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The Environmental Kuznets Curve Hypothesis as a Problematic: Beyond "Falsificationism"

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The Environmental Kuznets Curve Hypothesis as a Problematic: Beyond “Falsificationism”

A Thesis Presented

by

PAUL DAVID ERB

Submitted to the Graduate School of the
University of Massachusetts Amherst in partial
fulfillment of the requirements for the degree of

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Sociology

The Environmental Kuznets Curve Hypothesis as a Problematic: Beyond "Falsificationism"

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PAUL DAVID ERB

Approved as to style and content by:

Dan Clawson, Chair

Anthony Paik, Member

Z. Fareen Parvez, Member

Michelle Budig, Department Head
Sociology

ABSTRACT

THE ENVIRONMENTAL KUZNETS CURVE HYPOTHESIS AS A PROBLEMATIC: BEYOND "FALSIFICATIONISM"

SEPTEMBER 2018

PAUL DAVID ERB, B.A., UNIVERSITY OF CINCINNATI

M.A., UNIVERSITY OF MASSACHUSETTS (AMHERST)

Directed by: Professor Dan Clawson

Halfway into its third decade, the debate surrounding the environmental Kuznets curve (EKC) hypothesis has stalled with political economists and socio-ecologists ascendant and modernization theorists scrambling to give their apparently moribund perspective new life. But beyond the rise and fall of the EKC, there remains a second-order question and decades of data: how do the theoretical perspectives of these contenders shape what their protagonists do and don't see? How have they mistaken episodes of "talking past each other" for genuine dialogue? Which perspective has had the biggest impact on the other's way of thinking? A qualitative and quantitative analysis compares the top-ranking journals in economics with interdisciplinary journals of environmental economics revealing a categorical divergence in the types of critical thought deployed in the EKC debate over an almost 15 year period. The few articles appearing in the top ranking economic journals systematically fail to grasp the fundamentals of ecology which is evident in both their measurements and conclusions. I offer an abridged discussion of the critiques socio-ecology presents contemporary economics as what, in Kuhnian terminology, may well be described as a discipline in the crisis moment of a paradigm shift in no particular direction. I then conclude by siding with Habermas and Adorno against Popper's ideologically impoverished "falsificationism": progress in science depends as much on a theory of ideological critique as it does on the acquisition of technical knowledge. My intent has been to argue that ideological critique is empirically possible as the history of thought.

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CHAPTER I

IDEOLOGICAL CRITIQUE OR THE HISTORICAL EVALUATION OF INCOMMENSURABLE THEORIES: THE EKC AS A PROBLEMATIC

1.1 Introduction

In this paper I consider the environmental Kuznets curve (EKC) hypothesis as a case study to evaluate its problem (i.e., socio-ecology) instead as a *problematic*. The problem of the EKC is to empirically demonstrate whether or not the systemic logic of the commodity form (i.e., exponential growth) is compatible with a sustainable environment. In the old-fashioned Marxian language, the question is whether or not this systemic logic will ultimately become its own barrier as the ever expanding circuit of capital erodes its reproductive base. This debate has seen political economists and socio-ecologists face off against modernization theorists for two and a half decades in the case of the EKC hypothesis in particular, and decades longer more generally. For reasons concerning the nature of social science, the possibility of predicting the actual course of events for such a question seems unlikely. Although, this problem still has a crucial role to play as the stakes of environmental crisis continue to become more, rather than less, evident and social forecasting maintains the possibility of understanding an historical trajectory without the philosophical burden of a telos.

By focusing instead on the history of the EKC debate over the last 15 years, we can remain empirically grounded while asking a second order question investigating if and how alternative theoretical perspectives operate in incompatible ways. This question moves us beyond trying to assess which theoretical perspective best explains the evidence (the problem) to that of evaluating how these alternative theoretical perspectives measure and interpret the same phenomena incommensurately, and how such a divergence in thought and practice informs the different conclusions they often reach (the problematic). While I consider a review of the

development of problematics or ideological critique to be an important exercise, this paper focuses more narrowly on empirically demonstrating that alternative theoretical perspectives do in fact structure our critical thought.

CHAPTER II

METHODS AND DATA: DECADES OF CRITIQUING THE EKC HYPOTHESIS

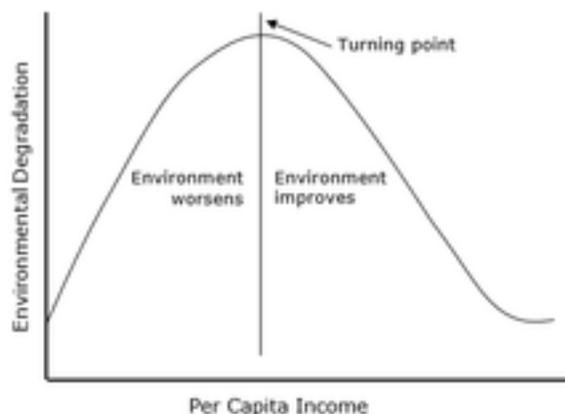
2.1 Identifying Alternative Theoretical Perspectives

In order to proceed with such a comparison, we must first identify alternative theoretical perspectives that might be compared. As I am considering the EKC hypothesis as a case study of the more general problem of socio-ecology, a viable way forward, it seems to me, is to form a comparison between those articles published in the most important journals in economics with those articles published in the interdisciplinary journals of environmental economics. Establishing a comparison this way does have limitations. For example, where articles are ultimately published is not a neat result of matching theoretical perspectives with particular journals. In reality, there is likely a great deal of variation. Furthermore, it is not as if theoretical perspectives are always explicit or well developed discussions in published articles. Much of this remains implicit. In order to overcome these concerns, my strategy here is to develop a comparison that is observable *across* disciplines. At the level of an interdisciplinary comparison, we should expect the articles published in journals to conform to those theoretical assumptions which demarcate their disciplines as such. Practically speaking, we should expect that interdisciplinary journals that explicitly express interest in both economic and environmental issues should attract a more developed or intricate understanding of ecology than mainstream economic journals. This is, at any rate, implied in the very notion of interdisciplinary studies. So while we might expect some of the random chaos of actuality that is present in any sort of historical comparison, comparing between disciplines should considerably mitigate the seriousness of this concern and give credence to my claim that I have a comparison between distinct types of thought with easily grasped institutionalized differences.

2.2 The EKC Hypothesis as a Comparison Topic

The EKC hypothesis was proposed by Grossman and Kruger (1995) at the beginning of the 1990s in the context of the debate surrounding NAFTA. After an environmental movement and a wave of influential socio-ecological literature (e.g., Bookchin, Catton Jr., Schnaiberg) in the decades immediately preceding Grossman and Kruger's topical thesis, many expressed concern that liberalizing trade would have an adverse effect on the environment. This was not only because environmental problems seemed to be the commonsensical result of the growing level of economic activity, but also because the Trade Agreement lacked an overarching regulatory program that would prevent the effective export of polluting activities or a potential 'race to the bottom' in which market pressures allow production to pit environmental regimes against one another. However, the initial success of the EKC hypothesis was to provide empirical evidence that as per capita income increased past a certain level, the increase of environmental pollutants would reach a point of inflection and fall thereafter (i.e., the function of income and environmental indicators is parabolic). In this way, it could be argued that economic growth was not actually anathema to a safe environment but a sound strategy for achieving it.

Figure 1: A Graphic Representation of the EKC Hypothesis



I consider the EKC hypothesis to be the best candidate for a topic of comparison for two reasons. The first reason is that the EKC hypothesis has considerable comparative strengths. Dating back to the early 1990s, it is perhaps the earliest example of economists empirically

confronting the environment systematically. It has also enjoyed significant popularity across disciplinary boundaries and received a large amount of attention. Though it is mostly addressed in the interdisciplinary journals of environmental economics its notoriety has ensured that it at least registers in mainstream economic journals. In addition to its popularity, it is significant because it faces squarely the question of whether or not capitalism's imperative for growth/accumulation is irreducibly opposed to the notion of a safe environment and hypothesizes a comprehensive solution native to existing economic theory. Both the processes of ecology and economic activity are incredibly abstract processes. The scope of the EKC hypothesis has the ability to consider both as total processes and not simply as isolated or discrete instances. With "everything on the table", this space resists narrowing the common ground between the disciplines from which the debate over the problematic might be jointly organized.

The second reason for favoring the EKC hypothesis as a topic of comparison is that the comparison between the interdisciplinary journals of environmental economics and the mainstream economic journals allows us to recognize that the interdisciplinary journals includes a wider background of thought and perhaps even formal education outside of economics, including the general influence of important ideas within socio-ecology. Hence, we can reasonably claim to have a clear comparison between types of thought, a contrast of a body of predominately economic thought against a body of more ecologically informed thought.

2.3 Data Collection

I collected my data in September of 2014 by querying the search term "environmental Kuznets curve" in ProQuest Social Science Premium Collection¹ with limitations set to form a

¹ I ended up using ProQuest Social Science Premium Collection on the advice of the social science research librarian at UMass (Amherst) on account of inconsistent query results from JSTOR, EBSCO, Web of Science, and what could

comparison between the most influential journals in economics with those of the interdisciplinary journals of environmental economics. The most influential journals in economics are operationalized as the ten journals with the highest impact factors as determined by *Journal Citations Reports*, a publication with citation counts for the Web of Science. Since 2000, only eight articles in these journals have used the EKC search term (see table 1). Of these eight, I excluded three because they do not substantively address the EKC hypothesis. Kitzmüller et al. (2012) only mention the EKC once in order to posit that it shares a similar logic with their actual topic, corporate social responsibility. Toll (2009) is excluded because the EKC search term only appears in the title of a referenced source and is not actually discussed in his review on the estimated costs of climate change. Finally, Portney's (2000) contribution is also excluded because the EKC hypothesis receives a single non-substantive mention throughout the course of his argument that incentive-based environmental regulations will become the norm by 2050. Because none of these critically engage with the EKC hypothesis, they cannot be compared across groups with other articles which do critically engage the comparison topic.

Table 1: Articles from Top 10 Journals

Articles from Top 10 Journals				
Journal:	Rank:	Impact Factor:	Articles:	Citations:
Journal of Economic Literature	1st	6.341	Zheng and Kahn (2013)	2
			Graff-Zivin and Neidell (2013)	2
			Kitzmüller et al. (2012)	10
			Copeland and Taylor (2004)	81
Journal of Economic Perspectives	4th	4.23	Toll (2009)	68
			Dasgupta et al. (2002)	90
			Esty (2001)	16
			Portney (2000)	4

be found in the archives of the journal's websites. I'm not sure if I will ever again overestimate the capabilities of what I presumed were standardized search engines.

In order to form the comparison group of articles in the interdisciplinary journals of environmental economics, I considered all articles appearing since January 2000 that used the EKC search term and have been cited more than 100 times in those economic journals which explicitly express concern for the environment or ecology either in their titles or journal description. I set the limitation of 100 citations and publications occurring since 2000 in order to form a manageable and relevant comparison with the articles in mainstream journals. As there were hundreds of articles published on the topic of the EKC in the interdisciplinary journals, limiting the selection to those with over 100 citations substantially reduced the number while maintaining those articles that arguably have had the most influence in shaping their readership's thought. Because the sample of articles from the top ten journals begins in 2000, I limited the selection of articles in the comparison group of interdisciplinary journals to this same year. Following these criteria, the comparison group of articles from interdisciplinary journals contains eight articles (see table 2).

Table 2: Articles from Interdisciplinary Sample

Articles from Interdisciplinary Sample				
Journal:	Rank:	Impact Factor:	Articles:	Citations:
Journal of Env Econ and Management	32nd	2.522	Stern and Common (2001)	133
Ecological Economics	33rd	2.517	Soytas and Sari (2009)	105
			Zhang and Cheng (2009)	117
			Soytes et al. (2007)	128
			Cole (2004)	104
			Dinda (2004)	279
			York et al. (2003)	162
World Development	62nd	1.733	Stern (2004)	401

I anticipate that some readers may object that a meaningful comparison is not possible, that the results of such a comparison will lack significance on account of the limited number of articles in the mainstream sample which only appear in generalist journals. Similarly, the fact that

many of these articles are review articles instead of empirical interventions might raise more questions about the validity of this comparison. However, these are not problems for the type of comparison I am making. Recall that my goal is to identify those articles arguably having the most influence on the way the comparison topic is conceived. From this perspective generalist journals and review articles are no less important (and arguably more important) than novel empirical studies in specialty journals.

But there is a more fundamental point: it actually happens to be the case that the mainstream coverage is limited in this way. There is nothing that prevented authors from making novel empirical contributions or initiating a lively debate in any of the non-generalist mainstream journals which are included in this sample. But this did not happen. For an historical method, this measurement of an “absence” provides us with data that is as empirically relevant as the more easily available data that conform to the “large-n” criteria favored by positivist methodologies. It is of course possible to “go where the data is” and analyze only the heterogeneity of the many available EKC articles in the interdisciplinary journals. But to do so sacrifices the question with which I began merely to satisfy contemporary sociology’s methodological bias towards positivism². Accepting that these mainstream journals were also part of the last fifteen years of the historical actuality which shaped our understanding of the EKC hypothesis, my task is to develop impartial methods that accurately report mainstream participation. Mainstream journals are no-less deserving of our critical attention than the interdisciplinary journals. While the coverage from the mainstream sample is limited, it is nonetheless present, and this serves as important evidence of a relative absence while still allowing us to evaluate the type of critical thought accessible in mainstream journals.

² For a particularly insightful contribution on this score see George Steinmetz’s “Odious Comparisons: Incommensurability, the Case Study, and “Small N’s” in Sociology” *Sociological Theory* 22:3, September 2004.

2.4 Categories of Critique: Types I and II

An initial reading of the data revealed a predictable divergence in the way in which the EKC hypothesis was critically approached. In order to test the possibility or degree of a critical divergence, I centered my evaluation on the critical focus given to the dependent and independent variables of the EKC function. Recall that the independent variable measures the economic abstraction 'growth' while the dependent variable measures the ecological abstraction 'environmental quality'.

I decided to classify as a type I critique, all critical efforts focused on the independent variables of the EKC hypothesis. The basic insight of the type I critique begins with the recognition that growth is neither static nor homogenous. Instead, this type I critique identifies both proximate causes (e.g., effects of technique, composition, and scale) as well as underlying causes (e.g., information transparency, education, regulatory capacity and enforcement) that dynamically effect how economic activity changes over time. Accordingly, type I critiques identify those control or independent variables other than income which are often responsible for improvements in environmental indicators such that environmentally conscious growth may be possible. *The underlying logic of an isolated type I critique is that any and all empirical evidence of an instance of the EKC is ipso facto evidence of the hypothesized EKC relation.*

A type II critique, on the other hand, focuses on the operationalization of the dependent variable of the EKC hypothesis. The basic insight of the type II critique begins with the recognition that specific environmental indicators are a single facet of a complex ecological process. Beyond the task of developing reliable measurements of a given indicator, type II critiques stress that there remains a second-order question of establishing said measure's relative ecological importance in relation to a myriad of possible indicators. Accordingly, the task of a type II critique is to establish that evidence of an improvement in a specific indicator is at the same time evidence

of ecological resilience or sustainability. *The underlying logic of a type II critique is that any and all empirical evidence of an instance of the EKC may or may not be evidence of the hypothesized EKC relation because this depends entirely on how one decides to measure "the environment".*

In many ways this distinction between types of critical thought boils down to the analytical distinction between environment and ecology. Schnaiberg (1980:9-10) began his influential book by observing that the adjective "environmental" has become so popular among scientists that its substantive meaning has been eclipsed by its formal meaning. "Whatever is outside of the immediate focus of study (the individual, the group, the society) represents its 'environment'. Should we follow up on this definition, virtually every problem is an 'environmental problem'." But rather than abandoning the term altogether, Schnaiberg recommends that we refine our understanding such that we read 'environment' as the structure or process of "ecology". Ecology is the dynamic and generative integration of living and nonliving systems that yield environments; environments are the product of ecology. "In other words, ecology examines the whole cloth; the other fields of biology, the threads of nature" that can be understood in terms of *populations, communities, ecosystems, and the biosphere*. We might stress that this includes the behavior--economic or otherwise--of said populations.

As I hope to make clear in this paper, both of these types of critical thought mirror each other in that they search for the dynamic process beyond the superficial abstractions of 'economic growth' and 'environmental quality'. My data are analyzed by the type of critique present in the articles in order to determine if the distribution of these critiques reflects the organization of my comparison groups (i.e., mainstream economic journals vs. interdisciplinary journals of environmental economics). This allows me to test whether or not there is evidence of a divergence in critical thought during the ideological process that organizes the contest of what "the problem of the environment" should be understood to mean.

CHAPTER III

THE POLITICAL-ECONOMIC NATURE OF THE EKC HYPOTHESIS AND THE ELISION OF ECOLOGY IN MAINSTREAM ECONOMICS

3.1 Introduction

My analysis of the comparison topic of the EKC hypothesis between articles in the top-ten economic journals and the interdisciplinary journals of environmental economics reveals three facts. Firstly, the coverage of the EKC hypothesis in the top-ten journals is incredibly limited. Secondly, there is categorical evidence of a divergence in critique type by sample in which all type II critiques are developed exclusively within the interdisciplinary journals of environmental economics. Together, these first two facts comprise two absences in the mainstream journals that are both present in the interdisciplinary journals. Thirdly, all articles agree that the plausibility of a successful EKC relationship depends on political regulations. Table 3 reports these distributions by sample.

3.2 Limited Mainstream Coverage

The first point to be made about the articles appearing in the sample of top-ten journals is the relative paucity of the topic's coverage. Between January 2000 and September 2014 only five articles were published that substantially address the EKC hypothesis. None empirically apply the EKC in a novel study. Only three articles (Zheng and Kahn 2013, Copeland and Taylor 2004, Dasgupta et al. 2002), all of which are review articles, directly address the EKC while the remaining two (Graff-Zivin and Neidell 2013, Esty 2001) find brief recourse to the EKC in their respective discussions of the net benefits of environmental regulations and the need for transparency in trade negotiations. Also of interest, these articles appear exclusively in generalist journals. Eight of the top ten journals did not publish a single study on the EKC during my almost fifteen year

period of analysis. These eight journals not addressing the EKC are the journals seen as publishing the main empirical, mathematical, and theoretical articles in the discipline. The two journals covering the EKC hypothesis are both seen by economists as generalist journals, intended to be accessible to economists regardless of specialty, and possibly even to non-economists. The appropriate sociological comparison would be the appearance of articles in *Contexts* but not the *American Sociological Review* or the *American Journal of Sociology*.

Not surprisingly, in contrast to the notably limited coverage of the EKC in the first sample, the coverage in interdisciplinary journals is much higher with hundreds of articles. Using my sample selection criteria in order to allow for a manageable comparison of the most influential articles results in a sample of eight. While two of these eight articles are reviews, the remaining six are empirical contributions. Three articles (Soytas et al. 2007, Soytas and Sari 2009, and Zhang and Chen 2009) address the so-called emissions-energy-income nexus. Cole (2004) considers the role of trade in the EKC hypothesis. York et al. (2003) as well as Stern and Common's (2001) contributions include notable methodological innovations.

Table 3: Distribution of Critiques by Sample

Distribution of Critiques by Sample				
Top-ten Journals:		Regulations	Type I	Type II
	Zheng and Kahn (2013)	x	x	
	Graff and Neidell (2013)	x		
	Copeland and Taylor (2004)	x	x	
	Dasgupta et al. (2002)	x	x	
	Esty (2001)	x	x	
Interdisciplinary Journals:				
	Soytas and Sari (2009)	x	x	
	Zhang and Cheng (2009)	x	x	
	Soytas et al. (2007)	x	x	
	Stern (2004)	x	x	x
	Cole (2004)	x	x	
	Dinda (2004)	x	x	x
	York et al. (2003)	x	x	x
	Stern and Common (2001)	x	x	

3.3 Divergence in Critical Thought: Qualitative Analysis

In this section my aim is to document that there are two distinct ways in which participants of the EKC debate critique its hypothesis. It is my view that these types of critique are not in existential competition but are both necessary if the relation between economic growth and ecological change is to be better understood. This is the putative goal of EKC studies. All of these articles have scientific merit and have made varying degrees of significant and important contributions to this literature. Nonetheless, I am arguing that a set of authors thinks entirely in terms of economics when their task is to consider a hypothesis that presents a relation that is in equal measure about ecology. This is a problem. My best guess is that those authors whose work I review would not disagree with my analysis which I don't believe is controversial. Why? Because their collective works accept that the abstraction of "per capita income" is inappropriate for considering the dynamics that underlie an incredibly heterogeneous process. There is no single type of economic growth. But still, when they turn towards "environmental quality" they fail to criticize it in the same way, almost until the point where one wonders if they appreciate that it too represents a dynamic process, that there is no single measurement that captures its complexity, that "pollutants" most certainly aren't the best indicator of such a measurement, and that the "essence" of ecology is complete endogeneity, to suggest a term familiar to econometricians.

In this section I will qualitatively consider three of the most important articles from each sample at the level of their structure, overall focus, and conclusions. I have selected these six articles in consideration of space and because they are the most representative. The articles excluded from the interdisciplinary sample do not include type II critiques and, having noted the heterogeneity in this sample, are not necessary to review at length. The articles excluded from the mainstream sample also conform to the general trend of exclusively thinking in terms of type

I critiques, but they all offer a less direct focus on the EKC hypothesis and make weaker comparisons for this reason. All six articles considered in the section are the highest cited from their samples, with the exception of Zheng and Kahn (2013) which is also notable for its much later date of publication. For reasons of clarity I have organized their presentation by sample and begin with the mainstream sample which develops exclusively type I critiques. The presentation of the interdisciplinary sample follows where type II critiques are highlighted.

3.3.1 Broaching Type II Critiques

Dasgupta et al. (2002:147-8) represent the earliest article from my sample of mainstream journals. Their contribution is structured around consideration of the EKC's critics: "[s]ome pessimistic critics argue that cross-sectional evidence for the environmental Kuznets curve is nothing more than a snapshot of a dynamic process." This clearly broaches type II critiques. The authors recognize that the problem with the EKC hypothesis is that it lacks dynamism, that it assumes a narrow focus. In good scientific fashion, they set out to evaluate the pessimist's claims which they divide into two groups: the "new toxins" and the "race to the bottom" group. According to Dasgupta et al. (2002) the new toxins group cautions that "even if certain pollutants are reduced as income increases, industrial society continuously creates new, unregulated and potentially toxic pollutants" (Dasgupta et al. 148). Our authors do not identify any references exemplifying the "new toxins" group, so it is hard to say if their scope is as ecologically impoverished as the one Dasgupta et al. (2002) attribute to them or if this is a significant misreading of the pessimists on their part. But it is clear from their own work that without exception Dasgupta et al. (2002:150) conflate or reduce "pollutants" and "toxins" as the only possible way of measuring the EKC's dependent variable of "environmental quality". Even when they recognize that the data they use are poor (e.g., when they claim that "for many pollutants

data is scarce everywhere"), they never escape the strictures of this narrow measurement and consider only pollutants, ranging from the well know air and water contaminants to the more diverse and potentially "new toxins" including: carcinogenic organic chlorine compounds, persistent organic pollutants like PCBs, dioxins, DDT and other pesticides, and toxic pollutants. As we will soon see, such conflation don't survive type II critiques.

Returning to the other pessimists under their consideration, the "race to the bottom" group, we see that Dasgupta et al. (2002) clearly understand the formal *economic* dynamics. As is well known in the literature, proponents of the "race to the bottom" analysis caution that the appearance of EKC in actuality might only be structural change driven by globalization and comparative advantage. With comparatively lower environmental standards developing economies can produce dirty goods cheaper which incentivizes the displacement of this production in developed countries where environmental standards make it more costly and less competitive internationally. These pessimists caution that, absent an overarching environmental regulatory regime, environmentally dangerous production can pit politically fractured states against each other in search of the most competitive (least regulated) possible conditions of production: a "race to the bottom".

While the validity of this argument certainly requires empirical verification, the problem with Dasgupta et al.'s (2002) analysis is simply that it lacks an understanding of the formal *ecological* dynamics. The "race to the bottom" is evaluated exclusively in terms of data that reduces the entire dynamic process of ecology to the aggregation of well-known air and water pollutants in urban areas. Perhaps it is the case that these are the only existing studies that conform to "large-n" positivist analysis and that this is therefore the most readily available data. But even then, Dasgupta et al. (2002) fail even to mention this convolution and its likely significance for EKC studies in general and their own analysis in particular.

Absent even a basic ecological understanding, Dasgupta et al. (2002) conclude that the empirical evidence supports the more optimistic claim (*contra* the pessimists) that the EKC is peaking sooner than was conventionally believed practicable, and that environmental quality is improving with lower income levels. This more optimistic scenario has been possible, they argue, because of the primary role played by environmental regulation, the benefits of economic liberalization, the pervasiveness of informal regulations between communities and producers, as well as access to better information. This may well be true. But there remains the important type II question: how would their analysis change if instead of considering only well-known air and water pollutants concentrated in urban areas, the studies they considered included indicators more representative of ecological change? Here, in the earliest EKC study from the mainstream sample, we find both evidence of the absence of type II critiques as well as a conclusion validating the existence of the generalized EKC hypothesis.

In *Copeland and Taylor (2004)* we find a truly insightful mainstream article the chief defect of which is that their critical efforts are directed entirely towards investigating the dynamics of “economic growth” while totally ignoring the problem of measuring the dynamics of the hypothesis's dependent variable, “environmental quality”. Their understanding of the general problem is commendable. They begin by noting that “the debate has often been unproductive because the parties differ greatly in their trust of market forces and typically value the environment differently” even to the point of lacking “a common language” (Copeland and Taylor 2004:7). They assert skepticism about a simple or predictable relationship between income and environmental quality and aim to move the conversation beyond the results of the existing models of per capita income and environmental quality. As they see it, the problem is that EKC models are often uncontroversial as far as “stylized facts” may go, but are open to various

theoretical interpretations because they say so little (or so many conflicting things) when their findings are analyzed.

For this reason Copeland and Taylor (2004:8) develop their own model which features "a simple pollution demand-and-supply system linking pollution levels to national characteristics (income, factor endowments, and technologies) and trading opportunities (comparative advantage and current trade restrictions)". It also includes measures of the three important effects in the EKC literature. Effects of scale consider how scaling the economy increases every aspect of it, while effects of composition and technique consider the structural changes that follow major transitions (typically categorized as primary agricultural, secondary industrial, tertiary information and service economies) as well as technical innovations. Because scaling up the economy only increases the environmental impact of economic activity while the structural changes often significantly reduce this impact, the important question has been whether or not their dynamic interaction is best characterized by structural changes overtaking scalar change. As we have just seen in Dasgupta et al (2002), another central topic in the EKC literature concerns the global composition of economic activities, not isolated and individual states. In other words, the explanatory power of an effect of composition for an EKC could appear locally but simply be the result of transitioning from an industrial to a service economy if consumption patterns remain by replacing domestically produced goods with imported goods. Hence, Copeland and Taylor's (2004) model allows for a more nuanced look at the structure of economic activity.

However, in Copeland and Taylor's (2004) model we quickly find that "environmental quality" is reduced entirely to two measurements of emissions: "Z=pollution emissions" and "e=emission intensity of production". Now to be fair, Copeland and Taylor (2004:10) are at least dimly aware of this limitation as they concede in foot-note five that: "One large omission from our review is any explicit discussion of renewable or nonrenewable resource use and

sustainability. For many in the developing world, the status of fish stocks, aquifers, and forests are key environmental indicators, but an analysis of trade's impact on resource use would take us too far afield." This statement doesn't admit the full complexity and dynamics of ecology (any more than their improved model does for economic growth), but it is clear that their critical energies have been entirely one-sided as they have taken us a considerable distance away from the static measurement "per capita income" but still employ a model which expects that "environmental quality" should be captured in the static, homogenous and easily quantifiable, "unit of pollution" available for "the private sector's marginal valuation" (Copeland and Taylor 2004:14). It is also worth emphasizing that although they suggest that ecological concerns "take us too far afield", a type II critique would recognize that they are equally important in evaluating the legitimacy of the EKC's hypothesized relation. However, save their fifth footnote and a couple of scattered comments observing the often severe scarcity of data and the fact that "current work has looked at only a very few pollutants, and there is very little empirical work assessing the effects of trade on renewable resources", anything broaching, not to mention developing, an ecological understanding of the problem is categorically absent (Copeland and Taylor 2004:66).

In addition to the admission that type II critiques are not considered and probably should be, we also find Copeland and Taylor (2004) advancing a more complex conclusion about the validity of the generalized EKC hypothesis. While Dasgupta et al. (2002) felt confident concluding that the hypothesized relation both existed and was following a more optimistic scenario, Copeland and Taylor (2004) appear much more conflicted in their statements. While they claim that the legacy of the EKC includes having "debunked the commonly held view that environmental quality must necessarily decrease with economic growth", that there is a "great deal of evidence supporting the view that rising incomes affect environmental quality in a positive way", they also conclude that "[s]ince the studies define environmental outcomes quite narrowly, we should be

wary of claiming too much", that they are "skeptical about the existence of a simple and predictable relation between pollution and per-capita income" Copeland and Taylor (2004:23,66,54,8).

These statements are contradictory if one wishes to hold on to the original thesis that economic growth is compatible with a safe environment because their relation generally follows the environmental Kuznets curve. In the absence of the generalized hypothesis it is problematic to argue that evidence of only some of the very few indicators we measure following the EKC hypothesis has "debunked" anything. It remains an open question whether or not all the unmeasured indicators of environmental quality validate or invalidate the general hypothesis. If the weak claim is all we have (i.e., that at least the concentration of well-known pollutants in urban water supplies and the air can get better as growth proceeds), the hypothesis is limited as a policy prescription. At any rate, where Dasgupta et al. (2002) spoke with confident optimism that the EKC existed and was improving faster than anticipated, Copeland and Taylor (2004) spoke with cautious optimism. And here again we find more evidence of a decidedly type I critical engagement in the mainstream sample which offers ambiguous support for an increasingly complex EKC relation.

Finally, *Zheng and Kahn (2013)* consider the environmental consequences of the last couple decades of spectacular growth in China. Their contribution also appears in the mainstream sample. They carefully note that they are considering only localized air and water pollutants and not the more general problems of climate change. While they acknowledge that various authors can point to examples of these air and water pollutants which support the EKC hypothesis, they unambiguously state that they do not consider it to be a unique or predictive law of motion (Zheng and Khan 2013:733). As they understand it, their main contribution is to look beyond the abstraction of "per capita income" and to focus on underlying causes of successful instances of

the EKC hypothesis. These include: the rising middle class demand for a better quality of life by way of greener cities, increased information transparency that encourages the accountability of governments and firms, endogenous technological change, and the inclusion of sustainability as a performance criterion for assessing local politicians. Looking at the model they present as characterizing EKC studies, again we see that “environmental quality” is measured exclusively as the abstraction of “emissions control”.

In an important paragraph for this paper, Zheng and Kahn (2013:749) summarize their work: "Throughout this section, we have focused on how China's urban growth affects local environmental quality but many externalities feature degradation of regional and global public goods. For example, the creation of greenhouse gas emissions is a global public good." In this section they also reference Wackernagel's “ecological footprint”, a measurement of sustainability that translates environmental impacts into the amount of land needed to offset those flows. In other words, much like Copeland and Taylor (2004), they acknowledge the one-sidedness of their intervention but go a bit further by making explicit reference to an approach that would make EKC studies considerably more ecological in depth and, hence, more significant. But here, again, we find evidence within the mainstream sample of type I critiques as well as something new for this sample: an unambiguous acknowledgement invalidating the generalized relation hypothesized by the EKC.

In all three of these most important mainstream engagements with the EKC hypothesis, we see that type II critiques are variously broached, but are never pursued as central in their own right. Consistently, the abstraction of “environmental quality” is accepted uncritically while the authors' critical energies seek to clarify the heterogeneity lying beyond the abstraction of “per capita income”. This despite the fact that the rise and fall of the environmental Kuznets curve depends on the relation between both of these two abstractions. Considered chronologically,

we see that the status of a generalized EKC relation receives strong support in Dasgupta et al. (2002), is regarded ambivalently with Copeland and Taylor (2004), and is explicitly rejected in Zheng and Kahn (2013). But if the mainstream sample merely broaches type II critiques while developing type I critiques, what does it look like to develop a type II critique? To answer this question, we now turn to the interdisciplinary sample.

3.3.2 Developing Type II Critiques

The problem for *York et al. (2003)*, who published one of the earliest articles in the interdisciplinary sample, is methodological: according to ecological consensus humans have dramatically changed the environment since industrialization in terms of the alteration of the chemical composition of the atmosphere, global changes in land coverage, alterations of major biogeochemical cycles, and a greatly accelerated rate of species extinction. While impact identities have existed for a number of decades, the challenge has been to model these impact equations in order to allow for hypothesis testing. Their solution is STIRPAT (the stochastic impacts by regression on population, affluence, and technology) with measurements of ecological elasticity. This model not only allows for hypothesis testing, but is able to measure "the responsiveness or sensitivity of environmental impacts to a change in any of the driving forces" (York et al. 2003:354). They argue that this better equips policy makers to face the challenges of ecology in the 21st century.

Their data suggest that changes in population result in proportional (i.e., unit elastic) changes in both carbon emissions and energy footprints (a composite measurement of the amount of productive land needed to service the absorption of carbon emissions, as well as the land area associated with nuclear power, hydropower, and fuel wood energy). The other major driver, affluence, is the independent variable of the EKC hypothesis. For carbon emissions, their

model suggests that lower levels of per capita income are elastic but become inelastic after \$2900, though total emissions are implied to continue an inelastic rise until per capita income reaches \$61,000. Because this figure lies far outside the range of observable income levels, the function is practically monotonic. In other words, if there is a carbon EKC it would appear to be out of reach. Similarly, the model also suggests that the energy footprint is also a monotonic function of affluence. However, the key difference is that changes in income are associated with an inelastic increase for income levels below \$22,850, but thereafter they increase elastically. In other words, the amount of land needed to service energy demands increases monotonically and at an increasing rate for every dollar after this inflection point. This is the exact opposite of the EKC hypothesis.

This is clear theoretical evidence of a type II critique as they begin not with the problem of investigating what lies beyond the abstraction of “per capita income”, but with the ecological consensus that human activity is a driving force of ecological change. Furthermore, unwilling to accept the static abstraction of localized air and water “pollutants” they use indexed “energy footprints” which more accurately reflects the dynamic complexity of ecology. Finally, York et al. (2003) empirically apply their model which suggest that, whatever heterogeneous activity lies beyond the abstraction of “per capita income”, it along with population are the major drivers of carbon emissions and energy footprints. Here, in the earliest article from the interdisciplinary sample we find evidence of both type II critiques as well as a conclusion invalidating the existence of the generalized EKC hypothesis.

Dinda's (2004:432) interdisciplinary contribution also exemplifies type II critiques in that the structure and focus of his review is to highlight that “[a]n Environmental Kuznets Curve reveals how a technically specified measurement of environmental quality changes as the fortunes of a country or a large human community change.” In other words, Dinda (2004) presents different

reasons for why the hypothesized EKC relation should not be considered to have generalized applicability which focus on the specification of “environmental quality”. He argues that this is the case for a number of reasons. For example, major indicators of environmental degradation rise monotonically. Observed instances of the EKC relation are generally limited to localized urban air and water indicators that cause immediate impacts and do not extend to globalized indicators with less immediate environmental impacts. Still other indicators rise, fall, and rise again revealing an “N” (as opposed to inverted-U) shaped curve. Unlike other studies that mildly acknowledge these problems in scattered qualifications to their central conclusion that the EKC exists, Dinda (2004) systematically organizes these critiques towards the conclusions that: 1) the EKC is one of many possible scenarios that an environmental indicator and economic growth could follow, and 2) determining its generalized applicability necessitates grappling with some decidedly ecological questions. For example,

1. is the EKC hypothesis valid for all types environmental indicators?
2. is it permanent?
3. is it valid for both individual countries and the world?
4. does it follow a sustainable development path?

Dinda (2004:448-50) concludes his survey by elaborating some important type II critiques:

Empirical studies have mostly used absolute measures of pollution like amount of emission or pollution rather than a relative measure (like pollution or emission per unit of output or per square kilometer, etc.)

[T]he concept of EKC cannot be applied to all the environmental factors. For example, land use change and/or biodiversity loss, which are irreversible, are conceptually different from air or water pollution.

Using environmental degradation index (EDI) and an appropriate measure of economic development (i.e., Human Development Index [HDI]), one can develop a global EKC model. It should be noted that empirical support for the existence of a global EKC for CO₂ emissions has not been found.

The non-availability of actual data on environmental quality is the major limitation of all EKC studies. Truly speaking, environmental quality is something that is not easy to be measured accurately. Therefore, an index of environmental quality, which could be a better measurement, should be developed and used to examine the EKC hypothesis.

More structural forms may warrant exploration, for some interdependence in our environmental indicators is probable. For example, use of pesticide may destroy useful insects or micro-organisms (and thus hamper soil fertility) or threaten bird species, but this kind of interdependence remains yet to be explored.

All of these critiques drive at Dinda's (2004:450) most basic point: "[W]e need economic models, which properly reflect the physical and ecological basis of economic activity, and important feedback between the economy and the environment." Here, we find evidence of another article from the interdisciplinary sample that both consistently develops type II critiques as well as invalidates the existence of the generalized relation hypothesized by the EKC.

The brunt of Stern's (2004:1420) interdisciplinary critical engagement with the EKC is classifiable as type I. Stern's main argument is that when the proper diagnostic statistic and specification tests are applied "we find that the EKC does not exist", that the actual hypothesized relation often varies considerably, and that many emissions and flows of waste rise monotonically. However, his critical engagement also includes type II critiques. For example, Stern (2004:1426) highlights the theoretical significance of incorporating an environmental indicator that estimates the total impact of economic activity and references two studies that have done this by way of "total energy use". Both authors found that this measure rises monotonically with per capita income. Finally, Stern (2004:1426) clearly grasps the interconnected nature of ecology:

Economic activity is inevitably environmentally disruptive in some way. Satisfying the material needs of people requires the use and disturbance of energy flows and material stocks. Therefore, an effort to reduce some environmental impacts may just aggravate other problems.

Here again, we find more evidence of type II critiques in the interdisciplinary sample invalidating the existence of a generalized EKC hypothesis.

In all three of these most important interdisciplinary engagements with the EKC hypothesis, we see that type II critiques are not merely broached, but often the central focus of the article. Consistently, the abstraction of “environmental quality” is critically problematized in order to consider the second order question of whether or not a given indicator of environmental quality is simultaneously an ecologically fit measurement of change. Unlike the mainstream sample which slowly drifted away from support of a generalized EKC relation, the interdisciplinary sample consistently concluded against the generalized relation whenever type II critiques were present.

3.3.3 Comparing Their References

If we can provide evidence of this divergence in critical thought by comparing the structure, focus, and conclusions of the articles from the two samples, should we not also expect that this divergence exists in their reading or understanding of the references they cite? By and large a comparison of the 30 references shared across samples by a total of three or more authors does not reveal any significant evidence of radically divergent interpretations of sources. For the most part, referenced articles are read very similarly, often with only insignificant variations in emphasis. In Appendix I, I present a detailed consideration of the two examples of shared references with the broadest range of interpretation across samples: Stern and Common (2001) and Arrow et al. (1995). But again, they are not revelatory to this paper's thesis.

So is it worth pursuing these types of comparisons? There might be further evidence of the origin of this divergence in critical thought in the alternative interpretation of shared references between dyads (so far I have only considered triads and quadrads). Or, perhaps, the

evidence explaining why their critical engagement varies is in the isolated references cited by individual articles. Perhaps.

But I expect the answer to this type of question just as easily lies beyond our ability to positively pin down. For example, maybe some authors spent more time reading and thinking about the ecological side of the EKC's hypothesized relation. Possibly some authors enjoyed the benefit of a liberal education which included a proper introductory class in ecology that was left to the care of an exceptional instructor. There are likely innumerable scenarios that could explain this divergence with equal plausibility, but I am not sure they could be demonstrated and do not believe their discovery would represent a significant contribution to this paper.

3.4. Divergence in Critical Thought: Quantitative

In this section I bolster my argument that there is a non-random distribution of critical thought by sample using a more quantitative approach to the content analysis of my data. Turning to the article's references, I ask whether or not we find a higher ratio of articles from environmental journals in the sample of interdisciplinary thought. If this happens to be the case, this would help to explain why it is only in this sample that we find evidence of ecological thought via type II critiques. The logic being: the more one reads journals that express an explicit interest in the environment, the more sensitive we might expect them to be to a basic and consistent framework of ecological thought. My second question quantifies the frequency of different terms used to reference to the dependent variable of the EKC hypothesis (i.e., "the environment") in order to determine if there is a non-random distribution in these terms. If my argument is correct, we should expect to find evidence that terms more ecologically indicative are concentrated in the interdisciplinary sample. More detailed tables are included in appendix II.

Table 4: Ratio of Environmental Journal References to non-Environmental Journal References (Abridged)

Ratio of Environmental Journal References to non-Environmental Journal References by Sample Articles (Abridged)		
	Ratio of Environmental Journal Articles to Total References:	Ratio of Environmental Journal Articles to Total Journal Articles:
Interdisciplinary Sample with Type II Critiques:		
York et al. (2003)	0.30	0.47
Dinda (2004)	0.44	0.66
Stern (2004)	0.41	0.58
Mainstream Sample without Type II Critiques:		
Dasgupta et al. (2002)	0.30	0.52
Copeland and Taylor (2004)	0.17	0.26
Zheng and Kahn (2013)	0.14	0.20

Across articles and samples we see that roughly two-thirds of references are from journals. The other third is generally reports, conference papers, books, and the occasional news article. Listing all of the unique journals that appear allows us to group them as environmental or not. This can be determined by their titles or by reading the descriptions on their websites. Comparing the number of unique journals referenced doesn't reveal strong confirmation of a pattern. Comparing the total number of references from environmental journals in each article reveals some evidence that the interdisciplinary samples has a stronger showing. Finally, when we compare the ratios of environmental journal articles to total references and total journal references, we see that the interdisciplinary sample references environmental journals more, almost without exception.

Table 5: Frequency of Selected Terms (Abridged)

Frequency of Select Terms (Abridged)				
	ratio of terms to pages			
	"pollution"	"environmental quality" etc.*	"indicators"	"ecology"
Interdisciplinary Sample:				
York et al. (2003)	0.07	0.93	0.60	1.33
Dinda (2004)	4.96	5.04	1.38	0.17
Stern (2004)	3.00	1.65	0.45	0.00
Mainstream Sample:				
Copeland and Taylor (2004)	9.44	0.75	0.03	0.00
Dasgupta et al. (2002)	6.95	1.14	0.10	0.00
Zheng and Kahn (2013)	3.44	0.56	0.22	0.07

* in addition to "quality" the following terms are included: degradation, pressure, improvement, impact, consequences, harm, outcomes, damage, risks, concerns, hazards

In table 5 selected terms are presented in order to determine whether or not they are randomly distributed. All of these terms are used to reference the same thing: the EKC hypothesis's dependent variable (i.e., the environment). The first term, "pollution" (and all derivations from its root "pollut-") is by far the narrowest abstraction for conceptualizing the measurement of the dependent variable. The second list of terms shorthanded as "environmental quality" represents a level of abstraction less narrow than pollution and hence conceptually more open to ecologically fit measurements of the dependent variable. Likewise, "Indicator/s" is also more conceptually open to ecologically fit measurements than "pollution". Finally, "ecology" (and all derivations from its root "ecolog-") is the term most open to conceptualizing the fullness of the dependent variable. Comparing the incidences and ratios (i.e., incidence : number of pages in an article) across samples, we see that there is an identifiable trend to use terms more inclusive to ecological thought or expression in the interdisciplinary sample. While I do not consider the evidence presented in either of tables four and five to be definitive, they are notable within the context of the earlier qualitative analysis, the basic argument from which they tend to bolster.

3.5 Divergence in Critical Thought: Conclusion

As was the case with the first finding, this, my second finding, has also been characterized by a further "absence". Largely absent from the mainstream sample is the development of any type II critiques. In this sample we see that type II critiques are broached, misunderstood, and even acknowledged, but never do we see an active or sustained critical engagement with the environmental Kuznets curve hypothesis's dependent variable. Turning to the interdisciplinary sample, we find that type II critiques are both central to the structure, focus, and conclusions of their articles. They are even developed and applied in novel studies. While I consider this

qualitative analysis of the article's structure and problem to be the most revealing and direct exposition of this divergence in critical thought, a quantitative analysis of key terms also bolsters this general finding. Hence, we find evidence of a non-random distribution of critical thought that correlates with the sample grouping.

3.6 The Political-Economic Nature of Successful EKC

Finally, every single article sampled agreed that the viability of an EKC relationship depends on extra-market regulatory regimes, that successful instances of the relation are not the automatic result of the regulatory regime of liberalized markets. Although discussion on policy, regulatory capacity, and enforcement are discussed throughout most articles, direct statements of the importance of political regulations are sometimes made as well. For example, in the mainstream, Dasgupta et al. (2002:152,163) write that:

However the available evidence suggest that regulation is the dominant factor in explaining the decline in pollution as countries grow beyond middle-income status.

If per capita income and environmental quality are to increase together, developing countries will require effective regulatory capabilities.

While Dinda (2004:439,442) states categorically in the interdisciplinary sample that

Pollution grows unless environmental regulation is strengthened.

Stern (2004:1421), also from the interdisciplinary sample, notes that regulation has long been established as part of the general EKC logic:

at higher levels of development, structural change towards information-intensive industries and services, coupled with increased environmental awareness, enforcement of environmental regulations, better technology and higher environmental expenditures, result in leveling off and gradual decline of environmental degradation (Panayotou, 1993, p. 1).

Again, in the mainstream sample, Copeland and Taylor (2004:8) note:

Moreover, there are strong indicators that this income effect works because increases in the stringency of environmental regulation accompany higher per-capita incomes.

CHAPTER IV
A DISCIPLINE IN CRISIS?

4.1 A Postpositivist Paradigmatic Shift

When we consider the problem of the environment represented by the case study of the EKC hypothesis, we find ourselves at something of a dead end. At the moment, decades of empirical and theoretical debates have seen modernization theorists steadily losing ground and struggling to re-invent themselves. As Perz (2007) has pointed out, this continuity has been almost entirely limited to the development of social theories about the environment. My own review of the EKC literature suggests that it is losing ground and struggling on this newer front as well. Faced with a situation in which the debate appears to have stalled without a decisive victory, my strategy has been to attempt to consolidate what we can learn about our competing theoretical perspectives by evaluating this history as a problematic. While this task lies beyond the immediate scope of this paper and should be accompanied with a proper review of ideological critique, the findings of this paper do indicate the importance of such a project. Consider the three basic findings:

1. the economic literature associated with the discipline's orthodoxy has all but ignored the problem of the environment presented in the case of the EKC hypothesis (i.e., economists most robust theory of economy and environment);
2. where the EKC debate has been engaged in mainstream journals, critical thought has systematically mischaracterized the nature of the problem by failing to understand the foundational principles of ecology;

3. all of the authors reviewed here agreed that extra-economic or political regulations are necessary for the improvement of environmental indicators (i.e., the market doesn't mysteriously self-regulate this problem).

Finding (3) highlights the well know political-economic contradiction which raises questions about the possibility and meaning of an independent science of economics. Simply put, what scientific knowledge stipulates when some category of human behavior should be governed by economic organizational regimes and when others should be governed by extra-economic organizational regimes? This is not a simple question. But in spite its foundational importance for the progression of the discipline of economics, it rarely even registers as a topic of passing concern. As we begin to understand the timely importance and complex interrelation of economic and ecologic processes, finding (3) gives us secure empirical grounding for investigating the foundations of economic knowledge.

Superficially, finding (2) isn't incredibly revelatory. We should intuitively expect that economist and ecologists will approach a problem that concerns them both from the different perspectives of their disciplines. The importance of finding (2) is more general. Economists need to recognize economic behavior as having a constitutive role within the ecological process. In light of the debate over the legitimacy of the EKC hypothesis, it has become increasingly difficult to even entertain that old positivist orthodoxy that viewed the science of economics as some fully formed universal knowledge isolated from the rest of reality. Post neoclassical economists face a serious question: how is disciplinary progress to proceed absent its organizational paradigm which has been roundly critiqued but nonetheless remains the orthodoxy? In Kuhnian terms, the discipline finds itself in a crisis but without a well-defined alternative paradigm towards which to shift. Instead, we find fractured critiques coming from the whole of the social and behavioral

sciences: psychological economics, the regulation or historical schools of economics, new political economists, institutional economics, as well as long standing sociological and anthropological critiques.

I believe considering the problems facing economic theory from the vantage of socio-ecology builds upon all of these critiques while revealing the extent of the crisis at new depths. For example, having acknowledged the possibility that our economic activity may be eroding the base from which it reproduces itself, how can we ignore the categorical absence in equilibrium theory for the objective dimension of our values? Theorizing value exclusively with the marginalist theory of value doesn't allow space for the recognition of the reflexivity of our action. In reality it has become clear that our subjective valuations play a role in constituting our environment which shapes the possibility of all future valuations. This is most striking when considering the possibility that the imperative of exponential growth could be approaching natural limitations. When we insist that economics return from the celestial clouds of some allegedly completed science and reconcile itself with the objective history its subjective theory helps constitute, we are faced with the challenge of retooling disciplinary terms like "efficiency" and "value" to recognize imperatives other than growth, imperatives (like ecology) which are presupposed by even the most narrowly technical definitions of these terms.

Finally, finding (1) suggests the up-hill battle facing this important process of interdisciplinary critique. Economists have their autonomy and can achieve a high degree of isolation from critiques that do not arise internally from their own institutionalized knowledge and practice. However, we are fortunate enough to find the topic of the EKC hypothesis broached in mainstream journals as well as the much more lively discussions in the interdisciplinary journals. This is crucial as it allows us to engage with a recognized literature and avoid being

charged as mere polemicists. But this also underscores the importance of the university as a place which needs to value more than technical knowledge.

This has long been recognized as the providence of the philosophy of science. We may be tempted to remember the Copernican revolution as a mere paradigm shift that happened more or less inexorably with the accumulation of contradictions in the geocentric model of the cosmos (i.e., as a shift between technical understandings). But Kant was wise to claim his own 'Copernican revolution' in recognition of what this great perspective shift tells us about the structure of thought, the process of critique, and the dialectical role of reason.

While it is beyond the scope of this paper to review the development and practice of critique in philosophy, the basic argument is worth broaching. Since Kuhn it has been widely recognized that the process of science is necessarily a social process. But the importance of critique continues to lag behind that of the acquisition of the technical understandings in the sciences. Put simply, the thesis is that the progress of science requires both the acquisition of imperfect technical understandings as well as a theory of ideological critique that is able to reason between incommensurable understandings. Elsewhere, I advocate the ontological turn of the critical realists as the most mature development in this direction of a postpositivist science.

CHAPTER V

THE ROLE OF IDEOLOGICAL CRITIQUE IN POSTPOSITIVIST SCIENCE

5.1 Beyond Falsificationism

In an understudied debate that saw the critical realists (Adorno and Habermas) square off against the critical rationalists (Popper and Albert), the question of critique took center stage.³ They disputed the question of how the process of critique should be understood. In reversing the positivist traditions of verificationism, Popper had famously argued that for a hypothesis to be scientific it must be falsifiable, that there must be experimental designs that could prove the theory wrong. In considering the EKC hypothesis I have argued that this particular hypothesis has frustrated the logic of Popper's falsificationism because whether or not a given study validated or falsified the hypothesis appeared to depend on the theoretical perspective employed. Different theoretical perspectives measured the same phenomena differently and these different measures often informed the conclusion of the experimental design. It was precisely in this sense that Adorno and Habermas critiqued Popper's notion of critique as ideologically naive and insisted upon a theory of ideological critique. In Habermas's words:

He [Popper] rightly discredits every form of primary knowledge, but even mistakes can only be found to be such on the basis of criteria of validation. For their justification we must adduce arguments; but where then are we to look for these if not in precisely that dimension--not of the origin but namely of the formation of knowledge--which has been ruled out? Otherwise the standards of falsification remain arbitrary....He assumes the epistemological independence of facts from the theories which should descriptively grasp these facts and the relations between them. Accordingly, tests examine theories against "independent" facts. This thesis is the pivot of the positivistic problematic which Popper still retains. (quoted in Giddens 1974:19)

My strategy in this paper has been to move beyond the pragmatic problem presented by the EKC hypothesis and to argue that it is more productive to confront it instead as a problematic.

³ The main contributions in this debate have been collected, translated (by Glyn Adey and David Frisby), and published as *The Positivist Dispute in German Sociology: Theodor W. Adorno et al.* (1976) Heinemann Educational Books, London

By documenting a divergence in critical thought between alternative theoretical perspectives, I have argued (contra Popper) that accepting thought as spontaneous is in principle unscientific and have instead argued (à la Adorno and Habermas) that ideological critique is as central to the progress of any science as is the acquisition of its technical knowledge.

Beyond falsificationism remains the task of systematically incorporating ideological critique into postpositivist science. In the case of the dispute between the critical theorists and critical rationalists, the critical theorists' point was that postpositivist science must overcome positivism's scientism and recognize the role of negative dialectics in revealing the limits of knowledge. The inability of the scientific methods of 19th century naturalism to comprehend the dimensions of social-psychology does not entail that these questions are in principle unscientific. If we instead follow the critical realist thesis of epistemological relativity, the ontological natures of these domains of reality only imply various natural limitations that determine the form of the possible science. Indeed, on the critical realist view, the intelligibility of any scientific activity and coherent conception of naturalism requires just such a concept of an ontological depth. In such a case, the critical realist practice of "explanatory critique" may hold some promise towards this end of elaborating a postpositivist sociological science.

APPENDIX A:

Comparing References

Dinda (2004), Stern (2004), and Copeland and Taylor (2004) all include Stern and Common (2001) in their respective bibliographies. While there is a significant difference in interpretation by sample, it focuses on a type I critique. For Dinda (2004:446) Stern and Common (2001) appear in the section "Is the EKC valid both for individual countries and the world?" where he considers the important contribution of the reference to be having provided evidence that "[t]he results of panel countries and that of individual or sub-sample countries vary widely." In other words, Stern and Common (2001) provide evidence that the EKC may not be valid both for individual countries and the world. In Stern (2004:1424,1429), Stern and Common (2001) appear in the sections "Result of EKC Studies" and "Econometric Critique of the EKC". For Stern, the significance of his earlier article is that it provides evidence that when the sample is augmented to include the lower income ranges of non-OECD countries, the estimated turning point for the sulfur data rises spectacularly. By way of reference, recall that the original Grossman and Krueger (1995) study estimated a turning point around \$4000 for sulfur emissions. Stern and Common's (2001) study estimates an out-of-sample turning point of just over \$100,000. Hence, for Stern (2004) the important point is that EKC studies suffer from omitted variable bias. Both of these interdisciplinary articles give the citation a slightly different emphasis, but capture the same strong critique of the EKC hypothesis.

Copeland and Taylor (2004:20) summarize Stern and Common (2001) in a single sentence: they "use data on sulfur emissions in 73 countries over 31 years, and by comparing OECD and non-OECD subsamples conclude that the evidence does not support a common EKC across countries." It appears in the much more general section "Growth and the Environment". Now, while their interpretation certainly isn't untrue, there is a notable difference in saying that Stern

and Common's (2001) study doesn't support a common EKC across countries and saying that once the omitted variables are included, the relation practically disappears. Without this latter piece of information, one might easily conclude that there is nothing terribly significant in the variation of EKCs across countries and that the relation more or less exists with statistically acceptable levels of variance. This is certainly interesting, but if we want to consider this as being evidence of anything it should be noted that this is not a conflict of a type II critique concerned with the measurement of "environmental quality", but centers on a type I critique concerning the omitted variable bias of low-income countries.

Turning to the second example, again we don't find anything too spectacular. Dinda (2004), Stern (2004), and Copeland and Taylor (2004) also all include Arrow et al. (1995) in their lists of references. Here we almost find some qualitative evidence of a divergence in type II critical thought. For Dinda (2004:447) one of the key points of the Arrow et al. (1995) is included in his article's section "Does the EKC follow a sustainable development path?". Referencing Arrow et al. (1995), he concludes: "There is no guarantee that the rising part and top of the EKC bypass ecological thresholds and sustainability constraints beyond which environmental deterioration is irreversible." Similarly, in Stern (2004:1426) this ecological aspect of Arrow et al. (1995) is also recognized: "The key criticism of Arrow et al. (1995) and others was that the EKC model...assumes that there is no feedback for environmental damage to economic production as income is assumed to be an exogenous variable....In other words, there is an assumption that the economy is sustainable." However, in Copeland and Taylor (2004:20), Arrow et al. (1995) appears only in footnote 30 following a discussion on the role of international markets. The section title is, again, the much more general "Growth and the Environment". The significant point they reference from Arrow et al. (1995) --which also appears in Dinda (2004) and Stern (2004)--is that if the EKC results from the displacement hypothesis and not income effects, then it couldn't be a universal

phenomenon available to rich and poor countries alike. These differences in interpretation are rather extreme as the former highlights that existing EKC models determine their hypothesized conclusion by excluding the possibility of environmental feedback.

Now this omission is not as severe as it may at first appear. After all, Copeland and Taylor do introduce a model that endogenously links real income (R) with pollution (Z). While it may seem curious that they should omit reference to Arrow et al.'s (1995) influential role in highlighting a critique central to their own intervention, I do not believe there is anything here which significantly contributes to my original question.

APPENDIX B:

Tables

Table 1: Articles from Top 10 Journals

Articles from Top 10 Journals				
Journal:	Rank:	Impact Factor:	Articles:	Citations:
Journal of Economic Literature	1st	6.341	Zheng and Kahn (2013)	2
			Graff-Zivin and Neidell (2013)	2
			Kitzmüller et al. (2012)	10
			Copeland and Taylor (2004)	81
Journal of Economic Perspectives	4th	4.23	Föll (2009)	68
			Dasgupta et al. (2002)	90
			Esty (2001)	16
			Portney (2000)	4

Table 2: Articles from Interdisciplinary Sample

Articles from Interdisciplinary Sample				
Journal:	Rank:	Impact Factor:	Articles:	Citations:
Journal of Env Econ and Management	32nd	2.522	Stern and Common (2001)	133
Ecological Economics	33rd	2.517	Soytas and Sari (2009)	105
			Zhang and Cheng (2009)	117
			Soytes et al. (2007)	128
			Cole (2004)	104
			Dinda (2004)	279
			York et al. (2003)	162
World Development	62nd	1.733	Stern (2004)	401

Table 3: Distribution of Critiques by Sample

Distribution of Critiques by Sample				
Top-ten Journals:	Regulations	Type I	Type II	
Zheng and Kahn (2013)	x	x		
Graff and Neidell (2013)	x			
Copeland and Taylor (2004)	x	x		
Dasgupta et al. (2002)	x	x		
Esty (2001)	x	x		
Interdisciplinary Journals:				
Soytas and Sari (2009)	x	x		
Zhang and Cheng (2009)	x	x		
Soytas et al. (2007)	x	x		
Stern (2004)	x	x		x
Cole (2004)	x	x		
Dinda (2004)	x	x		x
York et al. (2003)	x	x		x
Stern and Common (2001)	x	x		

Table 4: Ratio of Environmental Journal Reference to non-Environmental Journal References

	Ratio of Environmental Journal References to non-Environmental Journal References by Sample Articles					
	Interdisciplinary Sample with Type II Critiques		Mainstream Sample without Type II Critiques			
	York et al. (2003)	Dinda (2004)	Stern (2004)	Dasgupta et al. (2002)	Copeland and Taylor (2004)	Zheng and Kahn (2013)
Number of Journal References:	30	119	73	48	94	154
Ratio of Journal References to Total References:	0.65	0.66	0.72	0.58	0.65	0.71
Number of Unique Environmental Journals Referenced:	6	15	16	8	9	22
Total Number of References from Environmental Journals:	14	79	42	25	24	31
Ratio of Environmental Journal Articles to Total References:	0.30	0.44	0.41	0.30	0.17	0.14
Ratio of Environmental Journal Articles to Total Journal Articles:	0.47	0.66	0.58	0.52	0.26	0.20

Table 5: Frequency of Selected Terms

	Frequency of Select Terms								
	Incidence of terms			ratio of terms to pages					
	"pollution"	"environmental quality" etc. *	"indicators"	"ecology"	pages	"pollution"	"environmental quality" etc. *	"indicators"	"ecology"
Interdisciplinary Sample:									
York et al. (2003)	1	14	9	20	15	0.07	0.93	0.60	1.33
Dinda (2004)	119	121	33	4	24	4.96	5.04	1.38	0.17
Stern (2004)	60	33	9	0	20	3.00	1.65	0.45	0.00
Mainstream Sample:									
Copeland and Taylor (2004)	604	48	2	0	64	9.44	0.75	0.03	0.00
Dasgupta et al. (2002)	146	24	2	0	21	6.95	1.14	0.10	0.00
Zheng and Kahn (2013)	141	23	9	3	41	3.44	0.56	0.22	0.07

* In addition to "quality" the following terms are included: degradation, pressure, improvement, impact, consequences, harm, outcomes, damage, risks, concerns, hazards

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