

Cultural Economics: The State of the Art and Perspectives

BRUCE A. SEAMAN

Andrew Young School of Policy Studies

Department of Economics

GEORGIA STATE UNIVERSITY

e-mail: bseaman@gsu.edu

ABSTRACT

The intellectual development of cultural economics has exhibited some notable similarities to the challenges faced by researchers pioneering in other areas of economics. While this is not really surprising, previous reviews of this literature have not focused on such patterns. Specifically, the methodology and normative implications of the field of industrial organization and antitrust policy suggest a series of stages identified here as *foundation*, *maturation*, *reevaluation*, and *backlash* that suggest a way of viewing the development of and controversies surrounding cultural economics. Also, the emerging field of sports economics, which already shares some substantive similarities to the questions addressed in cultural economics, presents a pattern of development by which core questions and principles are identified in a *fragmented* literature, which then slowly coalesces and becomes *consolidated* into a more unified literature that essentially reconfirms and extends those earlier core principles. This *fragmentation* and *consolidation* pattern is also exhibited by the development of cultural economics. While others could surely suggest different parallels in the search for such developmental patterns, this way of organizing ones thinking about the past and future of this field provides a hoped for alternative perspective on the state of the art of cultural economics.

Keywords: Methodology; Scientific Development; Cultural Economics; Research and Policy.

Economía de la cultura: Estado del arte y perspectivas

RESUMEN

El desarrollo intelectual de la Economía de la cultura ha manifestado ciertas similitudes notables a los desafíos encarados por los investigadores que abren nuevos caminos en otras áreas de la economía. Aunque esto en realidad no resulta muy sorprendente, las revisiones previas de esta literatura no se han ocupado de tales modelos. Específicamente, la metodología y las implicaciones normativas del campo de la organización industrial y las políticas antitrust sugieren una serie de etapas identificadas aquí como la *base*, la *maduración*, la *reevaluación*, y la *reacción* que sugiere una manera de visualizar el desarrollo y las controversias alrededor de la economía de la cultura. Del mismo modo, el campo emergente de la economía del deporte, que comparte ya algunas similitudes significativas con las cuestiones tratadas en la economía de la cultura, presenta un modelo de desarrollo en el cual se identifican los asuntos y principios centrales en una literatura dispersa, que paulatinamente se va uniendo y acaba consolidándose en una literatura más unificada que básicamente reafirma y amplía esos principios centrales ya establecidos. El desarrollo de la economía de la cultura también evidencia este modelo de *fragmentación* y *consolidación*. Aunque es probable que otros pudieran postular comparaciones diferentes a la hora de buscar tales modelos de desarrollo, esta manera de organizar nuestro razonamiento sobre el pasado y el futuro en este campo ofrece una perspectiva alternativa acerca del estado del arte en el campo de la economía de la cultura.

Palabras clave: Metodología; desarrollo científico; Economía de la cultura; investigación y política.

Clasificación JEL: A14, B41, L82, Z11.

Artículo recibido en marzo de 2009 y aceptado en abril de 2009.

Artículo disponible en versión electrónica en la página www.revista-eea.net, ref. @-27117.

1. INTRODUCTION: CULTURAL ECONOMICS AND LESSONS FROM OTHER FIELDS

The stages of intellectual development of an academic discipline can be fascinating to monitor. Sometimes these stages become so visible that they become part of the common language used by even contemporary scholars (as exhibited, for example, by the “pre-Chicago,” “Chicago” and “post-Chicago” schools in industrial organization—terminology that is fully recognizable to any specialist in the field), in contrast to schools of thought being identified and named primarily in the light of historical hindsight (e.g., marginalism, institutionalism, and neoclassicism). Even in the absence of identifiable intellectual camps, the proliferation of fields and sub-fields of study within economics often exhibits revealing patterns of similarity and contrast, as has been the case with sports economics and cultural economics—both dealing with segments of the entertainment industry employing highly talented and heterogeneous labor inputs frequently in a live setting in which production and consumption of the product occur simultaneously¹.

Most broad surveys of the state of cultural economics are organized around specific topic areas such as taste formation, demand and supply, industrial organization, the art market, economic history of the arts, artists’ labor markets, Baumol’s cost disease, and public subsidies (Blaug, 2001). The earlier influential and widely visible Throsby (1994) list was not entirely overlapping and consisted of taste for the arts, markets for arts works, performing arts (with sub-categories of demand, production and cost, firm structure and behavior, technology and the “income gap”), labor market for artists (divided into labor supply, and earnings functions and career choice), and public arts policy (distinguishing positive from normative aspects). In both cases, the primary focus was the worthy goal of informing non-specialists about the questions cultural (or arts) economists had been asking, and how successful they were in answering them over the period since the publication by Baumol and Bowen of *Performing Arts: The Economic Dilemma* (1966), identified by them (and everyone else) as the *de facto* founding point of the field in “contemporary” form. Of course, there have been other broad surveys of the field, and Throsby (2006) provides a further useful “survey of the surveys” of cultural economics (pp. 4-6).

Another way to describe the broad topic coverage of cultural economics is to identify the seven instances in which the *Journal of Cultural Economics* has published a special issue, starting with the first in 1995 and ending with the most re-

¹ Others will quite correctly emphasize other fields with close subject matter and methodological ties to cultural economics. Thus, Towse (2008, p. 245) suggests that environmental, education, and health economics have the closest substantive ties to cultural economics. Similarly, Blaug (2001, p. 133) compares the progress of cultural economics favorably to the “moribund” research agenda of education economics, but less favorably to the progress made in health economics. The emphasis here on sports economics stems from some revealing patterns of how that field developed rather than from the substantive similarities it also clearly shares with cultural/arts economics (on the labor market similarities, see Seaman, 2003).

cent ten years later in 2005. The 19(2) 1995 issue was devoted to the economics of intellectual property rights, followed by the 20(3) 1996 issue focusing on the 30th anniversary of the publication of *The Performing Arts: An Economic Dilemma* (Baumol and Bowen, 1966). The 21(3) 1997 was devoted to the art market, and the following year a double issue (22, nos. 2-3, 1998) focused on the economics of museums. There was a tribute to the influence of Netzer's (1978) *Subsidized Muse* in the 23(1) 1999 issue. The assemblage of an especially noteworthy plenary panel at the 11th ACEI International Conference in Minneapolis led to a special issue (25 (4), 2001) devoted to those four papers (three on some aspect of artists' legal rights, and one examining the evolution of free lance music from 1650 to 1900). The mass proliferation of papers applying the contingent valuation approach to the arts (in part as a reaction to recognized methodological limitations to spending flow economic impact studies) required another double issue in 2003 (27, nos. 3-4). The final full special issue 29(3) in 2005 assembled papers on the movie industry. Part of the 32(4) issue in 2008 was devoted to a "Symposium on Patrons Despite Themselves", a tribute to the highly influential study by Feld, O; Hare and Schuster (1983) of indirect government support for nonprofit cultural institutions via favorable tax treatment of private contributions in the United States.

In their earlier reviews, both Throsby (1994, p. 26) and Blaug (2001, p. 133) agreed that progress had been made, but there was still much to be learned from the continuing application to the arts of the powerful theoretical and empirical tools of standard economic analysis, with Throsby adding a particular plea for urgency in developing better data in the arts. Throsby was less positive in tone for a more restricted audience just one year later, when he followed his recognition of the "evolving maturity" of cultural economics with the observation that "in one sense we have come no distance at all," identifying particular subject areas where "we are still searching for sound theories, good analysis, comprehensive data, or all of the above" (Throsby, 1995, p. 199). Blaug added a dispiriting note in bemoaning the inability of cultural economics to reciprocate durable new ideas to the larger discipline (at the still young age of only about 30 years as of 2001), i.e., to "suggest and promote developments that would spill over with benefit to economics and econometrics outside its own domain" (p. 133). In this he was joined soon after by Hutter (1994), who identified six "construction sites" (p. 264 some quite narrow and others going to the core of economic inquiry) of ongoing cultural economics research that might be expected to show promise of having an impact back upon economic theory in contrast to merely being another avenue for the application of existing economic theory. Of the six such construction site "agendas" (Baumol's cost disease; further clarification of public goods theory; dynamics of acquiring taste rather than just presuming given tastes; the rate of return on paintings; investigations of the impact of new technologies on production and consumption patterns; and the ancient issue of the theory of value), he concludes that this impact "begins to show only in a very few instances," with his primary example being the cost disease concept (p. 267).

Notably, both Throsby (1994) and Blaug (2001) made separate final points of special significance. Throsby (p. 26) reminded us that the arts and culture present special challenges that push researchers toward more tolerance for going beyond “tradition-bound” economics to embrace the potential contributions of other fields such as cognitive psychology and aesthetics. This tension that has existed within cultural economics regarding the proper methodology is a theme addressed in more detail in Section 5 below. Given the increasing impact that experimental methods and behavioral economics (with close ties to psychology) are having on the broader field of economics, and even on current public policy in the United States (Grunwald, 2009), the degree to which cultural economics has or has not embraced these new ideas is an important issue in evaluating its development. Meanwhile, Blaug (p. 133) makes the sobering point that the mere fact that cultural economics has made some progress (even if modest) is to be celebrated since “neither analytical nor empirical progress is assured in a subject like economics.” This possibility of “backsliding” or failing to make progress in generating answers to real questions despite what might seem like at least technical advancement is a theme addressed in Section 3 in the context of what is named the “backlash” stage of intellectual development of an academic field.

In contrast to these previous surveys, a more circuitous approach is taken here to assessing the current state of this field, one that requires two (and part of a third) initial detours before returning to the core issues. First, a brief history of industrial organization and antitrust economics (section 2) provides the context for introducing four “stages” of intellectual development in section 3 that will later be applied to the history of cultural economics in section 5. Then, the sub-field of sports economics, which is in some respects newer and less visible than cultural economics, is reviewed in section 4 for other possible lessons that are suggested in that section as relevant to evaluating developments in cultural economics². Following the consideration of how the four intellectual development stages might apply to cultural

² As noted in Seaman (2003) in a comparative analysis of artist versus athlete labor markets, the Sage Press *Journal of Sports Economics* dates from 2000 in contrast to the *Journal of Cultural Economics*, first published in 1977 under the auspices of the relatively informal 1973 founded Association for Cultural Economics (emerging as a Kluwer publication in 1994 as the official publication of the newly constituted Association for Cultural Economics, International (ACEI), prior to becoming a Springer publication in 2005). There are two sports economics associations younger than the ACEI: the recently formed North American Association of Sports Economists (NAASE), since 2007 formally affiliated with the *Journal of Sports Economics*, and the International Association of Sports Economists, IASE (founded in 1999, and collaborating with, but not having an official relationship with the *Journal of Sports Economics*). The media, performing and visual arts share a similar listing in the *Journal of Economic Literature* (JEL) classification system along with sports (the arts are part of entertainment under the L82 sub-listing of L8 Industry Studies: Services, while sports are L83 as part of “sports, gambling, recreation and tourism”). However, cultural economics managed to score its own classification as Z1, with Z11 being the especially relevant “economics of the arts and literature.” Hence, while important papers were published in sports economics as early as 1956 (see section 4 below), a recognizable field in sports economics is less mature and formalized than that of cultural economics.

economics in section 5, a summary with conclusions is provided in section 6. Cultural economists are blessed with fairly recent very substantive surveys of specific sub-fields, both highly technical (Ginsburgh and Throsby, 2006), and more comprehensive and accessible (Towse, 2003). Clearly the purpose of this reflection on the state of the art of the discipline is to address the much broader patterns of development over the roughly past forty years.

2. INDUSTRIAL ORGANIZATION/ANTITRUST ECONOMICS: RELEVANT HISTORY

One notable example with potential lessons for how to evaluate the field of cultural economics is the sub-field of antitrust economics (especially as practiced in the United States) within the broader field of industrial organization. For those working in this area it is universally recognized that the dramatic increase since the late 1950's in the role played by economic analysis in the development of American (and to some extent European) antitrust policy mirrored the evolution of industrial organization through three identifiable phases. For example, each of the five editions of the popular Kwoka and White (fifth edition, 2009) "antitrust revolution" casebook provides a brief introductory review of the relevant history of industrial organization (pp. 1-5).

Borrowing from but extending their discussion, this history includes three analytical "waves." The first was a largely descriptive/empirical school linked initially to Harvard based researchers (sometimes called pre-Chicago) who developed the paradigmatic structure, conduct, performance framework. This approach identified common entry barriers and numerous firm strategies capable of creating and extending economically damaging monopoly power (e.g., predatory pricing and preemptive capacity expansion, some forms of price discrimination, vertical strategies linked to tying contracts, vertical pricing restrictions, exclusive dealing and exclusive territorial contracts), hence generating a theoretical and empirical case against high concentration within an industry and justifying a relatively activist antitrust policy³.

The Chicago school approach represented a second wave that rigorously applied basic microeconomic theory to firm and industry strategies so as to "more accurately" evaluate those relatively "limited" behaviors likely to result in anti-competitive harm in contrast to benign or even efficiency enhancing pro-competitive consequences. Chicago orthodoxy was firmly critical of horizontal collusion and capable of opposing some horizontal mergers, but generated a largely theoretical presumption that most dominant firm strategies, especially ver-

³ The structure, conduct, and performance paradigm is perhaps linked most closely to books by Mason (1957) and Bain (1959), although Mason had published influential papers as early as 1939 and especially 1949, and Bain as early as 1951. Of course, an extensive later literature was built upon this framework.

tical ones, were either irrational for profit maximizing firms to pursue (e.g., predation) or would only be rational if they also led to higher outputs, lower costs, and/or improved product/service quality (e.g., tying, bundling, and most vertical contracts). This approach thus justified a more carefully targeted and less aggressive antitrust policy.

What then followed was a less coherent but increasingly influential (so far, more academically than judicially) post-Chicago “third wave” research agenda that extended Chicago theorizing into more dynamic, strategic and game-theoretic directions that often identifies conditions under which market failures are indeed as plausible as the pre-Chicago approach suggested, but using more sophisticated and arguably “better” theoretical tools than those used by either of its predecessors. This tradition has in part revived some anticompetitive concerns about predatory behaviors and vertical strategies as being potentially exclusionary, and has cautioned against excessive relaxations of antitrust enforcement and also warned against the dangers of increasingly complex network economies.

3. STAGES OF ACADEMIC FIELD DEVELOPMENT: A POSSIBLE SCHEMA

This historical pattern within industrial organization suggests a possibly more fundamental classification of stages of intellectual development that might be usefully applied to many fields, including cultural economics. These stages are:

- (1) **Foundation** stage: A description and clarification of the essential empirical realities combined with the development of a limited but compelling initial theoretical framework.
- (2) **Maturation** stage: Following the success of the founders in identifying important problems worthy of serious study, a dramatic expansion of research into such problems occurs possibly combined with criticisms of the inadequacy of the original theoretical framework and concerted efforts to prove that the rigorous analytical tools used elsewhere in related fields can bear fruit when applied to these newer issues and policy challenges.
- (3) **Reevaluation** stage: As the maturation phase evolves into a conventional wisdom regarding both the appropriate analytical tools and core policy prescriptions, a competing research agenda develops that questions the adequacy of the theory, the generality of the empirical findings, and the wisdom of the policy prescriptions.
- (4) **Backlash** stage: There is likely to also be a fourth stage during which defenders of the maturation (or even the foundation) research agenda and conclusions wage a counter-offensive against what they view as the brashness and arrogance of the reevaluation proponents. This “backlash” stage focuses in part on questioning whether the seemingly greater sophistication

in the theoretical and empirical tools is primarily cosmetic, i.e., an advance in form rather than substance, with little or no progress in actually understanding the real world phenomena being investigated.

Of course, being inspired by the post-1950 developments in industrial organization, the first three stages match up well to the pre-Chicago, Chicago and post-Chicago research agendas. Thus, in the pre-Chicago **foundation** stage, significant efforts were made to add critical descriptive detail to basic textbook concepts of firms and industries, with a focus on measuring concentration and identifying entry barriers as part of defining and measuring “structure.” Identifying the dizzying variety of actual firm pricing, marketing, and contractual practices was important to describe the real-world conduct of firms instead of resorting to the polar cases of price-taking firms (for which the only strategy was choosing the profit maximizing output) and pure monopolies (with the obligatory discussion of basic forms of price discrimination). This accumulation of “facts” to describe structure and conduct was then extended beyond mere classification to become a “structuralist” theory by positing that (bad) structure caused (bad) conduct and (bad) conduct caused (bad) performance (with the performance criteria being closely tied to the neoclassical welfare theorems of pure competition).

In large part it was this structuralist perspective (especially linked to static concentration measures) that motivated the Chicago revolt and generated a **maturat**ion of industrial organization from a largely descriptive discussion of various concentration ratios and studies of the empirical relationship between concentration and industry profits, into an applied area of core microeconomic theory, where the basic premises of the founders were subject to the rigors of theoretical scrutiny, a search for prior logical inconsistencies, and a critical reinterpretation of the econometric evidence. This led to a period of ascendancy of “Chicago IO” where it became almost embarrassing to believe that predatory pricing ever occurred, that tying contracts could be used to “leverage” market power, or that merely high static concentration ratios (or Herfindahl-Hirschman indices) could ever portend anticompetitive danger.

Two forces then combined to inspire the **reevaluation** “revolt.” As microeconomics continued to become ever more formalized, including the rise and dramatic popularity of game theory that further motivated more creative dynamic strategic modeling, “post-Chicagoans” became convinced that Chicago theory, welcomed as it was in making industrial organization a fascinating application of microeconomic theory, was just too static and too simplified to address the complex behaviors exhibited by the private sector⁴. Furthermore, there was increasing concern

⁴ The concept of contestable markets (introduced by such “non-Chicago” economists as Baumol, Panzer, and Willig (1982) with Baumol appearing here in a decidedly different role than the one he plays in cultural economics) clarified the formal (and quite limited) conditions under which even the threat of entry, in contrast to actual entry, was sufficient to generate the purely competitive result in even the most concentrated of markets. This was a counter theoretical development that in some ways went even beyond Chicago optimism about the limited conditions under which monopoly po-

that Chicago conventional wisdom had become a form of “religious orthodoxy” —because Chicagoans had proven that under some limited conditions “nothing could go wrong,” the allegedly false conclusion had been reached that under all conditions, nothing could go wrong. It was no longer necessary to examine any case specific “facts” if we had already proven that, e.g., predatory pricing was always irrational. Hence the reevaluation period witnessed the quest for more complex models to find the “exceptions” to the Chicago orthodoxy.

An especially fascinating example of the **backlash** phase applies to the industrial organization case, and is reminiscent of Blaug’s (2001) warning about progress not being assured in the field of economics. In the wake of the publication of the first two volumes of the *Handbook of Industrial Organization* (edited by Schmalensee and Willig, 1989), Peltzman (an esteemed representative of the Chicago school) wrote a scathing review (1991) of the generally highly technical contributions in the *Handbook* (which while not uniformly taking policy positions consistent with the emerging post-Chicago agenda, were certainly in that methodological tradition). He asks, “how is one to evaluate the *Handbook* or, more specifically, what the *Handbook* represents: a retreat from policy analysis, a deemphasis of empirical work, and a focus on formal theory?” (p. 204). He is particularly harsh about what he calls the “fascination” with game theory, which he predicts will “mark something of a peak in the field’s attraction to this form of theory” (p. 204), with the clear implication that its popularity will inevitably decline thereafter. After later identifying multiple crucial questions that the game theoretic models would have to answer if they were to be useful to understanding the real world, he wryly observes that “a particular game-theoretic model will not have to answer all of these questions, but it will typically have to answer a nontrivial subset” (p. 207), suggesting of course that such models regularly fail to do even that.

The degree to which cultural economics has exhibited these developmental stages of foundation, maturation, reevaluation and backlash is addressed in Section 5.

4. SPORTS ECONOMICS: DEVELOPMENTAL HISTORY AND SOME CULTURAL ANALOGS

Richard Caves ruefully observed in the Preface to his influential *Creative Industries* (2000) that he had been interested in writing about the arts for about two decades, but “thought it best postponed to a time when my reputation for professional seriousness could more comfortably be placed at risk” (p. vii). Economists with an interest in sports also recognize that risk, and in addition to the potentially important substantive similarities in these two relatively young fields, their status as being on the “fringe” has been an intrusive career reality. Are there notable similari-

power could be artificially generated. It is a “modern” theoretical development that plays almost no role in post-Chicago thinking.

ties in the way such “exotic” fields develop? This section highlights the relevant history and suggests some comparisons and contrasts with cultural economics.

Economic analysis applied to professional or amateur sports was hard to find and very sporadic and fragmented prior to about 1980. This is surprising inasmuch as the academic publication widely recognized as the intellectual foundation for the relatively young field of sports economics was published in a very prestigious economics journal before 1960. Rottenberg (1956) is universally credited with developing two of the most fundamental of the sports economics principles (and several others), and 2006 witnessed international efforts to celebrate the 50th anniversary of this achievement (including a collection of essays published by the University of Oviedo; Rodriguez et al., 2006).

The most important is the “invariance proposition,” i.e. whether athletes were free to accept contract offers from competing teams/clubs (what has become known as “free agency”) or were essentially the “indentured” property of the club that trained them and first signed them to a contract (the famous baseball “reserve clause”), the resulting allocation of players among competing teams will be the same, even though the burden of paying for training and the eventual distribution of rents from superior talent will be different⁵. The second fundamental Rottenberg principle is the “uncertainty of outcome hypothesis,” asserting that any sports league cannot be successful unless the competitive difference among the clubs is modest enough so as to ensure that no team or small group of teams wins consistently. The importance of this principle has been widely enshrined in the somewhat “socialistic” operation of most professional sports leagues, where cross-subsidization, revenue sharing, rewarding poor clubs with top “draft” choices of new players for the following season, and other active efforts to ensure “competitive balance” are common (although not identical between North American and other international settings, and not always believed to have been successful).

Two other early papers were also published in influential places and further explored unique challenges facing sports leagues (Neal, 1964; El-Hodiri and Quirk, 1971). The other significant paper of that era compellingly demonstrated the Becker discrimination proposition (first presented in 1957) that only in non-competitive environments (such as Major League Baseball, with its bizarre exemption from American antitrust laws) can otherwise inefficient worker discrimination survive despite its high opportunity costs (Gwartney and Haworth, 1974).

Cultural economics research had also appeared in very respected general readership journals in the relatively early stages of its development (e.g., prior to or

⁵ Rottenberg made this argument as applied to American baseball in 1956, four years before Ronald Coase (1960) wrote the paper that eventually led to his being identified with the “Coase theorem” proposition that as long as transactions cost are very low (ideally zero), the allocation of resources (although not the distribution of wealth) will be invariant regardless of the assignment of property rights. Nevertheless, many contemporary sports economists bemoan the reality that the Rottenberg invariance principle has largely become a footnote to the Coase theorem even in much of the sports economics literature (see Fort, 2005 for a rousing defense of Rottenberg).

contemporary with the 1977 first volume of the pre-Kluwer/Springer *Journal of Cultural Economics*). The earliest and arguably most influential was Moore (1966), whose rigorous estimation of the demand for Broadway theater tickets justified the common (but not universal) use of single rather than simultaneous equation systems in arts demand studies, generated price and income elasticity results that have been replicated many times over in different later settings, and remains one of the seminal arts demand studies (see also Seaman, 2006). By contrast, despite focusing on an area that has maintained great interest within the field, i.e., the return to paintings as an investment, neither Anderson (1974), nor Stein (1977) pioneered the kind of core principles that would make them classics in the field⁶. As argued by Ginsburgh, Mei and Moses (2006), it was the combination of the art price boom of the late 1980's coupled with the dramatic Baumol (1986) conclusion that the real long-term rate of return to art was a remarkably low 0.6 percent that motivated the boom in such studies (p. 949)⁷. Another highly visible early publication was the Scitovsky (1972) largely normative case for an expanded arts sector, which became influential within the field largely because it mirrored at least the polemical instincts of most scholars drawn to arts economics, and along with the broad topics of arts audiences, and paintings as an investment, the debate about arts subsidization has been as fundamental to the cultural economics agenda as concerns about competitive balance in leagues has been to sports economics⁸. A final pre-1977 paper worthy of note is the Globerman and Book (1974) study of statistical cost functions in the performing arts, still considered, despite its brevity of four pages, to be one of the best treatments of that subject.

⁶ While the focus in the text is on early articles in highly respected peer reviewed academic economics journals and not books, regarding the topic of rates of return on paintings, some (e.g., Shanahan and Hendon (1979, p. 3) have identified a 1961 book by Richard Rush (*Art as an Investment*) as an early "extensive" work in arts economics. But Rush was an art dealer more focused on giving field advice for the buying and selling of collectibles rather than in providing an objective academic study of art as an investment. Of course, Shanahan and Hendon were merely being comprehensive in their early attempt to find any published work that might be remotely relevant to what they hoped would be a new research agenda. Hence they also cited (1979, p. 3) as one of the 1960's era studies, the U.S. House of Representatives *Hearings on Economic Conditions in the Performing Arts, 1962*.

⁷ This Baumol finding of no higher than a 0.6 percent long run real rate of return remains the lowest of any other study reported by Ashenfelter and Graddy (2006) between 1974 and 2003 (Table 1, p. 920).

⁸ Of course this can generate frustration with an excessive concern with this "old" agenda. Ruth Towse, in her inaugural lecture upon assuming the chaired position in Economics of Creative Industries at Erasmus University, declared "we know enough about subsidies and audiences" as she called for cultural economics to move to new ground (2006, p. 9; she also makes this point in Towse, 2008, p. 246). One of the most highly regarded treatments of the subsidy issue was Netzer (1978), and also Austen-Smith 1980, reprinted 1994) has been called a "minor classic" by Mark Blaug (in his introduction to the *Journal of Cultural Economics* reprint). It is indeed unlikely that much more insight can be obtained regarding the normative issues involved, although continuing efforts to rigorously clarify the arguments empirically are highly valuable (e.g., Brooks, 2004). See also footnote 10 herein.

Given the professional visibility and eventual influence of the early sports economics publications, it is notable that it was a collection of academic papers stemming from a conference funded by the Brookings Institution (Noll, 1974) that most significantly “provided the spark to move sports economics from a subject rarely visited by scholars to a field of research with widespread relevance and popularity” (Rodriguez et al., 2006, p. 330). The December 1971 conference at the Brookings Institution drew a mix of academicians and industry experts (economists, lawyers and professional sports executives). It is interesting that in addition to the descriptive information exchanged by this mix of experts with orientations that mixed economics, sociology and history, the compendium of papers that was published was relatively technical and academically oriented, and despite the limitations this imposed on its audience, the conference and the resulting book “had a significant impact on the operation of professional sports leagues” (Smith, 1976, p. 325).

It was also a foundation (Twentieth Century Fund) that supported the research for the book that became the catalyst for the field of cultural economics, but of course Baumol and Bowen (1966) was not an edited collection of highly theoretical papers, but instead a largely empirical tour de force combined with arguably the only unique theoretical contribution of arts economics to the general economics discipline (see the discussion of Hutter’s 1994 characterization of the cost disease above in section 1). Regarding the visibility of this contribution to the broader field of economics, it is sometimes forgotten that the unbalanced growth/cost disease component of the Baumol and Bowen book also found its way into the top economics journal (*American Economic Review*) via two relatively short papers (Baumol and Bowen, 1965; Baumol and Baumol, 1967). Of course, reflective of the stages of development of a field, the robustness of the Baumol cost disease has certainly not gone unquestioned even among (and perhaps most vigorously by) cultural economists themselves⁹, and an entire special issue of the *Journal of Cultural Economics* was devoted to its evaluation (1996, 20(3)), as well as a volume of papers edited by Towse (1997).

The format of collected papers in edited volumes became quite prevalent as a way to disseminate sports economics research up through the 1990’s (prior to the founding of the *Journal of Sports Economics* in 2000). Two notable examples are the 1990 volume, entertainingly named *Sportometrics* (Goff and Tollison, 1990), and *Sports, Jobs & Taxes* (Noll and Zimbalist, 1997). Some useful papers in cultural economics also have appeared in edited volumes (some of them relatively hard to find prior to being “rescued” for reprinting in later edited volumes). Blaug (1976) edited the earliest of these serious collections of arts economics papers, and the first compendium linked to the Association for Cultural Economics (one of five sponsors) was from its inaugural international conference (Hendon et al., 1980), and for some years thereafter, the papers (of widely varying quality) presented at

⁹ An especially creative critique was provided by Cowan and Grier (1996).

the biennial conferences of the original ACE were assembled in book form and became an important part of the cited literature¹⁰.

While this practice generated a considerable number of books, none of these were appropriate as textbooks, a problem identified in the sports case by Leeds and von Allmen when they initially began working in the late 1990's on the first edition (2002) of their successful text (now in its third edition), *The Economics of Sports* (2008, p. xiii). Another successful sports textbook in its second edition is Fort (2006). Both of these are oriented toward North American audiences, a problem that has also plagued the textbook deficient area of cultural economics, where the sole book designed to serve as a text has been Heilbrun and Gray (2nd edition 2001), which despite its many strengths is limited to the performing arts, fine arts, and museums, and similar to the two sports texts above, is very North America oriented¹¹. Other non-edited books with path breaking cultural economics content (e.g., Moore, 1968; Throsby and Withers, 1979; and Caves, 2000) have served well as reference books and foundations for further research, but are problematic as textbooks.

This North American vs. rest of the world (largely European) subplot has plagued both fields. In sports, this seems to be largely an institutional conflict, where the primary weakness of the Leeds and von Allmen, and the Fort textbooks is the limited (but growing in later editions, especially with Leeds and von Allmen) attention paid to any non-North American sports. The void was partly filled by Dobson and Goddard (2001) with their *The Economics of Football*, but that largely substituted England (and to some extent the other European football powerhouses)

¹⁰ While not sponsored by the ACE, ACE officers (Shanahan and Hendon, 1979) were the organizers of a special 1979 issue of the *Journal of Behavioral Economics* (later renamed the *Journal of Socio-Economics*) devoted to "A Symposium on Subsidization of Cultural Activities," consisting of five papers (the last of which also generated a comment and further comment dealing with how to best model local decisions to subsidize museums (Seaman 1979, with comment by Roger A McCain). Another early collection of papers was devoted to "The Arts and Urban Development: Critical Comment and Discussion" (Hendon, 1980). As with many of the early papers and proceedings volumes from ACE conferences, this was published by the University of Akron, home base for William Hendon and the early *Journal of Cultural Economics*. A collection of nine papers including one focused on the United Kingdom, this invited volume stemmed from James Shanahan's work as a visiting scholar in urban affairs at the Charles F. Kettering Foundation, indicating again the importance of early support by foundations for the emerging field of cultural economics, with the Ford Foundation being especially vital to the generation of early arts sector data in the United States (see Seaman, 2006). And despite very limited budgets, the National Endowment for the Arts (NEA) was vital from the 1960's in funding research and generating data in the United States, some of which has continued to be useful to non-American researchers as well. Of course, other governments have funded critically important databases, including e.g., the Spanish Ministry of Culture "Survey of Cultural Facilities, Practices and Consumption of Spaniards," and the Ministry of Culture in France, which has produced large and comprehensive surveys important for improving demand studies in the arts.

¹¹ With eight years having passed since the appearance of the 2nd edition, it is also becoming seriously outdated, a problem worsened by the 2008 death of James Heilbrun. The status of a tentatively planned third edition is unclear.

for North America. Almost no attention is paid even to football in South America (Brazil is referenced one time; Argentina not at all), with Australian Rules football getting two references, rugby five (primarily rugby league rather than rugby union), and cricket two. This book, which is actually better designed as a technical reference source than a textbook, has not survived into a second edition. What has survived, however, is a more European strain of sports economics research that arguably identifies yet another early scholar as its founder, i.e., Peter Sloane. His two papers on European (largely UK based) football (1969, 1971) rigorously address unique issues in the labor market for this sport, as well as the modeling of the behavior of a football club. This work is in the same strong microeconomic theory tradition as were the Rottenberg (1956), Neal (1964) and other previously cited early pioneering work in sports economics, but focused on the unique features of a different sport.

The cultural economics version of a new textbook entrant designed to address, at least in part, regionally based deficiencies is the forthcoming (end of 2009), *A Textbook of Cultural Economics*, by Ruth Towse. This book has a broader scope of topics than does Heilbrun and Gray (2001), and is clearly more European and “rest of world” friendly. In this sense it shares the sports precedent of trying to correct a weakness in the breadth of the topic coverage that has to some extent divided North American and largely European audiences. In fact, there is another interesting and more methodological feature that has characterized some of the primarily continental European research in cultural economics (although not necessarily relevant to the new Towse text, although Towse is indeed especially alert to methodological distinctions). Cowen (1998) argues that there have been three broad approaches to cultural economics (using his terminology): mainstream neoclassicism; loose neoclassicism (emphasizing unorthodox features of the cultural sector, and hence requiring or at least tolerating more “imagination”); and “explicitly non-mainstream and often institutionalist” research that tends to be more “descriptive and empirical” without focusing on theoretical causal connections (p. 69). It is this third approach that Cowen associates uniquely with at least a certain strain of European cultural economics research.

To summarize, the development of sports economics was characterized by an array of quality papers published in top economics journals scattered across the early years from 1956 to 1974. Despite the fact that the most influential of these pioneered some of the most paradigmatic concepts in the field as early as 1956 (Rottenberg), little maturation of sports economics occurred until much later, although a notable increase in activity has been linked to sponsored work resulting in an edited volume in 1974 (Noll, Brookings Institution). This increasing volume of research was still fragmented and often appeared in edited volumes, until the late 1990’s and early 2000’s when academic associations of sports economists were founded, first edition textbooks (or potential textbooks) appeared (2001, 2002 and 2003), and a refereed journal devoted to sports research was created in 2000 (see also footnote 2 above), leading to considerable consolidation of the sports econom-

ics research agenda (although in many ways still focused on better understanding and extending the main issues addressed as early as 1956). Some distinctions have existed between research focused on North American team sports, in contrast to a wider variety of sports (including individual) and more international team sports, although the methodological focus of both North American and European (and other region) research has largely been on the same rigorous application of mainstream neoclassical economics to the often uniquely rich data that are generated from the sports sector (see Seaman, 2003 for further background and a special focus on labor issues).

Some initial parallels in this history were drawn to cultural economics: (1) the early appearance of research published in respected general readership journals well before any recognition existed of either sports or cultural/arts economics as a field of its own, although the sports papers cited above from 1956 to 1974 had a more long term influence than did the early-mid 1970's arts papers focused on rates of return to paintings; (2) the pivotal role played by foundation support (Brookings in sports; Twentieth Century Fund in the arts) in leading to a book (an edited collection of papers in sports, a jointly authored study in the arts) that kick-started the field; (3) the prevalence of many early papers appearing in edited volumes (not widely distributed) including from academic conferences, (4) the dearth of usable textbooks, with major issues including the adequacy of the scope of topics defined as relevant to the cultural sector, along with (5) the excess weighting of North American institutional arrangements, reflecting a certain tension in the perspectives of North American vs. rest of the world researchers, which includes not just differences in the institutional questions addressed, but at least for some, in the dominant methodological approach taken.

5. THE FOUR STAGES OF FIELD DEVELOPMENT APPLIED TO CULTURAL ECONOMICS

The four stages identified and initially described in section 3 were linked most closely to the intellectual development of industrial organization and antitrust policy. The most simplified summary of these four stages (call it version S for "simplified") might focus on the role of theory, including any applicable statistical or econometric theory, in investigating the relevant problems and empirical realities: (1) foundation stage as pre-theory/simple theory; (2) maturation stage as the application of basic core theory; (3) reevaluation stage as more sophisticated theory; and (4) backlash stage as critique of flawed or nonproductive theory. A further clarification of these stages builds upon the section 3 descriptions (call it version D for "detailed"):

1. Foundation stage as definitional, categorization, initial measurement and search for core paradigms and theoretical foundations, with initial applications of preliminary approaches.

2. Maturation stage as the victory of a dominant methodological approach followed by the extensive application of this approach to an ever expanding array of research problems within the scope of the definitions established in the foundation stage and further clarified while implementing the maturing research agenda.
3. Reevaluation stage as the discomfiting sense that the full potential of the field is not being realized by the decisions made in the maturation stage, with a focus on identifying the limitations of the research scope, current theoretical and empirical methods being used, and strength of the results obtained by the mature research agenda. While a search for increasingly more sophisticated techniques can characterize this stage, it can also include a disconcerting sense that some of the initial foundation stage insights that had been de-emphasized should be revived.
4. Backlash stage as pushback by those unconvinced that the maturation stage agenda was seriously deficient or that the improvements of the reevaluation stage are as valuable as alleged. This stage can also include a more concerted effort to act upon the growing recognition that a more sophisticated version of some foundation stage approaches may be especially fruitful.

A Suggested Description of the Four Stages of Cultural Economics

Foundation Stage

Few fields began with a more serious problem of data availability and data quality. Hence it was no surprise that even by 1994 (nearly thirty years after the appearance of Baumol and Bowen (1966), itself even more valuable for its generation of “hard data” than for its introduction of Baumol’s cost disease), Throsby bemoaned the “serious constraint imposed on research in cultural economics by the lack of comprehensive statistics on the arts industry and its sub- sectors” (1994, p. 26).

To deepen the challenge, no one was quite sure what cultural economics should be. That this question is still open may not be as serious as it was in the beginning, since clarity of purpose will naturally be a bigger challenge to a new enterprise than to an ongoing and at least modestly successful one¹². But it has clearly been a

¹² This ongoing scope problem no doubt confused more than few cultural economists who eagerly scanned the papers in the *Journal of Economic Perspectives* symposium on “Cultural Economics” (20, 2, Spring 2006). Instead of papers focused on the usual topics, they found an analysis of why China has been unable to take as much advantage of modern scientific developments as has Europe and the West; two papers exploring how religion and cultural belief systems may affect economic outcomes; and the role of family in the outcome of family firms. This “big C” vs. “little c” problem is of course well appreciated by “arts economists,” and Throsby has been especially vital in ensuring that we do not forget the broader context, addressing issues of cultural capital and sustainability (see, e.g., Throsby, 1995). Also, current plans to produce a second volume of the *Handbook of the Economics of Art and Culture* include a clear focus on these broader cultural issues, including relig-

distraction spending time clarifying why a particular definition of culture, or even of the arts, has been chosen (e.g., both Throsby and Withers, 1979, and Heilbrun and Gray, 2001 and also their first volume, exert more than trivial effort to do this, with both arriving at rather narrow definitions for the purposes of their books). Possible limitations of such narrow definitions were recognized very early, with Hendon et al. (1980, p. ix) tempering their applause for the influence of Baumol and Bowen (1966) by also noting “they are to be blamed for limiting research attention primarily to performing arts and to arts institutions rather than to cultural processes and artistic endeavors.”

The inherently interdisciplinary nature of this emerging field was both an opportunity and the source of some notable confusion. Ginsburgh (2001) identified six overlapping disciplines that made definition a challenge (art history, art philosophy, sociology, law, management and economics), and Seaman (2006, p. 418) noted the remarkable confluence of fields from which serious studies of the demand for the arts (just one part of the field) have arisen (economics, sociology, arts policy, psychology, and marketing). Because some economists doing cultural research are more familiar with and enthusiastic about these interdisciplinary contributions, a certain division with the field has been present from the very beginning, with one ongoing “litmus test” being whether references to the works of Bourdieu elicit appreciative recognition (generally among Europeans) or blind stares (almost always among North American economists, although not of course sociologists)¹³. Mossetto (1992) was another notable early proponent of taking advantage of the potential of other related disciplines and approaches, regularly combining more traditional economics tools with efforts to incorporate principles of aesthetics and traditional institutionalism into his analysis.

The result of this cacophony of voices and perspectives was an unsteady consensus on some key issues: (1) the field would primarily focus on the live performing arts, fine visual arts, and to some extent museums; (2) standard neoclassical economics would be a useful, but not the only methodological tool used to make progress in the field¹⁴; (3) a critical early priority would be data gathering and description, along with more simple statistical analysis (e.g., Baumol and Bowen, 1966), if needed, until the quality of the data caught up with the available econo-

ion, language, and a broader definition of culture to possibly include sports and more media issues (also see Throsby, 2006, p. 21).

¹³ There are many examples of European cultural economists invoking Bourdieu, one being López Sintas and García Álvarez, 2002).

¹⁴ It is fascinating that despite the skepticism about the capability of economics to adequately address the complex issue of cultural goods and services often expressed by some key founders of the original ACE and a core group of early arts economists, the very first issue of the *Journal of Cultural Economics* (1977) contained an extremely creative paper by Globerman and Book (1977) that remains one of the best (and nearly only) attempt to apply the Becker type “consumers as producers” model to the issue of how education might reduce the shadow price of consuming culture rather than just be a “taste shift” variable. It also included a quite interesting and very neoclassical exchange (continued actually in a later issue) regarding the application of the monopsony model to the Florentine and Siense Renaissance (Owen, and McCain, 1977).

metric tools; (4) Baumol's cost disease (Baumol and Bowen, 1965; 1966; Baumol H. and Baumol W.J., 1967) would serve as an accepted core theoretical contribution and credible justification, in addition to others linked to externality arguments, for supporting government subsidies to the arts as a way to ensure their survival in a hostile world (Scitovsky, 1972).

While this stage in cultural economics is not adequately described in the "pre-theory/simple theory" terms of the simple version of the stages theory (version S above), it does exhibit many features of the more detailed version of that schema (version D).

Maturation Stage

Despite the few, but notable, pre-1977 thoroughly standard neoclassical contributions published in other respected economics journals and books identified in Section 4 above, and the obvious neoclassical credentials of "co-founder" W.J. Baumol (even if the founding 1966 book was of necessity focused on defining and describing the data relevant to the performing arts problem and only in part developing a theory to explain it), the methodological direction of the emerging field was still in some doubt as it entered the maturation phase. After all, Troub (1980, p. 13) had appended a question mark to the section "Economics and the Arts: Complementary Research Programs?" of his review of conventional, institutional and neoinstitutional possible approaches. Seaman (1981), after surveying the cultural economics scene in the wake of the 1st International Conference in Edinburgh (and noting that some prominent participants clearly found the application of standard economic analysis to the arts to be strained and lacking an appreciation for the complexities involved) suspected that many in the young field "would favor a bit more art and a bit less economics in the economics of the arts" (p. 36). He then attempted neoclassical and institutionalist reconciliation en route to defending the potential benefits of creatively applying core economic methods.

Werner Pommerehne (1992) expressed considerable relief in his preface to a collection of selected papers from the 6th International Conference on cultural economics in Sweden, noting "a new development in cultural economics as it manifests a 'deepening' of the analysis of cultural pursuits" (p. vii). He goes on to applaud what he considers the various "outstanding example[s] of a serious attempt to suitably apply economic theory —instead of presupposing that culture and the arts are too exceptional to be subjected to an economic analysis." According to the highly respected Pommerehne, the maturation of the discipline was in gear, and that maturation required what his frequent co-author and similarly respected Bruno Frey (1994, p. v) would say about him upon his premature death: "He combined a forceful application of economics with a strong empirical orientation" (citing his ingenious use of sophisticated econometric techniques).

This maturation of the field then saw the continuing expansion of the application of primarily standard economics (largely microeconomics) and basic econometric tools to nearly all the many sub-fields, with the institutional changes of a

new publisher (Kluwer) and the formalization of the ACEI reflecting the expanding reach of the Journal of Cultural Economics (JCE) and the further development of “recognizable” and respected economics being applied to the issues of the cultural sector. In terms of applying core economic theory and making the JCE readable only by economists as opposed to its prior more mixed audience crossing many fields and including “practitioners” (also reflected in the composition of attendees and the nature of papers presented at international conferences), this pattern is very similar to the maturation stage (simple or detailed versions) described above.

Reevaluation Stage

Revaluations necessarily exhibit some dissatisfaction with the *status quo*, and the reevaluation stage in cultural economics focused on three major issues:

- (1) The inadequacy of some of the theory and the econometric techniques that had become standard in much of the literature.
- (2) The recognition that the boundaries of the discipline that had been established in the foundation stage and had served as the primary agenda for the application of the standard neoclassical economic models were too confining and were greatly limiting the influence of the field.
- (3) Rebellion against some of the core policy prescriptions that had been established in the foundation stage, and further developed by research in the maturation stage.

The focus naturally shifts to providing some examples of each of these three re-evaluations.

Inadequacy of Method

Cultural economists had long focused on investigating valuation questions. The profound issues of value is at the center of all of economics and also what had long been recognized as especially challenging about the arts and culture. Regarding fundamental value questions, the recent collection of papers in Hutter and Throsby (2009) attempts to improve on the contributions in the maturation stage by directly addressing what they call the “tension between economic and cultural modes of evaluation.” To some extent the dissatisfaction with our answers to the valuation question also reflected ongoing concerns that the tools of standard economics were indeed too limited to properly address the complexities of the arts and culture, a major concern during the foundation stage. This had long been a theme of Klammer (1996), and Frey (e.g., 2000), who warned of special dangers in the application of economics to the arts, and observed, “it may be fruitful to transcend the rather rigid limits of orthodox neo-classics” (p. 6). An especially well reasoned analysis of the current methodological challenges is provided by Rushton (1999).

A more pedestrian but policy rich second dimension of valuation issues was how to measure the value of, e.g., a symphony orchestra to a local region, a question that had long been addressed with the use of economic impact methods. Ongoing dissatisfaction among cultural (and regional) economists with the conceptual limitations, practical implementation problems, and misinterpretation of results stemming from the economic impact approach led to an explosion of research adapted from environmental economics applying the contingent valuation approach (as noted above, this was the focus of a combined two issues of the *JCE* in 2003). This is an example of seeking a more sophisticated theory as a substitute for an inadequate theory. Snowball (2008) provides an excellent analysis of the strengths and weaknesses of the various types of regional valuation models.

Another example of the search for more sophisticated models and techniques applies to one of the core research agendas in the field—an analysis of arts audiences and the demand for the performing arts. As reviewed in Seaman (2003), there has been a significant increase in the sophistication of the econometric methods used in recent years, what might here be called a shift from the maturation into the reevaluation stage as the limitations of the standard techniques of both theoretically modeling and empirically estimating demand have been recognized.

A final example is the considerable rethinking of the strengths and weaknesses of the “core” model in arts economics: the cost-disease model. Whether reassessed using the challenging arguments of Cowen and Grier (1996), or the many less bold but insightful papers published in Towse (1997) or in the *JCE* special issue on this subject (1996, 20(3)), it is clear that even iconic models stemming from the foundation stage and nurtured in the maturation stage have not been immune from rethinking in the reevaluation stage of cultural economics.

Inadequacy of Scope

While many examples might be cited (including the already discussed recognition by Ginsburgh and Throsby that a second volume of the *Handbook* may be needed to address the many areas of culture left out of the first volume), one can do no better than cite the Ruth Towse lament (2008) about the economics of copyright. That is, despite the dramatic importance of digitalization and the Internet that have caused upheavals in the way the arts (and most products) are produced and consumed, cultural economics has been deficient in rising to this challenge, at least as it applies to the lack of focus on the economics of copyright. The always-insightful observations of Tyler Cowen supplement this theme (2008) as he laments his own inability (and by extension all of the rest of us studying cultural economics) to remotely keep abreast of the implications of iPods, cell phones, Kindle, blogs, computer games and virtual realities. By noting that such developments (ongoing at a rapid pace) have changed cultural economics “an enormous amount in just the last five years: (p. 261), we are reminded that the way we study cultural economics has not changed very much in the past five years. By implication, we are falling behind, and rapidly.

A less dramatic but highly important example of addressing inadequate scope issues was Caves (2000), who recognized a gap in the seemingly mature arts literature regarding our understanding of how the structure of contracts affect observed patterns of arts industry behavior. His applications of the so-called modern theory of the firm focusing on transactions costs, vertical relationships, and possible “hold-up” problems were a significant development during the reevaluation stage.

Inadequacy of Policy Conclusions

The example of this reevaluation will be limited to one of the most dramatic developments among cultural economists: a growing criticism of the role of government in the arts sector. Although a major motivation behind many economists originally becoming involved in arts and cultural research was an advocacy interest for more public and private support for a financially vulnerable arts sector, some solid research by arts economists has generated a backlash against the case for more public support. Of course, there have always been cultural economist critics of an expanded government role in the arts (William Grampp being a persistent skeptic, 1986/87; and Tyler Cowen an articulate current critic). This reaction could indeed be included in the “backlash” stage of development depending on one’s conceptualization, but regardless of stage, this has been an important development.

Two notable contributors to this reevaluation of the case for subsidies are Abbing (2002) and Frey (2000). Abbing, himself also an artist, brings especially valuable perspectives to his claim that government policy has itself often been responsible for why artists are poor. Frey has long worked (in the past with Pommerehne) on applying insights from a kind of public choice perspective to examine how government support actually works, and how it can regularly generate negative consequences (including to creativity) despite its best intentions. The combination of entrenching “incumbents,” increasing entry barriers, and stifling creativity that are emphasized by Abbing and Frey make them perhaps unlikely allies with conservatives in expressing significant reservations about an expanded public role in the arts sector.

Backlash Stage

The primary example of a backlash in industrial organization was Peltzman’s (1991) attack on the shift of industrial organization toward excessively formal, game theory obsessed, and non-empirical/non-policy oriented research. An interesting example from cultural economics is Tabarrok’s (1998) review of Ginsburgh and Menger’s collection of papers (1996), which he calls “badly conceived,” even if at times making “solid” contributions, although “dull” reading (p. 287). His primary complaint is that the papers, while technically solid applications of the “standard toolbox of theory and statistics,” fail to address any of the “interesting and puzzling questions” peculiar to the arts that can inspire the creative attention of economists (p. 285). Since Ginsburgh is an especially competent micro-theorist who has regularly sought to inspire cultural economics to utilize the most ad-

vanced economics research methods and theories, this Tabarrok “backlash” which is seemingly inspired by a skepticism about focusing on research techniques rather than a careful selection of the research question and seeking an answer that may be of broad interest, even if utilizing more modest tools, is in the spirit of the Peltzman critique of the industrial organization reevaluation agenda. Regardless of the merits of the Tabarrok argument, it is a good example of a backlash stage of a field’s development.

A backlash against a quite different type of arts research is provided by Levy (1998), who is not impressed by the Klamer (1997) “reevaluation” stage attack on the maturation agenda from a distinctly non-neoclassical perspective. After noting that Klamer seems to be arguing that “economics cannot explain cultural stuff” (p. 64), Levy identifies what he considers the inadequacies of this critique, closing with the decidedly “backlash” observation that this debate between the “cultural critics” of economics and the economic perspective (which far predates the rise of the field of cultural economics) was lost so badly by the “cultural side” that “its existence is known only to the decaying tribe of historians of economics” (p. 65).

To end these backlash stage examples on a somewhat more “civil” note, I shamelessly cite two examples from my own work. In Seaman (2006), after reviewing the notable rise in more sophisticated econometric techniques, taste cultivation models and larger databases that have characterized recent demand studies in the performing arts, it is noted that “such analytical improvements to date have largely failed to generate substantial changes in the results or new insights about performing arts demand” (p. 466), followed by further specific examples of this failure. This is clearly a backlash stage argument, whereby doubt is expressed about the real contribution of ever more sophisticated research methods in what we call here the reevaluation stage, methods which were themselves devised in reaction to the seeming inadequacies of the research being done in the maturation stage.

The final example is the argument made in Seaman (2004) about the importance of re-discovering the merits of the earlier maturation phase research (e.g., as found in Throsby and Withers, 1979, which can even be identified with the foundation phase) that applied core industrial organization theory to the performing arts. That research explored issues of competitive behavior and firm/organization strategy before it was lost to the presumed paradigm of treating most performing arts organization as natural monopolies. This earlier potentially fruitful line of research was further lost in the enthusiasm surrounding the reevaluation phase rediscovery of the “creative industries” by Caves, and his refocusing any industrial organization research away from competitive horizontal interactions and toward vertical contracting investigations. The point was in no way to discourage the further development of the Caves insights, which are significant and vital to the field of cultural economics, but instead to lament the seeming loss of a useful earlier research agenda. In that sense, it is a definite backlash stage argument, and one that Towse (2007, p. xviii) properly identified with having an absence of “worries

about limitations of neoclassical economics.” At least on this agenda, she is correctly characterizing what Seaman (2004) presents as a backlash stage call for re-discovering a basic application of economics amidst the ongoing rush to further develop new techniques and expand the boundaries of the field.

6. SUMMARY AND CONCLUSIONS

While cultural economics is certainly one of the newer fields of economics, it naturally shares common features with other fields, both well established (health, education, and environmental economics), and similarly young (sports economics). The emphasis in this review of the state of the art of cultural economics has been less on overlapping subject matters than on overlapping stages in its intellectual and methodological development. In that context, it has also been argued that cultural economics shares some common features with the often contentious stages of development experienced by industrial organization and antitrust economics.

Even if one accepts the terminology and descriptions introduced in this argument regarding the specific stages of a field’s development: foundation, maturation, reevaluation and backlash, a quite different application of those stages to the field of cultural economics might be proposed than the one outlined in section 5. But in addition to the other enlightening surveys of how we got we are and what it means for the future of the field, it is hoped that this alternative approach offers some insights. It is also hard to dispute the general proposition that, as with sports economics, cultural economics has gone through a fragmentation and then a consolidation phase, although a very good case could be made that as the boundaries of the field of cultural economics expand once again, further fragmentation of research agendas will occur. However, with the mature institutional structure provided by the Association for Cultural Economics, International, and the increasingly well-established and indexed *Journal of Cultural Economics*, we should not expect the degree of fragmentation in reporting the results of such research that characterized the earliest pre-foundation periods of both sports and cultural economics.

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