provide a much richer and more in-depth understanding” than the traditional approach (Hands, 2001, p. 271).

Nelson’s methodological critique here revolves around investment in mathematical formalism and over-confidence in the power of explanatory unification. These two traits, which allegedly permeate mainstream economics (and drive economics imperialism), contribute to what the insularity of the mainstream and its reluctance to give credence to alternative approaches (or worldviews). This harkens back to Lakatos’ statements about the ‘thick skins’ and ‘tenacity’ of scientists, both of which drive scientific conservatism and adversity to reform (Lakatos, 1973, p 4).

Nelson would have the feminist analysis help make economics “flexible as well as hard, contextual as well as logical, human as well as scientific, and rich as well as precise” (Nelson, 1996, p. 150 in Hands, 2001, p. 271). From this statement, it appears that Nelson considers the Beckerian/neo-classical approach to be hard, logical, scientific, and precise. The issue then is that the typical mainstream approach is too hard, logical, scientific, and precise to accurately capture the sophistication of something as complex as the decision-making process of the family unit. While formal mathematics and logic – tautological systems which may be the closest things to a perfectly translatable language (Friedman, 1953, p. 186) – are designed to formulate only ‘hard’ and un-fuzzy

statements, Hands and Nelson argue that they fail to capture utmost complexity and therefore fall short of adequate explanation.

It is left to be wondered, however why economists persist in the use of formal mathematical modeling and quantitative methods. An explanation might be that tooling up in empirical methods (say, acquiring a Ph.D. in economics) serves as a litmus test for aptitude, which demarcates and distinguishes economics, to some degree, from other social sciences. The problem, if there indeed is one, is that quantitative methods are *too* seductive and sexy. Recall the Wolfer's statement on economics as "the sexiest trade alive" (Wolfers, 2010) and the allure of Becker's formalism. Perhaps quantitative methods are seductive enough to close the minds of empirically trained economists to alternative approaches, such as the more qualitative and broad-spectrum approach of Nelson's feminist economics.

Indeed, this is the claim that Nelson makes, although she does not reject the value of empirical methods. In fact, she endorses quantitative methods, just not their typical deployment or level of influence. She believes that there is an excessive level of investment in the neo-classical mainstream and in formal mathematical approach. She takes on Becker, the arch imperialist, because he encapsulates the narrow-mindedness and hegemonic power of the economic approach, with all its quantitative prestige and mysticism, and in its campaign for explanatory domination.

Nelson stands against economics imperialism as an academic and economics pluralist. She argues for more intellectual capital investment in

alternative (non-mainstream) approaches to economics (e.g., feminist economics). However, the crucial strength of Nelson is that she does not reject the value of the mainstream or quantitative driven approach, even though she critiques them. For her, feminist and neo-classical economics are can mutually enrich each other. Nelson would have more free exchange of ideas amongst distinguished branches of economics (not to mention the other social sciences) each of which would be determined by its own worldview and approach (Nelson, 1996, p. 150). In this way, Nelson accepts theory-ladenness and the general diversity of the world and turns them to scientific advantage. She is guided by the view that objectivity is enhanced when qualified practitioners share intellectual authority (Hands, 2001, p. 273) and the principle that the “object of thought becomes progressively clearer with the accumulation of different perspectives on it” (Berger & Luckmann, 1966, p. 11).

2.7 Becker, Nelson, and Lakatos

Nelson’s ideal of a pluralistic economics stands in stark dissimilarity to Becker’s ostensible explanatory unification and imperialism. Nevertheless, it is worth considering for a moment whether Becker is as imperialistic as he is alleged to be. There is evidence (Becker 1976, 1993; Swedberg, 1990) that Becker is cognizant of the limitations and fallibility of the economic approach and that he supports the independent functioning of the other social sciences. He writes,

I am arguing that the economic approach provides a useful framework for understanding all human behavior, I am not trying to downgrade the contributions of other social scientists, nor even to suggest that the economist's are more important...many of the important concepts and techniques are provided and will continue to be provided by other disciplines (Becker, 1976, p. 14).

In an interview with Swedberg, Becker does even more backpedaling. He argues that economists' obsession with individualism can sometimes be a limitation, and concedes that economists have something to learn from sociologists when it comes to organizing surveys (1990, p. 43). He recognizes "the problem of bringing social interactions into the analysis in a fruitful and useful way" and concedes that sociologists and 'economic sociologists' are more on track with that enterprise (p. 42). Becker even backs away from the claim that "rational choice" can handle "*all* human behavior" or "everything in social science," arguing that many of "the big questions," such as war and religion, are more adequately explained – for now – by sociologists (pp. 38, 40). In his 1993 Nobel Prize acceptance address, he says, "My work may have sometimes assumed too much rationality, but I believe it has been an antidote to the extensive research which does not credit people with enough rationality;" also, "I have intentionally chosen certain topics for my research...to probe the boundaries of rational choice theory" (p. 402). He likewise states that he is "open minded" to the possibility that "somebody will come along with a more general and powerful theory, which

includes rational choice as a special case, and that will have different behavioral implications” (Becker in Swedberg, 1990, p. 40).

Mäki (2008, pp. 22-23) identifies another passage from Becker as crucial:

So I start with the assumption that behavior is rational, and ask, “As I apply this to a particular problem, is there behavior that I *cannot* explain with the rationality model?” Since rationality can be pretty flexible and the data are often limited, I don’t frequently encounter decisive evidence against rationality. Anyway, this is my way of doing things. Others are more agnostic about the scope of rationality, so they will approach a problem by asking, “Does this look like rational behavior or is it better interpreted in a different way” Part of the difference, therefore, is the degree of *commitment* or *confidence* one has of finding rational behavior when investigating a particular set of phenomena (Becker in Swedberg, 1990, p. 41).

Mäki interprets this paragraph as Becker exhibiting “admirable awareness of...epistemic uncertainty” (Mäki, 2008a, p. 23). By this, he evokes the notion – found in Lakatos’ tenacity principle and in Kuhn – that scientists are invested in their theories, and adhere to them because of some degree of evidentially unsupported commitment (Lakatos, 1973, p. 4; Kuhn, 1970). Although Lakatos scornfully relegates such an explanation for the progress or degeneration of

theories to the 'external history,' Mäki takes such commitment as reasonable and inevitable (Lakatos, 1971, p. 102; Mäki, 2008a, p. 23). Nevertheless, Mäki writes of epistemic uncertainty as a constraint or limit on economics imperialism and explanatory unification.

Whatever the degree of subjective commitment and confidence on which Becker and his opponents rely, it remains radically fallible. The epistemological constraint I am proposing on economic imperialism advises against dogmatic commitment and recommends a strong sense of fallibility and openness to critical conversation across disciplinary boundaries. Personal and "strategic" commitment to a theory may do no harm, but only provided it is accompanied by tolerance and pluralism that derive from a deeper commitment to the uncompromised principle of fallibilism (Mäki, 2008a, p. 23).

It is self-evident that Becker is both strategically and personally invested in the economic approach, and that that investment is, to some degree, non-evidential. This has been explained by Lakatos' assertion that the theoretical hard core of any approach is, to some degree, taken on faith (Lakatos, 1974). However, faith is not enough. Lakatos argues that the research program in question must consistently produce theoretical and empirical progress by virtue of predicting novel facts. Mäki argues that for unsupported commitment to be scientifically healthy and

legitimate, there must be concomitant tolerance, pluralism, and openness to critical conversation. Although Mäki and the above discussion indicate that these attitudes are present in Becker, they are there in such miniscule amounts so as to be negligible.

For Becker, economics' sister social sciences play protective, auxiliary, or helper functions. Their contributions fit into the protective belt of the economics research program, but they themselves do not constitute rival research programs. He writes, for instance, that he is "skeptical whether major *theoretical* concepts from traditional sociology will have a major influence on the interaction between economists and sociologists" and even describes rational choice theory as an avenue through which sociologists could achieve "large influence" (Becker in Swedberg, 1990, p. 45, italics in original). Becker believes that economics students are, on average, "more talented" than sociology students, as measured by "analytical capacities and better training in formal skills"; that "the common denominator in much of the contact between economics and sociology will be rational choice theory as developed mainly, but not entirely by economists"; and that sociologists that employ rational choice theory may be called 'economic sociologists' but are nearly indistinguishable from economists, excepting their greater proclivity towards surveys, incorporating social constraints, and discussing war, religion, and other big and broad questions (Becker in Swedberg, 1990, pp. 42-43). In short, Becker believes that the more powerful and influential sociology of the future will employ rational choice theory, or the economics approach, and because it is approach which distinguishes economics from

sociology (Becker, 1976), will actually be economics²²; that the “analytical engine” of economics will continue to dominate the social sciences, will progress in terms of explanatory unification, and will periodically incorporate minor insights and methodological innovations from the other social sciences; and that non-economic sociologists, normative social scientists, and philosophers will continue to dedicate themselves to big, broad, and intractable questions, despite glacial progress.

Becker is also systematically unwilling to seriously discuss alternative approaches or criticisms of his own position (Blaug, 1992, p. 226). He is thereby resistant to critical conversation, or admitting the constraint of his own fallibility, even if he does – as Mäki points out – show openness to the use of varying degrees of rationality. During his 1993 Nobel address, he deflects criticism with the “simple” and unsubstantiated assertion that “no approach of comparable generality has yet been developed that offers serious competition to rational choice theory” (the economics approach) (Becker, 1993, p. 402). It is evident that even in 1990 and 1993, Becker’s faith in the economics approach’s potential for explanatory unification was unshaken.²³ Despite the minor concessions, Becker represents the type of “dogmatic and arrogant” economics imperialism that, Mäki argues, “should be resisted under all circumstances”:

²² Becker disagrees that sociologists “trained in rational choice theory” will be de facto economists; because they will still be trained in sociology departments, they will still be sociologists (in Swedberg, 1990, p. 43). This departmental-determinism is incommensurate with his approach-determinism, and the latter is taken to be the more explicit, relevant, and important.

²³ It is conceded that whether his attitude and approach have substantively changed since 1993 is not examined by this paper.

Economics imperialism is a form of economics expansionism where the new types of explanandum phenomena are located in territories that are occupied by disciplines other than economics, and where economics presents itself hegemonically as being in possession of superior theories and methods, thereby excluding rival theories and approaches from consideration (Mäki, 2008a, p. 24).

Characterization of Becker in line with this type of economic imperialism is consistent with Nelson and Hands' critique of Becker. Mäki's normative call for "a modest, tolerant, open-minded, pluralistic, and self-critical economics imperialism" is more consistent with Nelson's expansionism than with Becker's (Nelson, 1996). When examined through the lens of the Lakatosian framework, Nelson and Becker offer converse visuals.

Becker yearns for one huge economics research program, which would be orbited by the programs of the other social sciences. These moon programs would constitute the protective belt of the economics research program as well as its negative heuristic. They would likewise offer contributions to its positive heuristic problem-solving machinery, as well as "intelligent and thoughtful" hypotheses about certain big and broad problems (Becker, 1976, p. 38). These hypotheses would entertain and stimulate positive economists and, perhaps, thereby inspire a project in positive science.

Conversely, Nelson seeks a constellation of mutually enriching economics (and other social sciences) research programs. Each of these would be relatively comparable in size, replete with its own theoretical hard core, protective belt, and heuristics. Each would be determined, to some degree, by a particular worldview. For this to be achieved, the research program and resources of neo-classical economics would have to shrink while heterodox programs (e.g., feminist economics) would have to swell. Objectivity would be increased by the validation of alternative worldviews and the increased competition and flow of ideas.

It is clear that Becker and Nelson are incommensurate. Before offering a normative preference for one or the other, some comments are offered on two relevant topics, which help guide the discussion to its close – the actual diversity of economics and the social sciences and the distinction between prediction and explanation as objectives of science.

2.8 Complications and Conclusions

Despite their differences, both Becker and Nelson make the positive claim that economics is excessively limited and the normative claim that it should be expanded. Other scholars contest both claims, and assert that economics is already home to a great diversity of research programs.

In an interview with Swedberg, Oliver Williamson states, “Actually, one of the things that is probably frustrating to non-economists is that economics is so incredibly elastic. Once the economic content of a concept is understood,

economics finds a way to embrace it” (1990, p. 122). Swedberg points out that Sen had written one hundred and seventy articles by 1987, many of which are aimed at uniting economic and philosophical or economic and political questions (1990, p. 249). Asked by Swedberg about his prolific yield and wide ranging interests in economics (e.g., “technology, famines, gender, social choice, welfare economics, and so on”), Sen responds, “Well, first of all I am not certain that is so unusually wide because I think that a great many economists are extremely broad in their interests...the subject of economics also *lends itself* to having broad interests” (1990, p. 250). This is not a peculiar attribute of contemporary economics, associated with the advent of economics imperialism. Instead, it is consistent with the generations of economics defined by the work of Adam Smith, John Stuart Mill, and Karl Marx, whose economics was intermixed with, and interdependent on philosophy, politics, and law (Hands, 2006, pp. 1-2; Sen in Swedberg, pp. 250, 264-265).

All of these Swedberg interviews, furthermore, are found in a book dedicated solely to the documentation of the burgeoning relationship between economics and sociology (1990). Analogous cross-over projects and sub-disciplines – and the works which constitute or document them – exist in the intersection between economics and philosophy (Hausman & McPherson, 1996; Sen, 1987), economics and political science (Buchanan & Tullock, 1962; Downs, 1957), economics and psychology (Rubinstein, 2005), economics and cognitive science (Spencer, _;Rubinstein, _), economics and anthropology, and more. Economics imperialism has been important in the proliferation of similar

interdisciplinary projects, but even that phenomenon is a mere portion of regular economics expansionism, which also includes more even-handed and mutually enriching collaboration between economics and other disciplines (Mäki, 2008a). Even in the relatively limited sub-field of Lakatos applied to economics, Hands identifies over 20 surveys and over 50 case studies on “almost every conceivable area of economic thought” (Hands, 2001, pp. 287-288). Among the case-studied are numerous non-mainstream areas, including the psychological foundations of economics, experimental economics, radical economics, and Marxian economics (pp. 287-288). Hence, the claim that economics is narrow and desperately needs expansion or enrichment by increasing perspectives is dubious.

It follows that both Nelson and Becker exaggerate the narrowness and inflexibility of mainstream economics. In this way, through the lens of the Lakatosian framework, both scholars tend to describe the galaxy of economics as if there already is just one massive economics research program which is orbited by the research programs from the other social sciences (the schematic Becker wants to create and Nelson condemns). This is done, perhaps, for the sake of convenience, or because a more subtle or expansive discussion is deemed irrelevant. Indeed, they might be inclined to employ “isolation by idealization” – to “control for noise so as to isolate some important fact, dependency relation, causal factor, or mechanism” (Mäki, 2009, p. 30). Nelson is partially vindicated because of the power differential between the neo-classical mainstream approach and the heterodox approaches. Becker is partially vindicated because the majority of his work is seminal and instrumental in jumpstarting economics imperialism.

All the same, Nelson and Becker derive legitimization from their exaggerations. They add authority or persuasiveness to their arguments for expansionism and aggrandize the seeming originality and importance of their own work. Moderating or adulterating Becker and Nelson's extreme views of economics moderates or adulterates their arguments for expansionism. Those arguments are fundamental to the significance of their work.

One more point complicates the analysis of Becker and Nelson in this paper. Namely, that is the distinction between explanation and prediction as proper objectives of science. This is an object of interest in economics and philosophy of science, and can be seen in Nelson, Becker, and Friedman.

It has been argued that the primary methodological focus of Becker and Friedman is prediction. Hands argues that prediction is the exclusive objective of Friedman, who represents extreme methodological instrumentalism and falsificationism (Hands, 2001, pp. 55-57). Despite Friedman's influence, it has been argued that Becker is a half-hearted or failed falsificationist, and only a moderate instrumentalist (Section 2.5; Blaug, 1992, pp. 220-226). Becker (and Becker and Stigler) uses the word 'explanation' consistently throughout their work as well as words that invoke explanation. For example, Becker writes about the objective of "seeking to understand human behavior" (1976, p. 3), and how his doctoral thesis "used economic analysis to better understand racial discrimination" (p. 8). Understanding is analogous to explanation, not prediction.

Becker (like Friedman) puts "explain" in quotation marks, the first time he uses it, which just happens to be in the context of the sub-par economist

“postulating the required shift in preferences to ‘explain’ all apparent contradictions to his predictions” (Friedman, 1953, p. 188; Becker, 1976, p. 5). Later, however, he uses it twice (without quotation marks) in the same sentence to refer to his position (“...the economic approach to explain...”) as well as the position of the sub-par economist (p. 7). Becker’s last word is that “the economic approach provides a valuable unified framework for understanding *all* human behavior” (p. 14, italics in original). Similarly, Becker and Stigler use the term on nearly every page, in statements such as, “phenomena...usefully explained” (1977, p. 77) or “behavior [that] has been illuminated” (p. 89), or “alternative explanation[s]” (p. 89). Their conclusive statement is that “*all* changes in behavior are explained by changes in prices and incomes, precisely the variables that organize and give power to economic analysis” (1977, p. 89). Explanation features in every one of Becker’s essays, in the book “The Economic Approach to Human Behavior” (1976), in his Nobel Prize address (1993), and in his Swedberg interview (1990, p. 41).

Explanation also seems to be the presumed goal of Becker in Nelson and Hands, and it is on explanatory grounds that they critique his home economics. Becker’s approach, they argue, is deficient of “real understanding of the social forces (even economic forces) that undergird the institution of the family” (Hands, 2001, pp. 270-271; Nelson, 1996). Beyond Becker, who serves as a proxy, the main object of criticism for Hands and Nelson is neoclassical economics. They write of the preoccupation of “formal models” with “exhibiting the power of microeconomic theory...[not with] gaining knowledge” (Hands, 2001, p. 271).

Indeed, the association between Becker and explanation is difficult to avoid, considering Becker's claim that the economic approach can unify the "understanding of *all* human behavior" (1976, p. 14). This is routinely and correctly described as explanatory unification (Mäki, 2008a; Sen in Swedberg, 1990, p. 264) – nobody talks about predictive unification. If explanation is taken to be a serious objective, however, one must examine that explanation on theoretical grounds, using verisimilitude as the criterion of evaluation. Extreme falsificationism and instrumentalism cannot even approach this problem, hence it has been argued that Becker is better understood as a Lakatosian, and a user of at least a "whiff of inductivism," to establish the verisimilitude of theories (Section 2.5; Lakatos, 1978b, p. 159). When appraised on this verisimilitude criterion, both Nelson and Blaug are relentlessly pessimistic about Becker's explanatory success (Blaug, 1992; Nelson, 1996).

And yet it is recalled that there is no refutation without a superior solution. In that vein, Blaug was discussed because he provides illuminating criticisms, but dismissed because of his relentless adherence to a methodology – Karl Popper's falsificationism – which had (in this paper) already been written off (Sections 1.5, 2.5; Blaug, 1992). Blaug also helps reject the methodological adequacy of the methodology of scientific research programs (MSRP) and Lakatos' novel fact criterion as they are applied to economics. This was shown to pertain only when softened to include skillfully invented novel derivations of already known facts from a standard theory (Sections 2.5, 1.5; Mäki, 2008a, p. 8). Hands dismisses

the novel fact approach outright, arguing that it causes more trouble than it is worth (2001, p. 206).

The superior solution presented in this paper is offered by Mäki (2008). He conditions justifiable commitment to a certain theory or approach on the constraint of epistemic uncertainty and attendant tolerance, pluralism, and the principle of fallibilism (Mäki, 2008a, p. 23). Mäki overemphasizes Becker's rhetorical support for pluralism and fallibilism, and incorrectly identifies him with these traits (pp. 22-23). Instead, this paper makes the claim Becker is a "dogmatic and arrogant" economics imperialist who presents his economics approach "hegemonically as being in possession of superior theories and methods, thereby excluding rival theories and approaches from consideration" (Mäki, 2008a, p. 24). Becker would have the economic approach be *the* research program of the social sciences.

The philosophy of science work and the criticisms of Becker from Blaug, Nelson, and Hands show that there are many reasons for epistemic uncertainty, radical fallibility, and therefore methodological modesty in economics. Hence, Becker's level of commitment to the economic approach is not philosophically justifiable. Nelson, on the other hand, does satisfactorily represent pluralism and fallibilism, and she does so because of a cognizance of the relevant philosophy of science (Part 1).

Therefore, when it comes to normative-prescriptive arguments for the social sciences, depicted through the lens of Lakatos' MSRP, Nelson's view of a constellation of similarly sized, rival research programs is superior to Becker's

view of a dominant economic program, orbited and fed by programs from the other social sciences. That said, the current field of social science, if seen through the Lakatosian framework, probably would look like something between Becker and Nelson's ideals. It contains a remarkable diversity of economics research programs, including programs combined with and enriched by every other conceivable social science.

CONCLUSION

This paper has sought to give a historical and critical survey of some of the dominant philosophy of science of the first three quarters of the 20th century, and apply that philosophy to economics. In Part 1, the methodology of scientific research programs (MSRP) of Imre Lakatos emerged as the object of focus, and it

was shown contextualized and enriched by Lakatos' philosophy of mathematics as well as the philosophy and historiography of science of Karl Popper and Thomas Kuhn. A preference for Lakatos over Popper and Kuhn was expressed, and important problems and concepts of the philosophy of science – the problem of induction, theory ladenness, underdetermination, the problem of identification, and verisimilitude – were discussed in light of the theories and methodologies of these three philosophers.

These concepts from the philosophy of science and, especially, the MSRP were then applied to economics. Gary Becker's economic approach (1976) was presented as representative of neo-classical economics, and shown to be amenable to analysis by the MSRP – its hard core, protective belt, and heuristics were all identified. Critiques of Becker and neo-classical economics (Blaug, 1992; Hands, 2001; Nelson, 1996) were then presented along with alternative approaches. The claim was made that economics is an imperial science and that Becker is one of its imperialists, with an agenda of explanatory unification.

The objective of the paper has been to show some of the strengths and frailties of economics theory, in general, and Becker's approach, more specifically. Kuhn (1962/1970) was rejected early on because of unpardonable exaggeration, but his theory-ladenness principle was shown to drastically limit the objectivity of positive economists. Falsificationism was shown to be methodologically inadequate in economics, but persistently influential and important in its more moderate forms, such as the MSRP of Lakatos. The MSRP was also shown to be not wholly satisfactory when applied to economics, because

of the requisite adulteration of the novel fact criterion. Explanatory unification was shown to be virtually uncontroversial in theory, but severely limited in practice, because there are many reasons for epistemic uncertainty and radical fallibility in economics. These principles show Becker's level of commitment to the economic approach to be philosophically unjustifiable. Given the philosophical and practical constraints, methodological pluralism, as shown in Mäki (2008) and Nelson (1996), was argued to be a better bet for explanatory fecundity in economics and the social sciences.

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SUMMARY

This project was enacted by yours truly, Bradley Turner, over the course of the 2009-2010 academic year. Although the first person was eschewed in the paper, I will use it here. Indeed, I banned 'I' from the paper more out of

spontaneity than convention or deliberation. It often seemed unwise and unwieldy and I considered changing it numerous times. Regardless, it proved practicable and I persisted, but now I prefer a change of pace.

Although there are basic and straight-forward summaries in the Abstract, Introduction, and Conclusion of this paper, I do provide an informal summary here. However, I also muse on why I decided to do *this* project, some of the material, the process, and the triumph of completion.

This Capstone project is rooted in the philosophy of science and social science, epistemology, ontology, the methodology and philosophy of economics, and economics. I am graduating from Syracuse University with a B.S. in Economics and International Relations.

I was introduced to the material that eventually became this Capstone at the Pontificia Universidad Católica in Santiago, Chile, in Spring 2009, where I was studying abroad. I took a class there with Professor Felipe Eduardo Saffie Kattan, who was teaching a class called Economics and Philosophy. I was given assurance of accreditation and jubilantly signed up.

My experience was not all peaches and cream. Not only was the class my first in formal philosophy, but it was also taught by a relatively youthful and hip Chilean – in Spanish. The lectures consisted of enlightening gibberish. I found everything brilliantly interesting and stimulating, albeit challenging. Saffie Kattan surveyed everything from Aristotle versus Sophistry to Baconian empiricism to modern topics in positive (scientific) economics, such as models and model making, instrumentalism, behavioral economics, and the real business

cycle. There were brief forays into Foucault, Wittgenstein, the radical indeterminacy of language of Quine, and other tangentially relevant concepts and topics.

Part of the allure of the class was undoubtedly the talent and charisma of Saffie Kattan himself. There was also a sheen of novelty which attracted me – I have always been a curious magpie. Nonetheless, the material really did – and does – attract me profoundly. Although exclusively qualitative, it was also bound up with the empirical and scientific – the ideas and methods of economics that I had been taught at S.U. Economics through the lens of philosophy regained a lost luster.

I would be remiss if I failed to discuss the thinkers and topics which ultimately guided the exposition of my Capstone – Karl Popper, Thomas Kuhn, and Imre Lakatos – all of whom were covered during Saffie Kattan's course. These gentleman were active in the first three quarters of the 20th century formulating meta-methodologies for the understanding and appraisal of scientific thought. They dealt in the empirical, the methodological, the historical, and the normative-prescriptive. They are known and discussed as post-positivism philosophers because they were pioneers and vehicles for the mass dissemination of ideas (the theory-ladenness principle, the problem of induction, the tenacity principle, and more) which posed major problems for the arch synthetic-empirical and formalist (logic and mathematics) approach to the evaluation of science. Popper and Kuhn came first and stirred the pot. Lakatos then postulated a methodology and flexible structure (the Methodology of Scientific Research

Programs (MSRP)) which appropriated the best of his predecessors and discarded the worst. They did not reject empiricism and formalism, but rather argued for alternative ways of analyzing and using those methods. Most notably, Popper and Lakatos are known as falsificationists, whereas most of the scientist and philosophers who preceded them are known as verificationists. Some of their ideas persist, others have lost favor, but nobody can deny their epic influence on 20th century philosophy, social science, and even popular culture (esp. Kuhn).

Indeed, the second and more lengthy part (Part 2) of the paper (which has 2 Parts, each with numerous Sections) consists of identifying the influence of Popper, Kuhn, and Lakatos in contemporary economics theory. I used the MSRP to frame the discussion and incorporated concepts from the discussion of philosophy so as to enrich the discourse. More specifically, I focused on the theoretical apparatus and economic approach of Gary Becker, the University of Chicago economist and Nobel prize winner.

I discuss and defend Becker's economics approach and then establish its relation and contribution to what is known as economics imperialism. Economics imperialism is almost always, accurately, associated with a tendency towards explanatory unification, or the explanation of a huge (or unlimited) range of human behavior under the auspices of one holistic theory. This is an almost universally admired tendency in theory, but extremely controversial in practice – such as in the case of economics imperialism.

I argue that Becker represents a headstrong economics imperialism and academic and intellectual hubris. I explore critiques of Becker in light of the

tenets of falsificationism, the MSRP, and the concepts of philosophy discussed in Part 1. In the end, I conclude that Becker's confidence in and commitment to his particular economics approach is excessive and philosophically unjustifiable because it is superior, exclusionary, and overly committed to methods which are shown to have inherent epistemic uncertainty.

Becker is juxtaposed with a view from feminist economics and a methodology which both support a sense of fallibilism, situated objectivity (or subjectivity based on social circumstance), and a kind of academic, scientific pluralism. I express normative preference for the self-conscious pluralism over the imperialistic explanatory unification. Both Becker's imperialism and the pluralist outlook are depicted as displays in the framework of Lakatos' MSRP. I ended up arguing that both the Becker and the feminist economist viewpoint depend on an exaggerated perspective of the field of modern economics.

This conclusion was not exactly expected at the time that I began the project. In fact, a my set of ideas and expectations was constantly changing. It often happened that what I thought I knew was actually balderdash, what I thought I was arguing was actually tomfoolery, and that many of my grandiose statements were actually sophomoric over-simplifications and exaggerations. Not only that, but I found, to my chagrin, that many other scholars were as inclined as I was to over-simplification and exaggeration. I ended up doing little in the way of statement-making, but rather strived to make purposeful and elucidatory observations and explore relevant connections between philosophical positions and scientific methodologies.

Work from here will probably not continue in the economics philosophy and methodology vein. There simply does not seem to be much of a market for that type of philosophizing, especially in the United States – Europe is more amenable to that type of scholarship. Indeed, my opinion is that one should be wary about making statements, appraisals, or (even) critical observations of a scientific or empirically oriented social science unless one has been trained in that practice oneself. Which is to say, I do not much want to specialize in philosophy and methodology of economics unless I become trained concomitantly in economics. Anything can happen, of course.

Indeed, where I go from here is undetermined. Like all economic actors, I am constrained by limited information – about myself, about potential academic tracks of study, professional opportunities, and innumerable other things. One thing that can and must be said is that the completion of this Capstone is, for me, a triumph. I am confident that it has enriched me, and will be a major feather in my cap in times to come.

I step forward with head held high, crowned with honors, to face the vicissitudes of the unknown.