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The rise and fall of catastrophe theory applications in economics: Was the baby thrown out with the bathwater?

J. Barkley Rosser Jr.

Department of Economics, MSC 0204 James Madison University, Harrisonburg, VA 22807, USA

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Abstract

This paper discusses the mathematical origins of catastrophe theory, the various applications of it in economics, the controversy over its use, and the criticism of it as a fad, with the subsequent general disappearance of its use in economics. It presents a criticism of the criticism of the most famous application and a discussion of its current relevance and available alternatives. It concludes that indeed the baby was largely thrown out with the bathwater, and that catastrophe theory should be openly and properly used again in economics.

JEL classification: B23; C02

Keywords: Catastrophe theory; Multiple equilibria; Structural stability; Intellectual fads

1. Introduction

The science writer, Horgan (1995, 1997), has ridiculed what he labels ‘chaoplexology,’ a combination of chaos theory and complexity theory. A central charge against this alleged monstrosity is that it, or more precisely its two component parts separately, are (or were) fads, intellectual bubbles of little consequence. They

E-mail address: rosserjb@jmu.edu.

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would soon disappear and deservedly so, once scholars and intellects realized what worthless dross they truly were (or are). As the culminating centerpiece of his argument, Horgan introduced the label, 'the four C's,' which consist of cybernetics, catastrophe theory, chaos theory, and complexity theory.[1] More particularly, Horgan singled out catastrophe theory as the supreme example of an intellectual fad to which he compared chaos and complexity theory. This comparison was supposed to constitute the definitive proof of his argument, its pièce de résistance, the point that would send any right-minded or sensible individual running for their intellectual respectability while screaming in horror at the very idea of taking either chaos theory or complexity theory even remotely seriously. Clearly, Horgan considered catastrophe theory to be such an utterly worthless intellectual stock that the very mention of it in conjunction with another idea would trigger an immediate and relentless crash of the other idea's value in the intellectual bourse once and for all. Such is the current status of catastrophe theory as perceived by many observers of the intellectual scene.

That this is supposedly the status of catastrophe theory more generally means that it is also largely its status in economics as well. It is a discounted idea or approach or method or theory that no ambitious junior scholar would dare to refer to in a paper except either in ridicule or in a remote footnote with little further discussion. Within the last decade hardly any paper in a leading journal in economics has appeared that had any reference to 'catastrophe theory.' Recent papers that apparently use some version it often avoid mentioning that they are doing so (Wagener, 2003), although there seem to be fewer inhibitions in physics (Leahy, 2001; Kuznetsova et al., 2004) and biology (Li et al., 2004). Whether catastrophe theory was a loveable baby or a bucket of worthless bathwater, it has been largely thrown out by economists. The case would seem to be closed, with the widespread nature of its rejection seeming the final proof that it was really just bathwater after all and that Horgan was fully justified to hold it up as the prime example of a ridiculous and worthless intellectual fad.

This paper suggests that this viewpoint needs reconsideration. The reference to 'the baby being thrown out with the bathwater' was first applied to the question of catastrophe theory during a debate over its use by Oliva and Capdeville (1980) in *Behavioral Science*. This suggests that there was some bathwater that needed to be thrown out, but that catastrophe theory itself was not that bathwater, that it was in fact a not totally unloveable baby that deserved to be preserved and raised in a proper household. Sins of intellectual hype and exaggeration were committed as were inappropriate applications of the theory. It is not as widespread in application as its original proponents claimed and is not a general intellectual panacea. There was a fad and an intellectual bubble, and it was perfectly reasonable that there

[1] Rosser (1999) agrees with Horgan that the four C's are linked through their common use of nonlinear dynamical systems. But he argues that this is something to be celebrated and appreciated rather than denigrated or dismissed. Just as the term 'impressionism' in art was originally bestowed by a critic, so the 'chaoplexologists' should accept the label originally provided in derision and wear it in pride. Ironically at the time that catastrophe theory was first criticized in 1977, its main founder, Thom (1969, 1972) worried that it could 'have the same fate as cybernetics.' (Aubin, 2001, p. 274).

should have been a discounting and a downgrading. However, this has been overdone; the intellectual marketplace has inefficiently overshot on the downside. A sign of this is that the field in which the attitude towards catastrophe theory did not fall so low is mathematics, probably because it did not rise so high in the first place. Economists should reevaluate the former fad and move it to a more proper valuation.

2. The emergence of catastrophe theory out of general bifurcation theory

Catastrophe theory studies structurally stable singularities of dynamical systems, an important aspect of bifurcation theory. Bifurcation theory was principally invented/discovered by Poincaré (1880–1890) as part of his qualitative analysis of systems of nonlinear differential equations. This arose from his study of celestial mechanics and the famous three-body problem in particular. Would the orbits of the planets in the solar system escape to infinity, remain within certain bounds, or would the planets crash into each other or the sun? Beyond this question he investigated the structural stability of the system, studying if small perturbations to it would leave it relatively unchanged in its behavior or cause it to move in a very different manner, the central focus of bifurcation theory.

Although Poincaré was the first to formally analyze bifurcation theory, Arnol'd (1992, Appendix) has provided a list of precursors to Poincaré. According to Arnol'd, although an observer can find hints of it in some work of Leonardo da Vinci, the first clear presentation of the structural stability of a cusp point came in the study of light caustics and wave fronts was by Huygens in 1654. Critical points in geometrical optics were studied by Hamilton in 1837–1938, and by the late 19th century algebraic geometers such as Cayley, Kronecker, and Bertini were examining the singularities of curves and smooth surfaces, even in textbooks on algebraic geometry. Nevertheless, it was Poincaré who brought structure to this discussion.

Consider a general family of differential equations whose behavior is determined by a k-dimensional control parameter, μ

$$\mathrm{d}\mathbf{x}/\mathrm{d}t = f_{\mu}(\mathbf{x}); \quad \mathbf{x} \text{ in } R^n, \ \ \mu \text{ in } R^k. \tag{1}$$

Equilibrium solutions are given by $f_{\mu}(\mathbf{x}) = 0$. This set of equilibria will bifurcate into separate branches at a singularity, or a degenerate critical point. More precisely, a singularity occurs where the Jacobian matrix $D\mathbf{x}f_{\mu}(\mathbf{x})$ has zero eigenvalues. Intuitively a single stable curve of equilibrium points may split into several curves at such a point, with some stable and others unstable locally. At such points the first derivative may be zero but the function may not be at an extremum. There are many different kinds of bifurcations, with Guckenheimer and Holmes (1983) and Kuznetsov (1998) summarizing various types, with this analysis also being viewed as the study of non-hyperbolic equilibria of vector fields.

The distinction between critical points of functions that are non-degenerate (associated with extrema) and degenerate ones (singular, non-invertible Hessian) was further studied by Morse (1931) who showed how a function with a degenerate

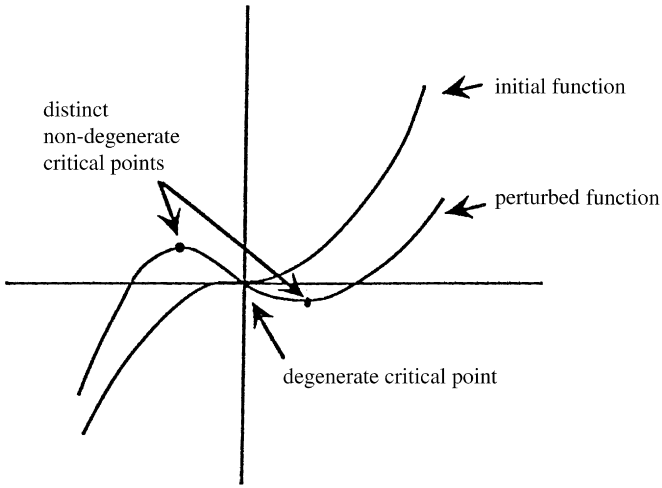

Fig. 1. Bifurcation at a singularity.

singularity could be slightly perturbed to a new function that would now exhibit two distinct non-degenerate critical points instead of the singularity. This was a bifurcation of the degenerate equilibrium and indicates the close connection between the singularity of a mapping and its structural stability (see Fig. 1).

Whitney (1955) followed Morse by discovering/inventing the two singularities associated with the two most commonly studied kinds of elementary catastrophes, the fold and the cusp (see Fig. 2), showing that these were the only two kinds of structurally stable singularities for differentiable mappings between two planar surfaces. Thus Whitney can be viewed as the real founder of catastrophe theory.

Following his discovery of transversality Thom (1956), developed further the classification of singularities, or of elementary catastrophes, although a more complete categorization would eventually be carried out by Arnol'd et al. (1985), who showed that for systems beyond a dimensionality of 11, the categories of catastrophes become infinite and thus difficult to categorize. Thom (1972, pp. 103–108) would label such catastrophes as 'generalized' or 'non-elementary.' More particularly, Thom (1972) studied the seven elementary catastrophes going up through six dimensions in control and state variables. Within the context of dynamical systems the control variables are usually conceived as moving slowly, perhaps exogenously to some degree, while the state variables are viewed as moving more rapidly, adjusting endogenously to changes in the control variables and moving to the equilibrium manifold. This became standard (or 'elementary') catastrophe theory.

Consider a dynamical system given by n functions on r control variables, c_i. The n equations determine n state variables, x_j

$$x_j = f_j(c_1 \dots c_r). \tag{2}$$

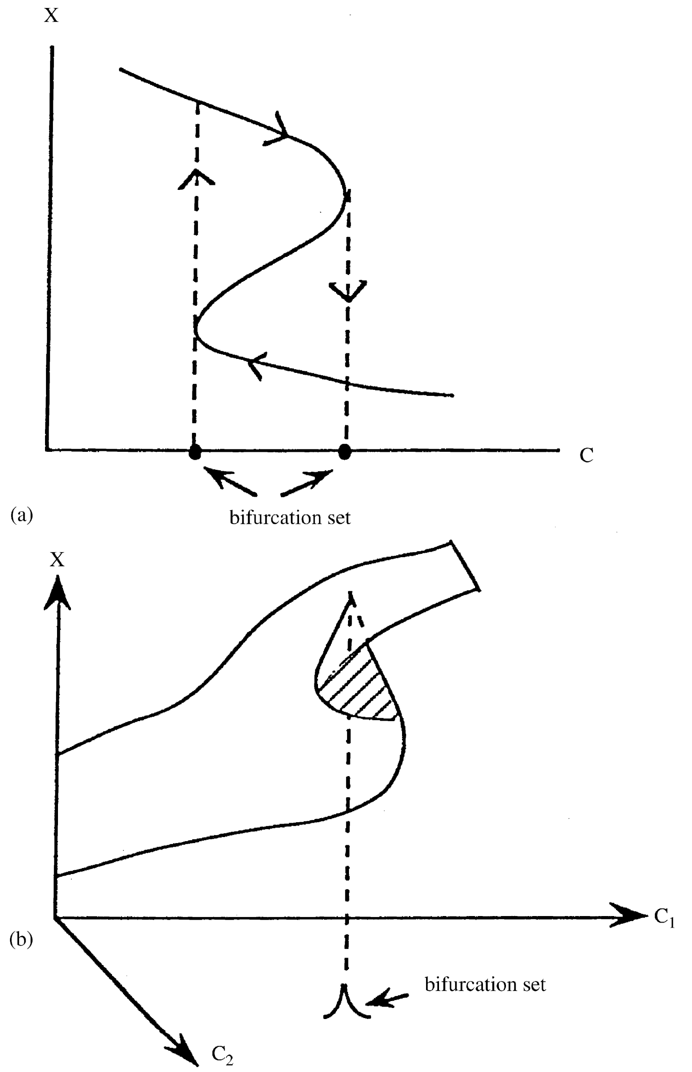

Fig. 2. (a) Fold catastrophe, (b) cusp catastrophe.

Let V be a potential function on the set of control and state variables

$$V = V(c_i, x_j) \tag{3}$$

such that for all x_j

$$\partial V/\partial x_j = 0. \tag{4}$$

Under regularity conditions this set of points constitutes the equilibrium manifold, M, and an example is seen in the cusp catastrophe seen in Fig. 2, which is characterized by two control variables and one state variable. In much discussion the control variables are characterized as being 'slow,' whereas the state variables are characterized as being 'fast.' The usual presumption has been that the state variables adjust quickly to be on the equilibrium manifold while the control variables move the system around on the manifold. The catastrophe function is the projection of the equilibrium manifold into the r-dimensional control variable space, with its singularities the main focus of catastrophe theory.

Thom's Theorem, proven by Malgrange (1966) and Mather (1968), states that if the underlying functions f_j are generic (qualitatively stable under slight perturbations), if $r<6$, and if n is finite with all but two state variables being represented by linear and non-degenerate quadratic terms, then any singularity of a catastrophe function will be structurally stable (generic) under slight perturbations and can be classified into 11 different types. There are seven such types for $r<5$, and Thom (1972) provided colorful names for each of these, along with detailed discussions of their various characteristics, with further discussion carried out by Trotman and Zeeman (1976). For $r>5$ and more than two control parameters, the set of possible catastrophes is infinite, although as noted above Arnol'd et al. (1985) have extended the finite set somewhat.

Overviews of the theory and some applications across disciplines can be found in Poston and Stewart (1978), Woodcock and Davis (1978), Gilmore (1981), Thompson (1982), Arnol'd (1992), and Castrigiano and Hayes (1993). Portions of the mathematical presentation given here as well as the discussion of the controversy about catastrophe theory can be found in Rosser (2000a, Chapter 2).

Although some applications of higher dimensional catastrophes have appeared in urban and regional economics, most applications in economics have involved the two simplest forms known to Whitney in 1955, the fold and the cusp depicted in Fig. 2. In order to analyze a particular model using one of these one must make assumptions regarding how the system moves between equilibria in situations of multiple equilibria, which is a more general problem in bifurcation theory (Wiggins, 1990, p. 384). This is often dealt with via conventions. Under the Maxwell convention a system will immediately jump to a new equilibrium zone, whereas under the delay convention a system will remain in the old equilibrium zone until the last possible point before it vanishes (Gilmore, 1981, Chapter 5). In his original discussion Thom (1972, p. 44) explicitly simplified his analysis by assuming the Maxwell convention. For real systems all kinds of intermediate possibilities abound and must be determined empirically. More generally for the fold catastrophe four kinds of behavior can occur: *hysteresis*, *bimodality*, *inaccessibility*, and *sudden jumps*, with *divergence* also happening for the cusp catastrophe.

The question of Thom's assumption of the Maxwell convention is associated with a deeper problem that we have avoided so far, that the definition of catastrophe theory itself is somewhat fuzzy (although we effectively presented one in the opening sentence of this section). His use of the Maxwell assumption rules out a variety of cases such as the cusp case studied by Whitney originally.[2] Thus the neologizer of the

[2]An anonymous referee provides a summary definition of the Thom version of a metabolic catastrophe point that exhibits this problem. 'Let M and Λ be the manifolds of 'internal parameters' and 'control

term, Zeeman (1974, p. 623) complains that 'there is strictly speaking no 'catastrophe theory,' but then this is more or less true for any non-axiomatic theory in mathematics that attempts to describe nature.'

Disagreements about the definition are relevant to the discussion of the controversy of catastrophe theory made below. One issue is exactly the degree to which time necessarily enters into the definition. Thus, Thom assumes that catastrophe theory is necessarily about dynamical systems, and this is the approach used above. However, it can be argued that the broader singularity theory of Whitney is about mappings more generally that may not involve time at all. Arnol'd (1992, p. 6) especially emphasizes that 'the combination of singularity theory and its applications should be called catastrophe theory,' although he attributes this formulation to Zeeman.[3]

This split between the original Thom formulation and the more generalized Arnol'd formulation also shows up in regard to the question of whether or not the systems must have a potential function, for which there is a necessary symmetry condition that all cross-partial derivatives must be equal. Again, the broader singularity theory does not require this, and forms that resemble the elementary catastrophes can appear within this theory even while not fulfilling the stricter assumption about the existence of a potential function. This would become a central issue in the later controversy over catastrophe theory.

3. Some applications in economics

Critics of catastrophe theory have argued that many applications of it in many fields violated necessary assumptions or were carried out questionably for one reason or another. Let us list a few examples of applications in economics, most of which were done in a reasonable manner.[4]

(*footnote continued*)
parameters.' For every $\lambda \in \Lambda$, there is a vector field X_λ given on M, and an attracting equilibrium $a(\lambda) \in M$ of the vector field X_λ. A metabolic catastrophe point is any value of λ for which $a(\lambda)$, or one of its derivatives, is discontinuous.'

[3] Arnol'd (1992, p. 2): 'Singularity theory is a far-reaching generalization of the study of functions at maximum and minimum points. In Whitney's theory functions are replaced by mappings…Catastrophes are abrupt changes arising as a sudden response to a smooth change in external conditions.' Arnol'd (1992, p. 7): 'Since smooth mappings are found everywhere, their singularities must be everywhere also, and since Whitney's theory gives significant information on singularities of generic mappings, we can try to use this information to study large numbers of diverse phenomena and processes in all areas of science. This simple idea is the whole essence of catastrophe theory.' I thank two referees for bringing to my attention the importance of this issue. I also note the irony that there are competing definitions of chaos theory as well (Rosser, 2000a, Chapter 2).

[4] For more extensive discussion of the urban and regional examples see Rosser (1991, Chapters 9–11), the ecological and environmental examples see Rosser (1991, Chapters 12–14), and for the international finance examples see Rosser (1991, Chapters 15–16). For more extensive discussion of the microeconomic theory examples see Rosser (2000a, Chapter 3), macroeconomic models see Rosser (2000a, Chapter 6), capital theory examples see Rosser (2000a, Chapter 8), and for the financial market examples see (Rosser, 2000a, Chapter 5).

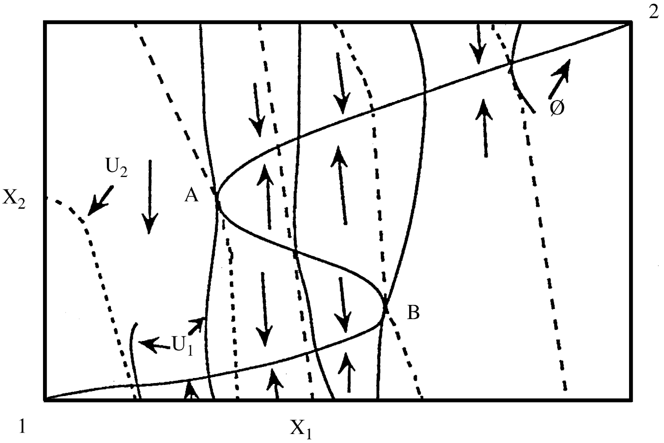

Fig. 3. Pareto set with catastrophe thresholds.

The earliest published application was due to Zeeman (1974) and was an effort to model bubbles and crashes in stock markets. This example has been criticized (Sussman and Zahler, 1978a; Weintraub, 1983), using arguments we shall discuss later.

Debreu (1970) set the stage for applying catastrophe theory to general equilibrium theory in distinguishing regular from critical economies, the latter containing equilibria that are singularities. Discontinuous structural transformations of general equilibria in response to slow and continuous variation of control variables can occur at such equilibria. Analysis of this possible phenomenon was carried out using catastrophe theory by Rand (1976)[5] and Balasko (1978).[6] Rand in particular derives such a case when at least one trader in a pure exchange economy has non-convex preferences, as depicted in Fig. 3, which shows the Edgeworth–Bowley box for such a case.

[5]Two years later, Rand (1978) would publish the first self-conscious model of chaotic dynamics in economics, although others had provided such examples earlier without realizing what they were, e.g. Strotz et al. (1953). This latter paper drew on the nonlinear accelerator model of Goodwin (1951), which could also potentially be analyzed using catastrophe theory in the same manner as that of Kaldor (1940), but which this author is unaware of anyone actually doing so.

Although not specifically using catastrophe theory, Debreu's colleague in mathematics at Berkeley, Smale (1974) also studied structural stability of general equilibria, drawing on his earlier work on genericity that also provided a foundation for chaos theory (Smale, 1967). Smale was in close contact with Thom and Zeeman during the early 1970s (Aubin, 2001), and would later encourage Hal Varian to study catastrophe theory, leading him to develop his business cycle model based on the Kaldor model, discussed below (I thank Weintraub, 1983, for this information, who interviewed Varian).

[6]Balasko ultimately sided with the critics of applications of catastrophe theory in economics by noting that some of the necessary mathematical conditions are rarely fulfilled, especially that of a potential function.

Bonanno (1987) studied a model of monopoly in which there were non-monotonic marginal revenue curves due to market segmentation. Multiple equilibria can arise with smoothly shifting cost curves, which he analyzed using catastrophe theory.

Perhaps the most influential application of catastrophe theory in economics was to the analysis of business cycles in a paper by Varian (1979). He adopted a nonlinear investment function of Kaldor (1940) as modified by Chang and Smyth (1971) to construct the following model:

$$\mathrm{d}y/\mathrm{d}t = s(C(y)) + I(y,k) - y, \tag{5}$$

$$\mathrm{d}k/\mathrm{d}t = I(y,k) - I_0, \tag{6}$$

$$C(y) = cy + D, \tag{7}$$

with y as national income, k as capital stock measured against a long-run trend, $C(y)$ as the consumption function, $I(y,k)$ as the gross investment function with I_0 an autonomous level of replacement investment, and s the speed of adjustment parameter assumed to be rapid relative to the movements of the capital stock. The nonlinear investment function was assumed to have a sigmoid shape and would shift with the capital stock as depicted in Fig. 4, with $S = I$ being the equilibrium condition.

Within this model a hysteresis cycle with discontinuities can arise as the investment function shifts back and forth during the course of a business cycle, as depicted in Fig. 5.

Varian then extended this model by allowing the consumption function to include wealth, w, as a control variable as follows:

$$C(y,w) = c(w)y + D(w), \tag{8}$$

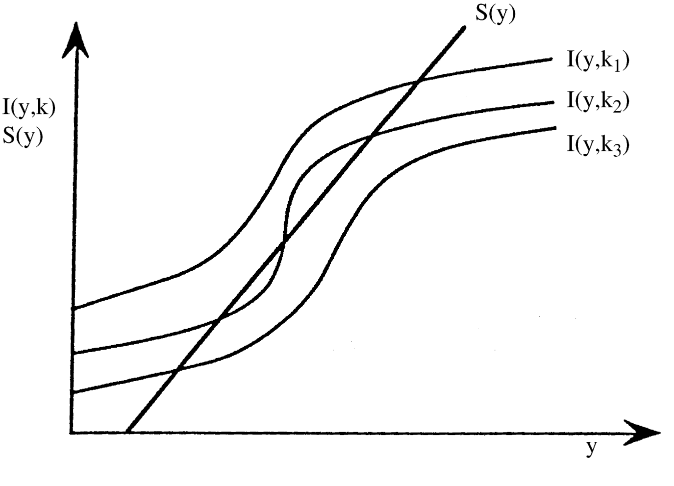

Fig. 4. Nonlinear investment function.

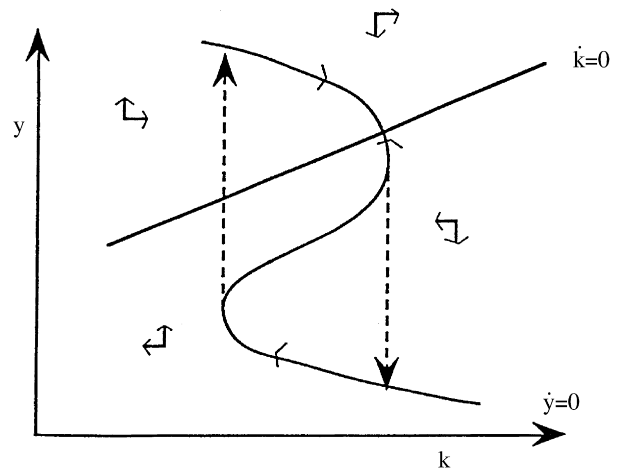

Fig. 5. Business cycle as fold catastrophe.

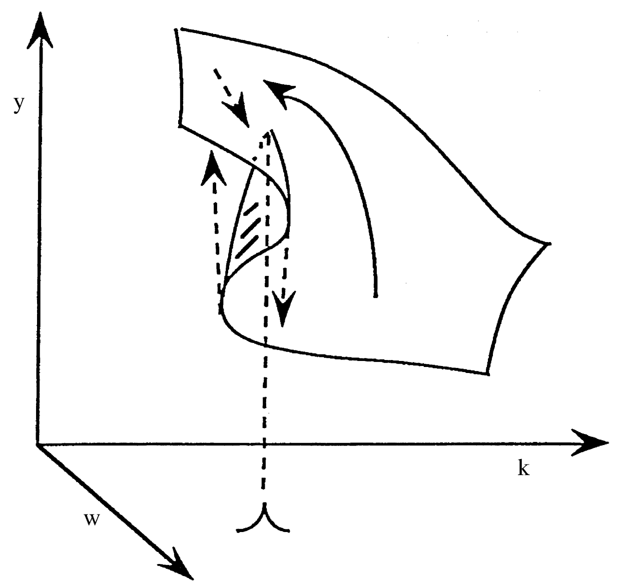

Fig. 6. Business cycle as cusp catastrophe.

with $c'(w) > 0$ and $D'(w) > 0$. This formulation allows for a tilting of the savings function such that there are no longer any multiple equilibria. This allowed Varian to distinguish between simple recessions and longer term depressions. This was depicted by a cusp catastrophe in which wealth is the splitting factor, as depicted in Fig. 6.

One of the few efforts to empirically estimate a catastrophe theory model in economics was of a model of inflationary hysteresis involving a presumably shifting

Phillips Curve. This was due to Fischer and Jammernegg (1986). The method they used was a multi-modal density function due to Cobb (1978, 1981), with further discussion by Cobb et al. (1983) and Cobb and Zacks (1985, 1988). For U.S. data for the period of June 1957 to June 1983, they found a cusp point in the space of the unemployment rate and inflationary expectations of about 7% for each variable. This drew on an ad hoc model suggested by Woodcock and Davis (1978), and in effect argued that this system could be viewed as a cusp catastrophe, with the economy jumping to the 'higher' sheet of the equilibrium manifold during 1973–1974 and then back down again, but then jumping up again at the end of 1977 only to gradually come back down by going around the cusp point after 1980. It is curious that this multi-modal approach developed by Cobb is mentioned by few of the critics of practical applicability of catastrophe theory.

Drawing on models due to Bruno (1967) and Magill (1977), Rosser (1983) analyzed dynamic discontinuities in an optimal control theoretic growth theory model that contained capital theoretic paradoxes. Ho and Saunders (1980) developed a catastrophe theory model of bank failure when risk factors go beyond critical levels.

The areas of urban and regional economics saw especially large numbers of applications of catastrophe theory, including the use of catastrophe theory models of higher dimensionality than the three-dimensional cusp catastrophe seen above, although some of this work was done by geographers rather than economists and much of it is open to questions regarding ad hocracy. Amson (1975) initiated the formal use of it with a cusp catastrophe model of urban density as a function of rent and 'opulence.' Mees (1975) modeled the revival of cities in medieval Europe using the five-dimensional 'butterfly' catastrophe. Wilson (1976) studied modal transportation choice as a fold catastrophe. Dendrinos (1979) modeled the formation of urban slums using the six-dimensional parabolic umbilic or 'mushroom' catastrophe. Andersson (1986) modeled 'logistical revolutions' in interurban transportation and communications relations and patterns as a function of long run technological change using a fold catastrophe. Following a fully mathematically satisfying framework, structural change in regional trading systems was analyzed using the five-dimensional hyperbolic and elliptic umbilic catastrophes by Puu (1979, 1981a, b)[7] and by Beckmann and Puu (1985).

Within ecologic–economic systems considerable focus has been paid to systems in which there are discontinuous changes in biological populations, including collapses to extinction as a result of interaction with human activities. The multiple equilibria model of fishery dynamics in the case of backward-bending supply curves was initially studied by Copes (1970), and Clark (1976) examined it in the context of catastrophe theory. The basic pattern is depicted in Fig. 7 in which outward shifts of

[7] Puu (1989) was probably the first to analyze an economic model using both catastrophic and chaotic dynamics in a model of business cycles in which an economy experiences temporary periods of chaotic dynamics immediately after catastrophic jumps occur. Rosser (1991) has labeled such a phenomenon as being 'chaotic hysteresis.' Puu was specifically inspired by the Goodwin (1951) model. Almost certainly the first to combine catastrophe theory with a cellular automata model were Albin and Hormozi (1983) in their model of technological change limited by information and institutions.

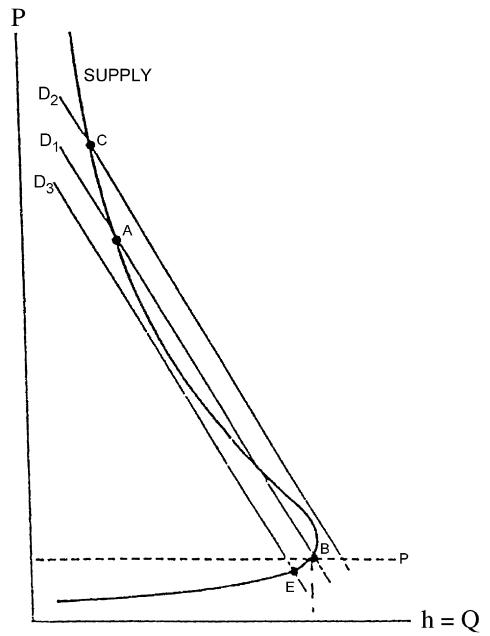

Fig. 7. Overfishing catastrophe with backward-bending supply.

the demand curve due to rising incomes or preferences for fish can lead to discontinuous changes in equilibria. A somewhat similar model with improved fishing technology as a control variable was due to Jones and Walters (1976).

Another vein of argumentation drew on models of predator–prey dynamics, such as the spruce budworm dynamics in forests modeled by Ludwig et al. (1978). Walters (1986) examined a fold catastrophe model of Great Lakes trout dynamics using such a predator–prey model to study how yields could be maximized while avoiding a catastrophic collapse by using a so-called 'surfing' strategy, refuting the widespread argument that catastrophe theory has no practical application. Unsurprisingly there has continued to be more interest in catastrophe theory models, or variations on them, in ecologic–economic modeling than in other areas of economics, although often with multiple equilibria in fold or cusp patterns that are not identified with catastrophe theory explicitly (Wagener, 2003).

In international finance, George (1981) studied foreign currency speculation in a model with non-convex risk preferences, using a cusp catastrophe that essentially followed Rand's (1976) general equilibrium model. Although he did not put it formally into a catastrophe theory framework, Krugman's (1984) model of multiple equilibria in the demand for foreign currencies could rather easily be put into such a framework following along the lines of the Varian (1979) approach.[8]

[8]Krugman's (1991) core-periphery model of regional economic structure could also be easily put into a catastrophe theory framework, although he has not done so. However, Baldwin et al. (2001) have used the

4. The debate and downfall of catastrophe theory

The major controversy and debate surrounding catastrophe theory erupted quite early in the process during the late 1970s. The outcome of this debate would be a residue that gradually corroded the support for using catastrophe theory and culminated in the current widespread disdain. Again, among mathematicians the view is widely held that although there was an overhyping of catastrophe theory in the first place, the current disdain is overdone and that catastrophe theory is a proper method if used correctly.

The most important criticisms of catastrophe theory applications in general were by Zahler and Sussman (1977), Sussman and Zahler (1978a, b),[9] and Kolata (1977). Responses appeared in *Science* and *Nature* in 1977, with a more vigorous and extended set of defenses appearing in *Behavioral Science* (Oliva and Capdeville, 1980; Guastello, 1981),[10] with the first of these being the source of the line that 'the baby was thrown out with the bathwater.' More balanced overviews came from mathematicians (Guckenheimer, 1978; Arnol'd, 1992).

The critics succeeded in pointing out some dirty bathwater.[11] The most salient points include: (1) excessive reliance on qualitative methods, (2) inappropriate quantization in some applications, and (3) the use of excessively restrictive or narrow mathematical assumptions. The third point in turn has at least three sub-points: (a) the necessity for a potential function to exist for it to be properly used, (b) that the necessary use of gradient dynamics does not allow the use of time as a control variable as was often done in many applications, and (c) that the set of elementary catastrophes is only a limited subset of the possible range of bifurcations and catastrophes. These arguments relate to applications of catastrophe theory in general rather than to economics specifically.

Regarding excessive reliance on qualitative methods, it is true that the majority of catastrophe theory models have had that character. Indeed, this criticism can be leveled at most of bifurcation theory as it was developed by Poincaré and his various followers, especially those in Russia (Andronov et al., 1966). Nevertheless, this does not rule out specific quantitative models under the right circumstances. Even critics Sussman and Zahler agree that there are possible applications, especially in physics and engineering such as with structural mechanics where specific quantifiable models can be derived from underlying physical laws. This is harder in economics, but not as

(*footnote continued*)
language of 'catastrophic agglomeration' in connection with a closely related model, a continuing sign that in urban and regional economics there has remained more openness to such approaches as there has also within ecological economics. Another paper to mention possibly using catastrophe theory for the model studied was Gennotte and Leland's (1990) model of financial market dynamics. Krugman (1996) once wisecracked that he had 'forgotten more catastrophe theory than most people ever knew in the first place.'

[9]A satire of Sussman and Zahler appeared under the alleged authorship of "Fussbudget and Snarler' (1979).

[10]Psychology is a field that has remained somewhat more open to using catastrophe theory than some others (Guastello, 1995).

[11]They also made arguments that looked serious at the time but petty in retrospect, such as that some crucial papers in catastrophe theory initially appeared in unrefereed Conference Proceedings.

difficult as in some of the 'softer' social sciences. Most examples in economics have been ultimately qualitative in nature.

Closely related has been the second criticism regarding spurious quantization. Putting these first two together, some critics argued that the only viable applications of catastrophe theory were the qualitative ones, which were ultimately useless, and the ones that attempted to be useful and quantitative were improperly done, at least in the softer social sciences. Some of Zeeman's work in particular was among the most fiercely criticized in this regard, particularly his study of prison riots (Zeeman, 1977, Chapters 13, 14) in Gartree Prison in 1972 using a cusp catastrophe, with 'alienation' measured by 'punishment plus segregation' and 'tension' measured by 'sickness plus governor's applications plus welfare visits' as the control variables. Data points drawn from these were imputed to exhibit two cusp surfaces. This study was validly criticized as using arbitrary and spurious variables as well as improper statistical methodology.

Sussman and Zahler (1978a, b) went further to argue that any surface can be fit to a set of points and thus one can never verify that a global form is correct from a local estimate. One should be cautious about extrapolating a particular mathematical function beyond a narrow range of observation, but this argument smacks indeed of 'throwing the baby out with the bathwater,' seeming to deny the possibility of any kind of confidence testing for nonlinear econometric models. There are critics who hold such positions, but they are generally held for all econometric models, not merely the nonlinear ones.

As noted above in the discussion of Fischer and Jammernegg (1986), it is possible to use multi-modal methods developed by Cobb (1978, 1981) and others. Crucial to these techniques are data adjustments for location, often using deviations from the sample mean, and for scale that use some variability from a mode rather than the mean. These methods have problems and limits, such as the assumption of a perfect Markov process in dynamic situations. An alternative proposed specifically for estimating the cusp catastrophe model is the GEMCAT method due to Oliva et al. (1987), although Guastello (1995, p. 70) has criticized this technique as subject to Type I errors due to an excessive number of parameters. In any case, the general issue here is that empirical studies of quantitative models should conform to accepted statistical and econometric methodologies, and they are not in principle more difficult to apply to the estimation of catastrophe theory models than they are to any other kind of nonlinear model.

An outcome from this debate over qualitative methods and spurious quantization was a split between the two main promulgators of catastrophe theory, Thom and Zeeman. Whereas Zeeman was the main author of the quantitative studies that came under criticism, Thom had always been more the abstract theoretician and philosopher of catastrophe theory.[12] He eventually came to agree with Zeeman's

[12] It is not accidental that there remains a more favorable attitude towards catastrophe theory in Thom's homeland of France, favorable to highly abstract thought, with Lordon (1997) providing a recent application in economics. This may also have to do with the less dramatic meaning that the word 'catastrophe' has in the French language than it does in English, with minor social faux pas regularly

critics (Thom, 1983), arguing that 'There is little doubt that the main criticism of the pragmatic inadequacy of C.T. [catastrophe theory] models has been in essence well founded' (1983, Chapter 7). More broadly he defended the strictly qualitative approach, criticizing what he labeled as 'neo-positive epistemology.' Catastrophe theory was to be used for 'understanding reality' and for the 'classification of analogous situations.' Even before the controversy broke he had declared (Thom, 1975, p. 382):

> On the plane of philosophy properly speaking, of metaphysics, catastrophe theory cannot, to be sure, supply any answer to the great problems which torment mankind. But it favors a dialectical, Heraclitean view of the universe, of a world which is the continual theatre of the battle between 'logoi,' between archetypes.

Such remarks led Arnol'd (1992) to refer to the 'mysticism' of catastrophe theory. More generally Thom would argue that catastrophe theory showed how qualitative changes could arise from quantitative changes as in Hegel's dialectical formulation (see Rosser, 2000b for further discussion).

Regarding the first of the arguments about mathematical assumptions, the need for a potential function to exist is one that is a serious problem for many economics applications. Some clearly satisfy this assumption, with the general equilibrium ones by Rand (1976) and Balasko (1978) fulfilling this, as well as the regional models of Puu (1979, 1981a, b) and Beckmann and Puu (1985). This requirement that led Balasko to argue that proper applications of catastrophe theory to economics would necessarily be limited. One response due to Lorenz (1989) is that the existence of a stable Lyapunov function may be a sufficient alternative, which will hold for many models, although such cannot in general be demonstrated for purely qualitative models. Another response is that implied by the above discussion of definitions, that this is really a problem for the more narrowly defined original version of catastrophe theory, but is not so much of one for more broadly defined singularity theory versions.

Varian (1981, p. 108) suggested that the narrower version of catastrophe theory 'is well developed only for studying *local catastrophes of gradient systems*' (italics in original), citing Golubitsky (1978). However, Golubitsky and Schaeffer (1985, p. 167) argue that the complaining about the non-existence of a potential function is 'a red herring' for systems that can be reduced to $n = 1$, in which case using contact equivalence will generate the 'same set of pictures' and there will be a 'correspondence between catastrophe theory and singularity theory that can be made in either direction, through differentiation or integration, as appropriate. In other words, for $n = 1$, potential functions always may be constructed.'[13] In this regard, Leahy (2001) notes that the quartic cusp catastrophe ($f(x) = -x^4/4 + ax^2/2$)

(*footnote continued*)
described as 'catastrophes.' Weintraub (2002, p. 182) argues that Thom was a 'Bourbakist.' Although Thom was initially trained by French Bourbakist mathematicians, the form of intellectual abstraction he pursued in this later period was very anti-Bourbakist in spirit and abjured formal, axiomatic approaches.

[13] I thank Brock for bringing these references to my attention.

and its negative are symmetric and form the universal family of one-parameter catastrophe functions.

The second mathematical limitation involves the fact that gradient dynamics do not allow for time to be an independent (or control) variable, a point especially emphasized by Guckenheimer (1973). Thom (1983, pp. 107–108) responded to this by arguing that an elementary catastrophe form may be embedded in a larger system with a time variable. Indeed in his original discussion Thom (1972, pp. 38–40) makes clear that what is being discussed are dynamical systems in a $B \times t$ space, where the structural characteristics are in the B part of it. If the larger system is transversal to the catastrophe set in the enlarged space, then there will be no problem. Of course, it will be very difficult to determine this in practice. However, while most reject the idea of time as an explicit variable in catastrophe theory models, we note the discussion above regarding the definition of catastrophe theory, and that broader views of it may not find this so objectionable.

Finally there is the argument that the elementary catastrophes are only a limited subset of the possible range of bifurcations and discontinuities. As Arnol'd (1992) shows that there are infinite such sets and even infinite families of such sets as the number of dimensions exceeds 11, this is clearly true, but is only a criticism of the idea that catastrophe theory is some kind of general answer to all questions and problems. It is not a valid criticism of using catastrophe theory in situations for which it is appropriate.

5. Criticism of an economic application

Regarding economic applications of catastrophe theory, there was relatively little specific discussion during the debates in the late 1970s. The main economic application discussed was probably the first one ever made, Zeeman's (1974) model of stock market crashes. However, much of the criticism directed at this model was misguided. That these criticisms fed into the current negative attitude towards applying catastrophe theory in economics therefore calls for correction.

Zeeman models stock market dynamics as reflecting the interactions of two different kinds of agents, *fundamentalists* who know what the true value of an asset is and who buy when the asset is below that true value and sell when it is above that value, and *chartists* who chase trends, who buy as price rises and sell as price falls. The formulation is somewhat different from most economic models in that what is modeled is the rate of change of price rather than the level of price. This rate of change of price is J, the state variable. It is modeled as determined by the excess demands of the two groups, F for the excess demand of the fundamentalists and C for the excess demand of the chartists. These two are the control variables then for a cusp catastrophe in which F is the normal factor and C is the splitting factor, as shown in Fig. 8. If all agents are fundamentalists, then the market will be well-behaved and stable, with a unique equilibrium that is actually a rate of change of price, although if the equilibrium for the midpoint equals zero then that will coincide with the random walk model. As C increases and the cusp point is passed, possible

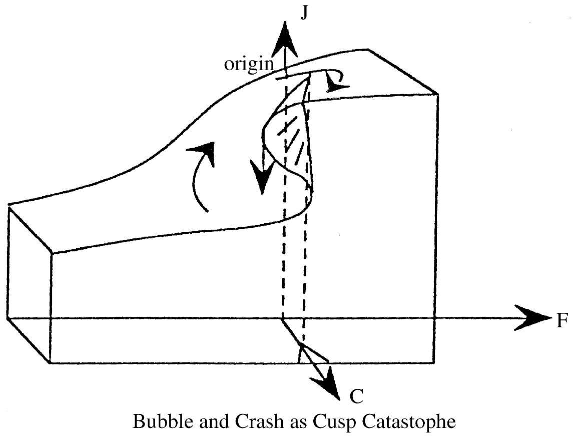

Fig. 8. Bubble and crash as cusp catastrophe.

instability and discontinuous changes in J appears. Zeeman's original story involved C rising as the price accelerates until there is a crash; then C declines as chastened investors revert to more cautious fundamentalist behavior.

In their most sustained critique of applied catastrophe theory, Sussman and Zahler (1978a) criticized Zeeman's model on three grounds. First (pp. 133–134), they argue that the model is essentially tautological, arguing that it does not explain crashes in a 'nontrivial' way, but 'at best *restate*[s] the fact that there are crashes, not *account*[s] for it…There are jumps because there are jumps.' It is true that for Zeeman's model to generate discontinuities, he must assume his accompanying story regarding the respective behavior of his traders. However, this story is quite reasonable and is consistent with many more recent models of heterogeneous agents in financial markets in which the balance between fundamentalists and chartist types centrally determines the dynamics of bubbles and crashes (Arthur et al., 1997; Lux and Marchesi, 1999; De Grauwe and Grimaldi, 2006). In addition, Brock and Hommes (1998) provide a detailed bifurcation analysis of such a model, and broader overviews can be found in Hommes (2006) and LeBaron (2006).

Second (pp. 138–139), they argue that not all crossings of the bifurcation value will be identified as crashes if they are 'too small.' This may be true, but it is also silly to deny Zeeman's observation that if it is 'very large, causing a steep descent, then we are liable to call the recession a *crash*.' Third (p. 198) they argue that 'Zeeman's stock market model predicts that a purely speculative market cannot crash,' which they claim is 'simply wrong.' However, it is Sussman and Zahler who are simply wrong about this, as it is the market in which there are no chartist speculators that cannot crash.[14]

[14]Of course one can observe a discontinuous decline, or 'crash,' in a rational expectations model of financial dynamics without speculators if a large, negative information shock arrives suddenly. However, this is not the usual sense in which the term crash is used and is simply an instantaneous and efficient adjustment as modeled in the jump-diffusion literature (Merton, 1976). While a crash may be triggered by

Another criticism was made by Weintraub (1983), although he did not argue against ever using catastrophe theory in economics. Coming from a discussion of the tâtonnement process in general equilibrium theory and the stability conditions for that process, he concluded that Zeeman's model implied that chartist traders must have upward-sloping demand curves. He then cited Stigler (1948) who argued that there never has been a true Giffen good with an upward-sloping demand curve. Regarding the idea that a stock market participant might believe that tomorrow's price will depend on what the price has been, Weintraub (1983, p. 80) declared, 'There is no evidence whatsoever to support such an hypothesis,' citing Malkiel's (1975) random walk model.

As regards the claim that a chartist speculator must have an upward-sloping demand curve, which cannot exist because Stigler said so in 1948, what is involved is not a static demand curve, but a situation where the demand curve shifts outwards as the price rises (or accelerates). Weintraub might well respond that in this case it is meaningless to describe the surface in Fig. 8 as an *equilibrium manifold*. But, he himself notes it is not a proper general equilibrium manifold anyway because J is a rate of change of price rather than price itself. Thus it is a different kind of equilibrium, one about a pattern of shifts in demand curves rather than about movements along fixed demand curves. Stigler's argument is simply irrelevant.

Weintraub's argument that somehow chartist traders cannot to be allowed in economic models because of their apparent irrationality reflects its era. This was before Black's (1986) speech to the American Finance Association on noise traders and probably more importantly before the stock market crash of 1987.[15] These events, and the continued appearance of more bubbles and crashes since, have made models using heterogeneous agents, not all of them perfectly rational, much more acceptable.

The Zeeman model has its oddities, but many criticisms of it were fallacious. The idea that these criticisms constituted a case for not using catastrophe theory in economics was absurd. Although current models of financial market dynamics do not generally use catastrophe theory per se, the Zeeman model has regained its respectability and is now recognized as a source of useful insights, and with Rheinlander and Steinkamp (2004) specifically studying it anew.

(*footnote continued*)
the arrival of negative information, it is thought to involve a larger change in price than is justified by that new information, reflecting the reversal of the speculative dynamics.

[15]An immediate response to the 1987 crash was to use chaos theory to model it, then near its intellectual bubble peak, although in retrospect Zeeman's catastrophe model looks more relevant for explaining such large discontinuities. Guastello (1995, pp. 292–297) later did this. Rosser (1991, Chapter 5; 1997) extended the model to a five-dimensional butterfly catastrophe to explain the phenomenon of the 'period of distress' observed in historical bubbles by Kindelberger (2000, Appendix B), in which there is a gradual decline for a period after the peak but prior to the crash. This occurred in 1987 with the peak in mid-August and the crash on October 19. An alternative approach to modeling the period of distress has been done using a heterogeneous interacting agents model with financial constraints (Gallegati et al., 2005).

6. What are the alternatives today?

If one wishes to examine the structural stability of a particular pattern of bifurcation or to compare the topological characteristics of two distinct patterns of discontinuities in economics, then catastrophe theory is the most appropriate method to use for sufficiently low dimensional systems with gradient dynamics derived from a potential function. If, however, one is simply modeling dynamic discontinuities within economic processes, other alternatives exist, most not relying on specific mathematical assumptions that frequently do not hold. Alternative approaches are indicated especially if one of the control variables in the process is time.

Among modern complexity theorists many methods have emerged that can produce phase transitions or dynamic discontinuities in models with heterogeneous interacting agents. Interacting particle models from statistical mechanics in physics have been a source (Föllmer, 1974), with the mean field method one that provides distinct bifurcations describing phase transitions between different forms of system organization (Brock, 1993). Another arises with multiple equilibria when the basins of attraction boundaries are fractally interwoven with each other as in Lorenz (1992). Yet another involves self-organizing criticality wherein small exogenous shocks can trigger much larger endogenous reactions (Bak et al., 1993). Still another uses synergetics, especially the use of the master equation approach (Weidlich and Braun, 1992). In its emphasis upon distinguishing between slowly changing control variables and more rapidly changing slaved variables, the synergetics approach resembles catastrophe theory.

Finally we note the increasing use in economics of models positing multiple equilibria. Often these models generate equilibrium surfaces similar to the equilibrium manifolds of catastrophe theory, although they may fail to fulfill all of the mathematical characteristics of true catastrophe theory. Nevertheless, these models can produce dynamic discontinuities as control parameters are varied in ways that cause the system to cross bifurcation points that separate one equilibrium zone from a discretely different equilibrium zone, thus effectively being close siblings of catastrophe theory. While they have long been known as possibilities (von Mangoldt, 1863; Walras, 1874; Marshall, 1890), it is only recently that their use has become widespread.

An example of such an approach uses Skiba points (or regions or surfaces), originally studied for convex–concave production functions in optimal growth models (Skiba, 1978; Dechert and Nishimura, 1983). The Skiba point separates the basins of attraction of the distinct equilibria and for this model was used to explain dualistic growth outcomes like endogenous growth models. More recently this has been applied more widely (Deissenberg et al., 2001).

A striking example is due to Wagener (2003) of multiple equilibria in an ecologic–economic model of pollutants in a lake system (Brock et al., 1999; Mäler et al., 2003; Brock and Starrett, 2003; Dechert and O'Donnell, 2006). Catastrophe theory is not mentioned per se, but Wagener finds a sufficient condition for a Skiba point to exist for this lake system is for a Hamiltonian cusp bifurcation as described in Thom's book (1972, p. 62) to exist.

Brock has argued that catastrophe theory may be limited in its applicability to this case because of symmetry condition requirements (Colander et al., 2004, p. 173) and notes that the lake game may not fulfill these conditions, even as it may well fit into the broader view of catastrophe theory. Symmetry conditions are required to prove the Morse Lemma (Castrigiano and Hayes, 1993, Chapter 1), but are not required for the Malgrange–Mather preparation theorem, key to proving the Thom theorem (Castrigiano and Hayes, Chapters 10 and 11). However, as noted above, as long as catastrophe theory is limited to gradient systems with potential functions, the symmetry condition of equal cross-partial derivatives is necessary, but not sufficient (Apostol, 1969, pp. 340–341). Symmetry also guarantees integrability, which has important implications for welfare analysis of dynamic games, such as the lake model, which fails to fulfill these conditions, although the special category of 'potential games' does fulfill these conditions (Sandholm, 2001, 2006). This question links back to the somewhat unresolved question regarding the definition of catastrophe theory, with more generalized views tied to generalized singularity theory less dependent on such symmetry conditions, whereas the narrowest of definitions closely tied to the original Thom formulation assumed their presence.

7. Conclusion

Catastrophe theory experienced one of the most dramatic intellectual bubbles ever seen. After a gradual development over many decades, it burst onto the intellectual scene in the early and mid-1970s following the publicizing of the work of Thom and Zeeman. Part of the reason for its faddishness at that time was the socio-cultural environment. Radical political movements abounded, and dramatic changes in the world economy were happening such as the extreme shocks to food and oil prices in the early 1970s. The idea that huge, sudden, and revolutionary changes might happen had considerable widespread appeal, especially among dissident intellectuals, but inappropriate applications of the theory undermined its credibility. A counter-attack came in the late 1970s, and as the 1980s wore on fewer applications of catastrophe theory were seen, especially in economics, although catastrophe theory always retained more respectability among mathematicians as a special case of bifurcation theory. Nevertheless, there were many proper applications of catastrophe theory in economics before the counterattack's influence was fully felt.

Criticisms of applications of catastrophe theory included that it involved excessive reliance on qualitative methods, that many applications involved spurious quantization or improper statistical methods, and that many models failed to fulfill mathematical conditions such as possessing a true potential function. Also, responding to the claimed universal applicability of catastrophe theory it was noted that the elementary catastrophes are only a subset of the more general set of bifurcations and singularities. Nevertheless, empirical methods such as multi-modal models can be used for estimating catastrophe theory models, which have been little used in economics.

The general critics of catastrophe theory also subjected Zeeman's (1974) of financial market dynamics to harsh criticism. However, reevaluation suggests many of these criticisms were misguided. To the extent that economists have avoided using catastrophe theory because of those critiques, they should no longer do so. With the decline of catastrophe theory, other methods of modeling dynamic discontinuities in economic models have appeared, some of them around longer than catastrophe theory and some closely connected to catastrophe theory such as analyzing Skiba points in multiple equilibria dynamical systems.

In sum, it would appear that the baby of catastrophe theory was largely thrown out with the bathwater of its inappropriate applications. Although there are serious limits to its proper application in economics, there are many potential such applications, especially when one considers its broader version in generalized singularity theory rather than the narrower version originally formulated by Thom. Economists should no longer shy away from its use and should include it in the family of methods for studying dynamic discontinuity. It should be revalued from its currently low state on the intellectual bourse, righting the wrong of its excessive devaluation, while avoiding any return to the hype and overvaluation that occurred in the 1970s. A reasonable middle ground can and should be found.

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