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THE RISE AND FALL OF CATASTROPHE THEORY APPLICATIONS IN ECONOMICS: WAS THE BABY THROWN OUT WITH THE BATHWATER?

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THE RISE AND FALL OF CATASTROPHE THEORY APPLICATIONS IN ECONOMICS: WAS THE BABY THROWN OUT WITH THE BATHWATER?

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Introduction

The science writer, John 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 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.[2] 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.

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[2] 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, René Thom, worried that it could "have the same fate as cybernetics." (Aubin, 2001, p. 274).

That this is indeed its widely perceived status (or lack thereof) was recently reinforced after the death of its publicly identified founder, the French mathematician René Thom. Obituaries and other reports in media outlets discussed how he had been somewhat depressed and withdrawn in his latter years because of the decline in status of his most famous intellectual progeny. Such reports would not be made if there were nothing to them. Although as we shall see, many of the crucial ideas of catastrophe theory were invented/discovered well before he began to study them, he was the person who first proposed its use as a form of applied topology (Thom, 1969) and was as responsible for the original publicity that generated the reputed fad or intellectual bubble, the other principal figure being the Warwick mathematician, E. Christopher Zeeman (1977), who was responsible for coining the term "catastrophe theory."

That this is supposedly the status of catastrophe theory more generally means that it is not surprising that this 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 in her right mind would evenly remotely 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" in it all in any context, however obscure or remote.[3] Whether catastrophe theory was actually a loveable baby or merely a bucket of worthless bathwater, it has most definitely been thrown out by economists, as it has been by scholars from many other disciplines as well. The case would seem to be closed. Indeed the widespread nature of its rejection would seem to be the final proof that it was really just bathwater after all and

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[3] An example of this is Wagener (2003), to be discussed below, which uses catastrophe theory without ever mentioning its name. Of course there has been a non-trivial literature in recent years that has discussed catastrophe insurance. But such discussions rarely, if ever, rely upon catastrophe theory per se.

that Horgan was fully justified to hold it up as the prime example of a ridiculous and ultimately worthless intellectual fad.

This paper will suggest that this viewpoint may need some 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 more than two decades ago by Oliva and Capdeville (1980) in the journal *Behavioral Science*. This suggests that indeed there was some bathwater that needed to be thrown out, but that catastrophe theory itself was not that bathwater, that it was in fact an at least not totally unloveable baby that deserved to be preserved and raised in a proper household. The position ultimately taken in this paper will partly agree with this perspective. Sins of intellectual hype and exaggeration were committed as were inappropriate applications of the theory. It is not as general in application as its original proponents claimed, and most certainly it is not a general intellectual panacea. There was a fad and an intellectual bubble, and it was perfectly reasonable that there should have been a discounting and a downgrading to some extent. However, it would appear that this has been overdone, that the intellectual marketplace has inefficiently overshot on the downside. A rather telling sign of this is that probably the field in which the attitude towards catastrophe theory did not fall so low, partly because it did not rise so high in the first place, is mathematics. It is indeed time that non-mathematicians became cognizant of this fact and reevaluated the former fad in order to move it to a more proper valuation. This includes in economics as well, where the baby should be brought back in from the outside to be given a proper high chair, if not the highest one in the house.

This paper will proceed by first reviewing the mathematical origins and background of catastrophe theory. Then it will review the details of some of its applications in economics. This will be followed by an examination of the controversy and debate regarding the use of catastrophe theory, with a special focus on discussion of the Zeeman (1974) model of financial market dynamics. Finally, we shall note some alternative approaches that are being used now in order to model the phenomenon of dynamic discontinuity in economics. Although catastrophe theory may deserve to have its own high chair in the house once again, there are some other babies that deserve at least as high a high chair as it does.

The Emergence of Catastrophe Theory out of General Bifurcation Theory

It can be said that catastrophe theory is a special case of singularity theory, which is in turn the key to bifurcation theory, part of the study of nonlinear dynamical systems. Bifurcation theory is widely argued to have been invented/discovered by the great French mathematician, Henri Poincaré, as part of his qualitative analysis of systems of nonlinear differential equations (1880-1890). 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 the system would leave it relatively unchanged in its behavior or cause it to move in a very different manner. It was this particular investigation that led to bifurcation theory.

Although Poincaré was the first to formally analyze bifurcation theory, there was already a fairly well established body of knowledge in mathematics about it. The Russian mathematician, Vladimir Arnol'd (1992, Appendix) has provided a list of precursors to the work of Poincaré. According to him, although an eager enough observer can find hints of it in some of the 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 due to Huygens in 1654. Critical points in geometrical optics were studied by Hamilton in 1837-38. By the late nineteenth century many algebraic geometers were examining the singularities of curves and smooth surfaces with some of these discussions even ending up in textbooks on algebraic geometry. Among those engaged in such studies included Cayley, Kronecker, and Bertini. Nevertheless, it was Poincaré who brought structure to the discussion of these topics.

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

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

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 a zero real part for one of its 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) providing a good summary of the various types.

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The distinction between critical points of functions that are non-degenerate
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(associated with extrema) and degenerate ones (singular, non-extremal) was further
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studied by George Birkhoff (1927), a follower of Poincaré, and also by Birkhoff's
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follower, Marston Morse (1931). In particular, Morse showed how a function with a
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degenerate singularity could be slightly perturbed to a new function that would now
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exhibit two distinct non-degenerate critical points instead of the singularity. This was a
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bifurcation of the degenerate equilibrium and indicates the close connection between the
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singularity of a mapping and its structural stability (see Figure 1).
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[insert Figure 1]

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Hassler Whitney (1955) followed Morse by studying different kinds of
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singularities and their stabilities. This was the sort of approach that was essentially the
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origin of catastrophe theory, although nobody was using that term yet. In fact, Whitney
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discovered/invented the two singularities associated with the two most commonly studied
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kinds of elementary catastrophes, and the only ones that are stable in all their forms, the
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fold and the cusp (see Figure 2). He showed that these were the only two kinds of
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structurally stable singularities for differentiable mappings between two planar surfaces.
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Thus Whitney can be viewed as the real inventor/discoverer of catastrophe theory.
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[insert Figure 2]

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Following his discovery of transversality (1956), René Thom developed further
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the classification of singularities, or of elementary catastrophes, although a more
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complete categorization would eventually be carried out by Arnol'd, Gusein-Zade, and
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Varchenko (1985), who showed that for systems beyond a dimensionality of eleven, the
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categories of catastrophes become infinite and thus difficult to categorize. Thom (1972,

pp, 103-08) 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. This became standard catastrophe theory.

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

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

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

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

This set of points constitutes the equilibrium manifold, M, and an example is seen in the cusp catastrophe seen in Figure 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. In turn, 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 (not to be confused with projection functions found in game theory)..

Thom's Theorem, which was rigorously demonstrated by Malgrange (1966) and Mather (1968), states that if the underlying functions fj 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

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catastrophe function will be structurally stable (generic) under slight perturbations and

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can be classified into eleven different types. There are seven such types for r < 5, and

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Thom (1972) provided colorful names for each of these, along with detailed discussions

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of their various characteristics, with further discussion carried out by Trotman and

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Zeeman (1976).[4] For r > 5 and more than two control parameters, the set of possible

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catastrophes is infinite.

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Although there have been some applications of catastrophes of somewhat higher

dimensionality in economics, especially in urban and regional economics, most of the

applications have involved the two simplest forms already known to Hassler Whitney in

1955, the fold and the cusp depicted in Figure 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. For the simplest case of the fold

catastrophe four kinds of behavior can occur: *hysteresis, bimodality, inaccessibility,* and

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sudden jumps. An additional phenomenon that can occur in the case of the cusp

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catastrophe is *divergence,* which involves the increasing separation of the two planes of

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the equilibrium manifold as the value of the so-called *splitting factor* control variable

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increases in value. When its value is sufficiently low, there are no discontinuities and the

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system is controlled by variations of the so-called *normal factor,* the other control

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variable.

Some Applications in Economics

As we shall see, critics of catastrophe theory have argued that many applications of it in many fields were either done in violation of necessary assumptions or were carried out in other ways that are questionable for one reason or another. Let us list a few examples of applications in economics, most of which in this author's view were done in a reasonable manner.[5]

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 much criticized (Zahler and Sussman, 1977; Weintraub,1983). We shall return later to discuss the criticisms of this particular model after we discuss the question of the broader criticisms of catastrophe theory and the debate that arose around it.

Although he did not use catastrophe theory directly, Debreu (1970) set the stage for doing so in regard to general equilibrium theory with his distinction between regular and 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)[6] and

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[5] For more extensive discussions of such applications in economics, see Rosser (1991, 2000a).

[6] 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, MacAnulty, and Naines (1953).

Although not specifically using catastrophe theory, Debreu's colleague in mathematics at Berkeley, Steve 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 discussed below (I thank Roy Weintraub, 1983, for this information, who interviewed Varian).

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Balasko (1978).[7] Rand in particular derives such a case when at least one trader in a pure

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exchange economy has non-convex preferences, as depicted in Figure 3.

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[insert Figure 3]

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        Bonanno (1987) studied a model of monopoly in which there were non-

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monotonic marginal revenue curves due to market segmentation. Multiple equilibria can

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arise with smoothly shifting cost curves, which he analyzed using catastrophe theory.

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        Perhaps the most influential application of catastrophe theory in economics was to

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the analysis of business cycles in a paper by Varian (1979). He adopted a nonlinear

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investment function of Kaldor (1940) as modified by Chang and Smyth (1971) to

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construct the following model.

$$dy/dt = s(C(y)) + I(y,k) - y \qquad\qquad (5)$$

$$dk/dtg = I(y,k) - I_0 \qquad\qquad (6)$$

$$C(y) = cy + D, \qquad\qquad (7)$$

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with y as national income, k as capital stock measured against a long-run trend, C(y) as

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the consumption function, I(y,k) as the gross investment function with I_0 an autonomous

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level of replacement investment, and s a speed of adjustment parameter assumed to be

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rapid relative to the movements of the capital stock. The nonlinear investment function

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was assumed to have a sigmoid shape and would shift with the capital stock as depicted

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in Figure 4, with S = I being the equilibrium condition.

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[insert Figure 4]

[7] 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.

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 Figure 5.

[insert Figure 5]

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

$$C(y,w) = c(w)y + D(w), \qquad\qquad (8)$$

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 Figure 6.

[insert Figure 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).[8] 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 percent 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 1973-74 and then back down

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[8] For further discussion see Cobb, Koppstein, and Chen (1983) and Cobb and Zacks (1985, 1988). It is striking that few of the critics of catastrophe theory who charge that it cannot be used practically or empirically seem to be aware of this work.

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again, but then jumping up again at the end of 1977 only to gradually come back down

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(by going around the cusp point after 1980.

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Drawing on models due to Bruno (1967) and Magill (1977), Rosser (1983)

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analyzed dynamic discontinuities in an optimal control theoretic growth theory model

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that contained capital theoretic paradoxes. Ho and Saunders (1980) developed a

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catastrophe theory model of bank failure when risk factors go beyond critical levels.

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The areas of urban and regional economics saw especially large numbers of

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applications of catastrophe theory, including the use of catastrophe theory models of

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higher dimensionality than the three dimensional cusp catastrophe seen above, although

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some of this work was done by geographers rather than economists. Amson (1975)

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initiated the formal use of it with a cusp catastrophe model urban density as a function of

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rent and "opulence." Mees (1975) modeled the revival of cities in medieval Europe using

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the five dimensional "butterfly" catastrophe. Wilson (1976) studied modal transportation

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choice as a fold catastrophe, Dendrinos (1979) modeled the formation of urban slums

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using the six dimensional parabolic or "mushroom" catastrophe. Structural change in

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regional trading systems was analyzed using the five dimensional hyperbolic and elliptic

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umbilic catastrophes by Puu (1979, 1981a, b)[9] and by Beckmann and Puu (1985).

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Andersson (1986) modeled "logistical revolutions" in interurban transportation and

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communications relations and patterns as a function of long run technological change

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using a fold catastrophe. Some of these applications were somewhat ad hoc, although the

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[9] 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."

ones by Puu and by Beckmann and Puu especially stand out as fulfilling all the

mathematical conditions for proper application of catastrophe theory.

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 Figure 7 in which outward shifts of 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).

[insert Figure 7]

Another vein of argumentation drew on models of predator-prey dynamics, such as the spruce budworm dynamics in forests modeled by Ludwig, Jones, and Holling (1978). Most notably, 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. This example refutes the widespread argument heard that catastrophe theory had no practical application. Unsurprisingly there has continued to be much more interest in catastrophe theory models, or variations on them, in ecologic-economic modeling, although often these are models with multiple equilibria in fold or cusp patterns that are not identified with catastrophe theory explicitly (Wagener, 2003). Of course this may

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well reflect the current disrepute in which catastrophe theory is so widely held and the

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desire not to be tainted with it.

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In international finance, George (1981) studied foreign currency speculation in a

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model with non-convex risk preferences. This used a cusp catastrophe and essentially

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followed the approach used in the Rand (1976) general equilibrium model. Although he

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did not put it formally into a catastrophe theory framework, Krugman's (1984) of

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multiple equilibria in the demand for foreign currencies could rather easily be put into

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such a framework following along the lines of the Varian (1979) approach.[10] Of course

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many models are now studied of multiple equilibria in foreign exchange rate models,

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with many of these taken very seriously given the numerous foreign exchange crises that

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have occurred in recent years.

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The Debate and Downfall of Catastrophe Theory

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The major controversy and debate surrounding catastrophe theory erupted quite

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early in the process during the late 1970s, prior to the full working out of many of the

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applications noted in the previous section. But the outcome of this debate would be a

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residue that would gradually corrode the support for using catastrophe theory and

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culminated in the widespread disdain that we see today. Again, before we get into the

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details of the debate, I note that among mathematicians the view is widely held that

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although there was an overhyping of catastrophe theory in the first place, the current

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[10] 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, Martin, and Ottaviano (2001) have used the 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, in contrast with most sub-fields of economics. Another paper to specifically mention the possibility of using catastrophe theory for the model studied was Gennotte and Leland's (1990) model of financial market dynamics. Krugman (1996) in particular once wisecracked that he had "forgotten more catastrophe theory than most people ever knew in the first place."

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disdain is overdone and that catastrophe theory is a perfectly proper method to use in the

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right circumstances.

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     The most important criticisms of catastrophe theory applications in general were

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by Zahler and Sussman (1977), Sussman and Zahler (1978a, b),[11] and Kolata (1977).

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Responses appeared in *Science* and *Nature* in 1977, with a more vigorous and extended

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set of defenses appearing in *Behavioral Science* (Oliva and Capdeville, 1980; Guastello,

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1981),[12] with the first of these being the source of the line about "the baby was thrown

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out with the bathwater." More balanced overviews came from mathematicians

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(Guckenheimer, 1978; Arnol'd, 1992).

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     The critics indeed succeeded in pointing out some pretty dirty bathwater.[13] The

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most salient points include: 1) excessive reliance on qualitative methods, 2) inappropriate

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quantization in some applications, and 3) the use of excessively restrictive or narrow

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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. We note that these

arguments relate to applications of catastrophe theory in general rather than to economics

specifically, although most of them apply to at least some applications in economics.

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

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

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

Regarding the charge that there was an excessive reliance on qualitative methods, it is certainly true that the vast majority of catastrophe theory models have had that character. Indeed, this criticism can be leveled at most of bifurcation theory more broadly as it was developed by Poincaré and his various followers, especially those in Russia such as Andronov, Leontovich, Gordon, and Maier (1966). Nevertheless, this fact does not necessarily rule out specific quantitative models under the right circumstances. Even critics such as Zahler and Sussman agree that there are possible applications in some cases, especially in physics and engineering such as with structural mechanics where specific quantifiable models can be derived from underlying physical laws. This is harder to do in economics, but not as difficult as in some of the "softer" social sciences. Nevertheless, although we saw some examples in the previous section, it is certainly true that in economics most examples have been ultimately qualitative in nature.

Closely related to this criticism has been the second one regarding spurious quantization. Putting this one and the first one together, the critics would argue 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, and we shall examine the criticism in relation to his work specifically in economics in the next section. A particular object of criticism was his study of prison riots (Zeeman, 1977, chaps. 13, 14). He modeled them for 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. A scattering of data

points drawn from these was imputed to exhibit two cusp surfaces. This study was criticized as using arbitrary and spurious variables as well as improper statistical methodology. Most of the criticism seems valid for this example.

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. Certainly 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." It would seem to deny the possibility of any kind of confidence testing for nonlinear econometric models. Certainly there are critics of econometrics who hold such positions, but they are generally held for all econometric models, not merely the nonlinear ones. To the extent that this is a valid criticism, it is not one about empirical estimates of catastrophe theory models, per se.

As noted in the discussion above of the Fischer and Jammernegg (1986) study, it is possible to use multi-modal methods as 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 certain problems and limits, such as the assumption of a perfect Markov process in dynamic situations. An alternative that has been proposed specifically for estimating the cusp catastrophe model is the GEMCAT method due to Oliva, Desarbo, Day, and Jedidi (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 there is no particular

reason why such methods 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.

A curious outcome resulting 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.[14] He eventually came to agree with Zeeman's 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, chap. 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 (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."[15]

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[14] It is perhaps not an accident that there remains a more favorable attitude towards catastrophe theory in Thom's homeland of France, home of highly abstract thought, with Lordon (1997) providing a recent application in economics.. This observer has speculated that it 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 described as "catastrophes." Weintraub (2002, p. 182) argues that Thom was a "Bourbakist." Although he 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.

[15] 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.

Such remarks would lead Arnol'd (1992) to make references to the "mysticism" of catastrophe theory.

Regarding the first of the arguments regarding strict mathematical assumptions, the need for a potential function to exist is one that is a serious problem for many economics applications. There are ones that 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). It was this requirement that led Balasko to argue that proper applications of catastrophe theory to economics would necessarily be limited, and this is a strong argument. 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.

The second mathematical limitation involves the fact that gradient dynamics do not allow for time to be an independent variable, a point especially emphasized by Guckenheimer (1973) well before the noisier critics took the stage. Thom (1983, pp. 107-108) responded to this point by arguing that an elementary catastrophe form may be embedded in a larger system with a time variable. 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. Certainly "catastrophe theory" models that use time as an explicit independent variable are not really catastrophe theory models.

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 eleven, this is clearly true. But this is really only a criticism of the

idea that catastrophe theory is some kind of general answer to all questions and problems.

In the current situation where it is widely denied that it has any relevance or usefulness,

such a criticism has rather outlived its day. This is not a valid criticism of using

catastrophe theory in situations for which it is appropriate.

Criticism of an Economic Application

Of course it is not much of a defense of catastrophe theory to point out that

criticisms made of specific models happen to have been misguided. But in regard to

economic applications, there was relatively little specific discussion during the main

debates in the late 1970s. The main economic application that was discussed was

probably the first one ever made, the model of stock market crashes due to Zeeman

(1974). As we shall now see, much of the criticism directed at this model was seriously

misguided. That these criticisms have somehow 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 Figure 8. If all agents are fundamentalists, then the market will be well-behaved and stable, with a unique equilibrium, keeping in mind that this equilibrium is actually a rate of change of price, although if the equilibrium for the midpoint equals zero, then that will essentially coincide with the random walk model. As C increases and the cusp point is passed, the possibility of instability appears and of discontinuous changes in J. Zeeman's original story involved C rising as the price accelerated until there was a crash, at which point C would decline as chastened investors reverted to more cautious fundamentalist behavior. [insert Figure 8]

The more widely publicized critique of this model came in the Zahler and Sussman (1977) general assault on applications of catastrophe theory, with their critique of this model being their one salvo specifically aimed at economics applications. They declared the model to be "unscientific" on grounds that the chartist agents would not have rational expectations. In retrospect such a charge seems quite ridiculous, although it is now largely forgotten among economists that this was the basis of their rejection of Zeeman's model. The model may well be open to criticism on other grounds, such as its making J equal the rate of change of price rather than the level of price. But to reject this model because it does not assume rational expectations for all its agents, and to use this as a basis for more broadly criticizing catastrophe theory, looks like not only throwing out the baby with the bathwater, but in fact throwing out the baby when the water it was bathing in was reasonably clean and not even in need of being thrown out.

How could they make such a silly argument? It must be remembered that 1977 was pretty near the peak of the intellectual bubble of rational expectations, although that idea certainly has considerable value and continues to be used by many macroeconomists in many models. Financial economists in 1977 were especially enamored of efficient markets models of rational, homogeneous agents, arguably even more so than most other economists. It only began to become intellectually respectable to allow for heterogeneous agents in financial market models after Fischer Black (1986) gave his famous presidential speech to the American Finance Association in which he suggested the possibility that "noise traders" might exist. This idea became suddenly much more respectable after the Dow-Jones Average for the U.S. stock market declined 22 percent on October 19, 1987. More recent experience with obvious stock market bubbles and crashes has made such ideas even more acceptable among financial economists, and today such models are commonplace, especially given the broader shift to using heterogeneous agent models that has been inspired by the complexity modelers of the Santa Fe Institute and their associates in the econophysics movement.[16] Interestingly, although few of these models use catastrophe theory explicitly, the Zeeman model is now increasingly cited in these papers and its essential insight has been supported, that the stability or instability of financial market reflects the balance between traders who behave more like the traditional fundamentalists and those who behave more like the traditional

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[16] A much studied model of the stock market with heterogeneous agents who have evolving strategies was developed at the Santa Fe Institute by Arthur, Holland, LeBaron, Palmer, and Tayler (1997). An example of econophysics modeling of heterogeneous agents in financial markets is Lux and Marchesi (1999).

chartists as posited by Zeeman. Despite Zahler and Sussman's earlier dismissal, the Zeeman model is back to some degree.[17]

A less well-known criticism was made later by Weintraub (1983), although he did not criticize using catastrophe theory in economics in general. 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 declared in italics (1983, p. 80), "*There is no evidence whatsoever to support such an hypothesis*," citing Malkiel (1975) and the random walk model. Although Weintraub was writing after Zahler and Sussman, this was still prior to the speech by Black (1986) and the dramatic events in the stock market in 1987, a period the random walk model was more widely accepted than it is today.

As regards the claim that a chartist speculator must have an upward-sloping demand curve, which cannot exist because George 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 reasonably respond that in this case it is meaningless to describe the surface in Figure 8 as an *equilibrium*

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[17] Much of the immediate intellectual response to that event was for many economists to try to explain it using the sensitive dependence on initial conditions idea of chaos theory, which was very much near the peak of its intellectual bubble at that time. Almost nobody bothered to use the Zeeman model, which in retrospect looks much more suitable for explaining the kinds of really big discontinuities one observes in a major stock market crash. One who has used studied the 1987 crash using the Zeeman model is Guastello (1995, pp. 292-297). Rosser (1997) extended the Zeeman model using the five dimensional butterfly catastrophe model to explain the phenomenon observed by Kindleberger (1999), that many historical bubbles experienced a period of gradually declining prices during a "period of distress" after the peak and prior to the main crash. Thus in 1987 the market peaked in August and then gradually declined before plunging on October 19.

manifold. But, as he himself points out, it is not a proper general equilibrium manifold anyway because J is a rate of change of price rather than price itself. It is thus a different kind of equilibrium entirely, one about a pattern of shifts in demand curves rather than about movements along fixed demand curves. Stigler's argument is simply irrelevant.

The Zeeman model may have its oddities, but many of the criticisms of it were fallacious. The idea that these criticisms somehow constituted a case for not using catastrophe theory in economics is simply absurd. Although the 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 for understanding such dynamics. A historical wrong has been righted.

What Are the Alternatives Today?

If what one wishes to do is to examine the structural stability of a particular pattern of bifurcation, or perhaps more specifically to compare the topological characteristics of two distinct patterns of discontinuities in economics, then proper catastrophe theory is clearly the most appropriate method to use for sufficiently low dimensional systems with gradient dynamics derived from a potential function. If, however, what one is interested in doing is simply to model dynamic discontinuities within economic processes, other alternatives certainly exist, most of which do not rely on the specific set of mathematical assumptions that frequently do not hold in specific situations. The use of alternative approaches is especially indicated if one of the control variables in the process under study is time.

Among modern complexity theorists a variety of methods have emerged that can produce phase transitions or dynamic discontinuities of one sort or another in models with heterogeneous, interacting agents. Interacting particle models from statistical mechanics in physics has been the origin of some of these (Föllmer, 1974), with the mean field method one that provides distinct bifurcations that describe phase transitions between different forms of organization of a system (Brock, 1993). Another arises in cases of 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, Chen, Scheinkman, and Woodford, 1993). Still another uses the idea of synergetics, especially as involving 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 has a much stronger superficial similarity to catastrophe theory.

Finally we must note the increasing spread throughout economics of models that posit multiple equilibria. In many cases these models generate possible equilibrium surfaces that have similarities to the equilibrium manifolds of catastrophe theory, although they often 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. Such models are so widespread and ubiquitous that it is not worth even beginning to list them, although they have been knows as long as economists have been aware of the possibility

of multiple equilibria. This is now approaching a century and a half (von Mangoldt, 1863; Walras, 1874; Marshall, 1890). When such models generate equilibrium surfaces that resemble those of true catastrophe theory, we are dealing with something that is rather like a close sibling of catastrophe theory. This sibling now sits ever taller in the high chairs of the house of economics, and its close relationship with catastrophe theory might as well be more generally recognized.

An increasingly popular example of such an approach is that using 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 along the lines seen using endogenous growth models. More recently this has been applied to a wide variety of problems (Deissenberg, Feichtinger, Semmler, and Wirl, 2001). An especially striking model is due to Wagener (2003) of multiple equilibria in an ecologic-economic model of pollutants in a lake system (Brock, Carpenter, and Ludwig, 1999; Dechert and Brock, 2000). Catastrophe theory is never mentioned in this paper explicitly, but when it comes to determining the conditions under which a Skiba point exists for this lake system, Wagener finds that a sufficient condition is for the existence of a Hamiltonian cusp bifurcation as described in Thom's book (1972, p. 62). Catastrophe theory may be all but dead, but in the guise of the study of Skiba points and related phenomena, it lives again.

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 René Thom and Christopher Zeeman. One can readily speculate that part of the reason for its faddishness at that time was the condition of 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 more dissident intellectuals. But widespread applications of the theory that were inappropriate either theoretically or methodologically undermined its credibility. A counterattack came in the late 1970s, and as the 1980s wore on, fewer and 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 applications of catastrophe theory in economics that were properly done 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 quantizations or improper statistical methods, and the general failure of many models to fulfill certain mathematical conditions such as possessing a true potential function or by including time as an independent variable in the analysis. Also, in response to those suggesting some kind of universal applicability of catastrophe theory it was noted that the

elementary catastrophes were only a small set of the more general set of bifurcations and singularities. Nevertherless, empirical methods such as multi-modal models exist that can be used for estimating catastrophe theory models, with these approaches having been used surprisingly little in economics.

The general critics of catastrophe theory also subjected Zeeman's (1974) of financial market dynamics to harsh criticism. However, a careful reevaluation suggests that some of these criticisms were misplaced and misguided. To the extent that economists have shied away from using catastrophe theory because of those critiques, they should no longer do so.

With the decline of catastrophe theory, a variety of alternative methods of modeling dynamic discontinuities in economic models have appeared, although some of them have been around for much longer than catastrophe theory has, and some have close connections with catastrophe theory, especially the analysis of Skiba points in multiple equilibria dynamical systems.

In sum, it would appear that indeed the baby of catastrophe theory was largely thrown out with the bathwater of its inappropriate applications to a large extent. Although there are serious limits to its proper application in economics, there remain many potential such proper applications. Economists should no longer shy away from its use and should include it with the family of other methods for studying dynamic discontinuity. It should be revalued from its currently low state on the intellectual bourse and right the wrong of its excessive devaluation, while avoiding any return to the hype and overvaluation that occurred during the 1970s. A reasonable middle ground can and should be found.

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distinct
non-degenerate
critical points

initial function

perturbed function

degenerate critical point
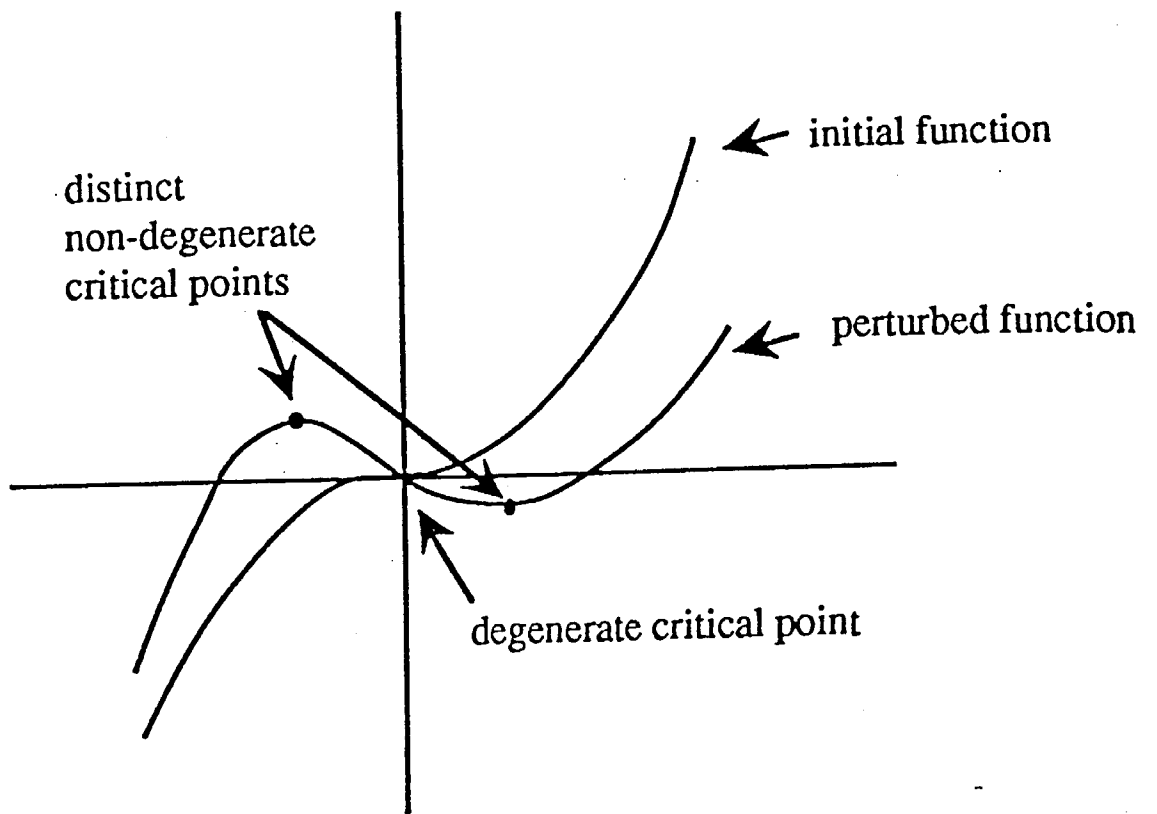
Figure 1:   Bifurcation at a Singularity
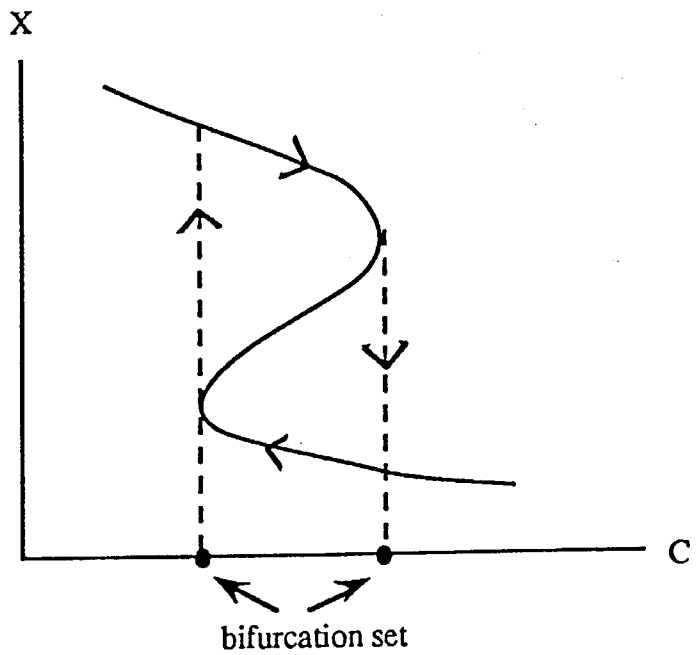
Fold Catastrophe
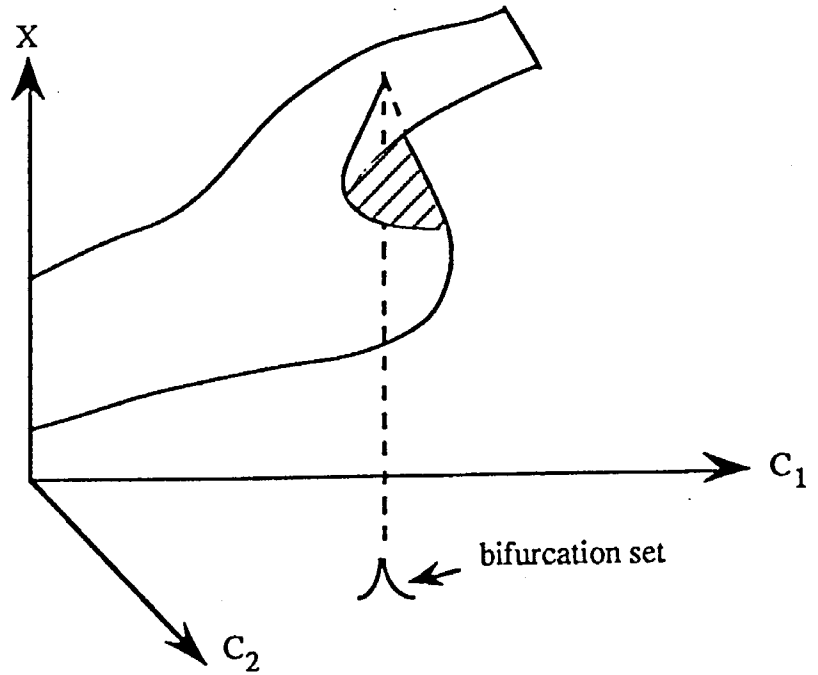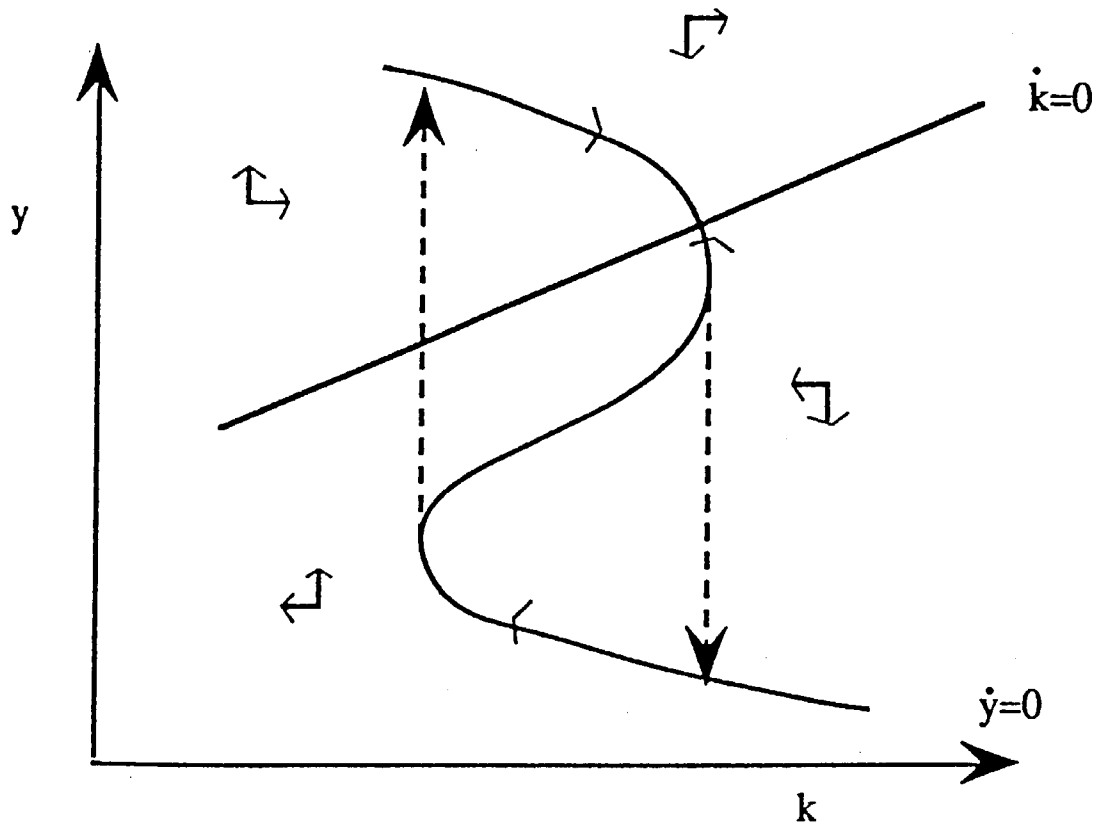

Cusp Catastrophe

Figure 2
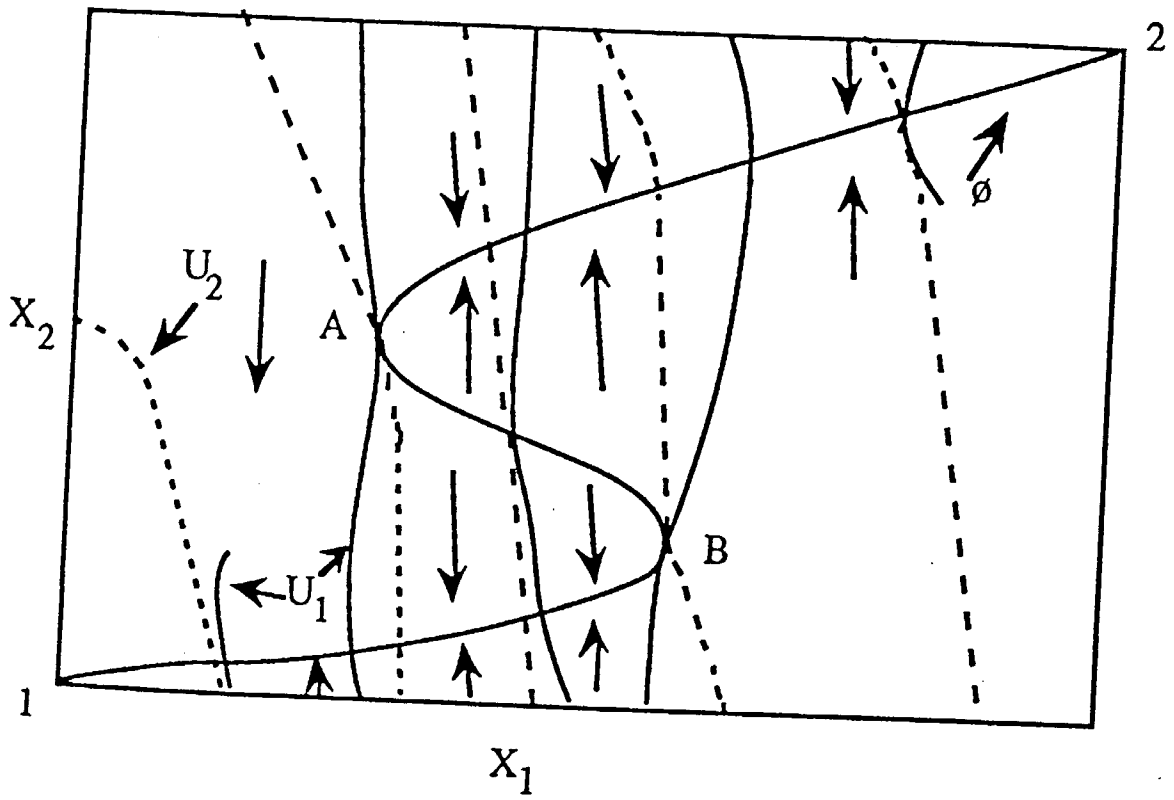
Pareto Set with Catastrophe Thresholds
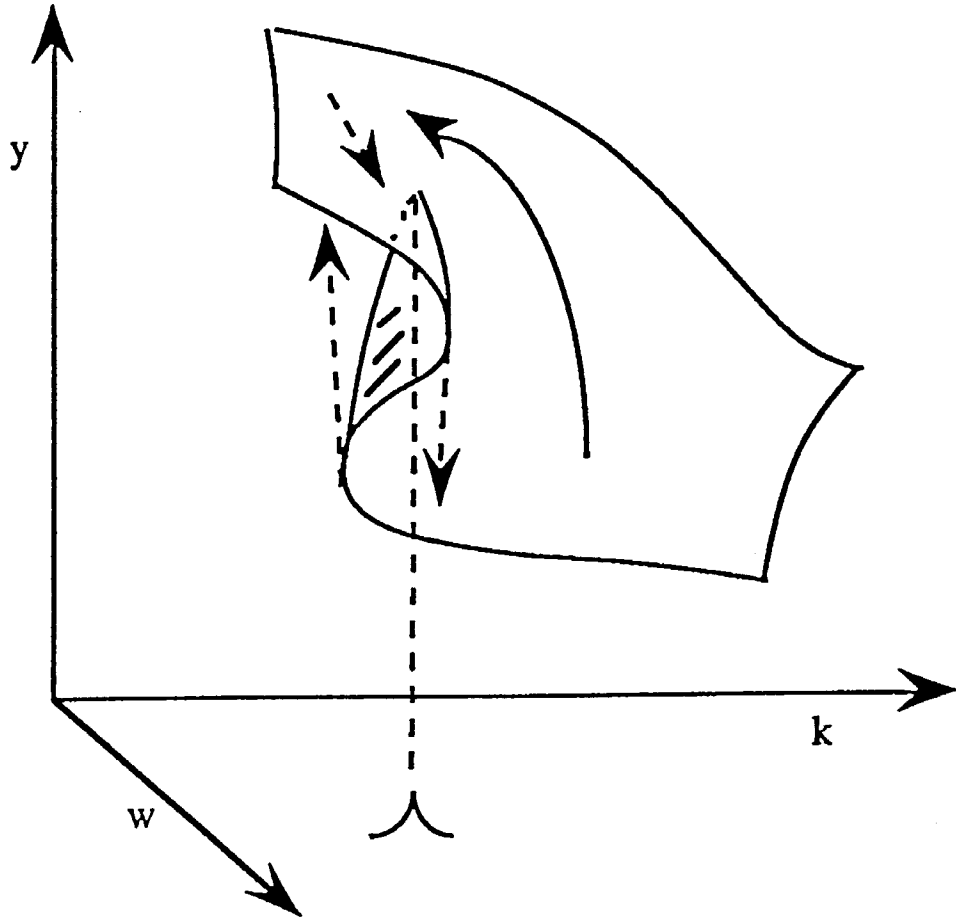
Figure 3
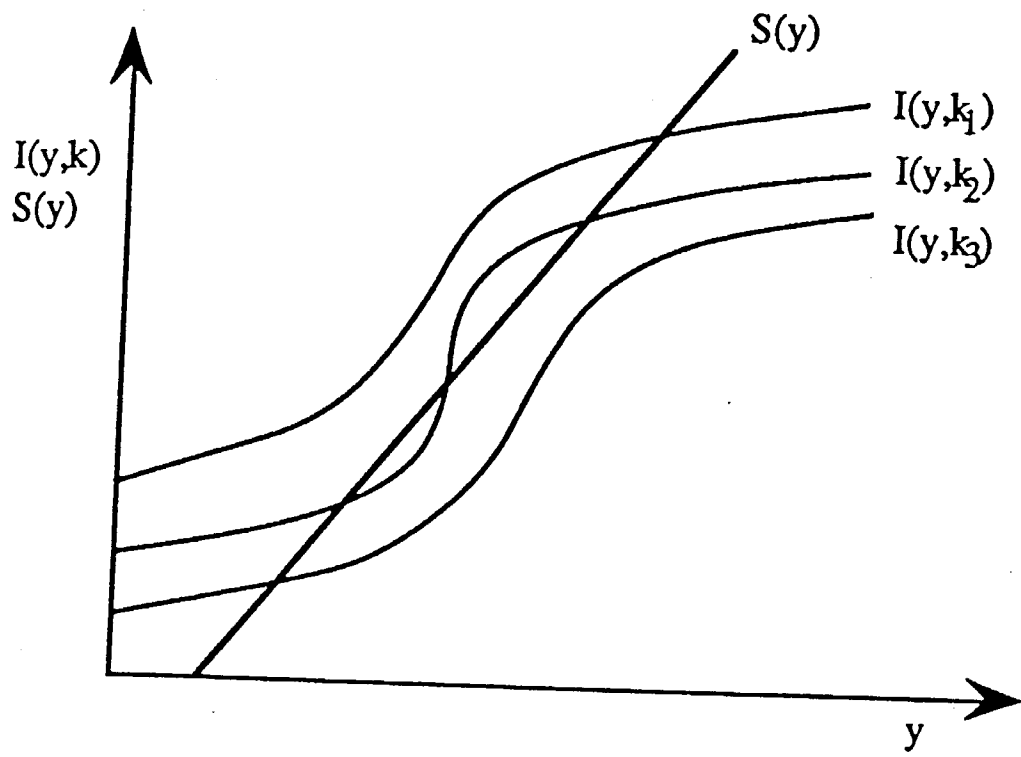
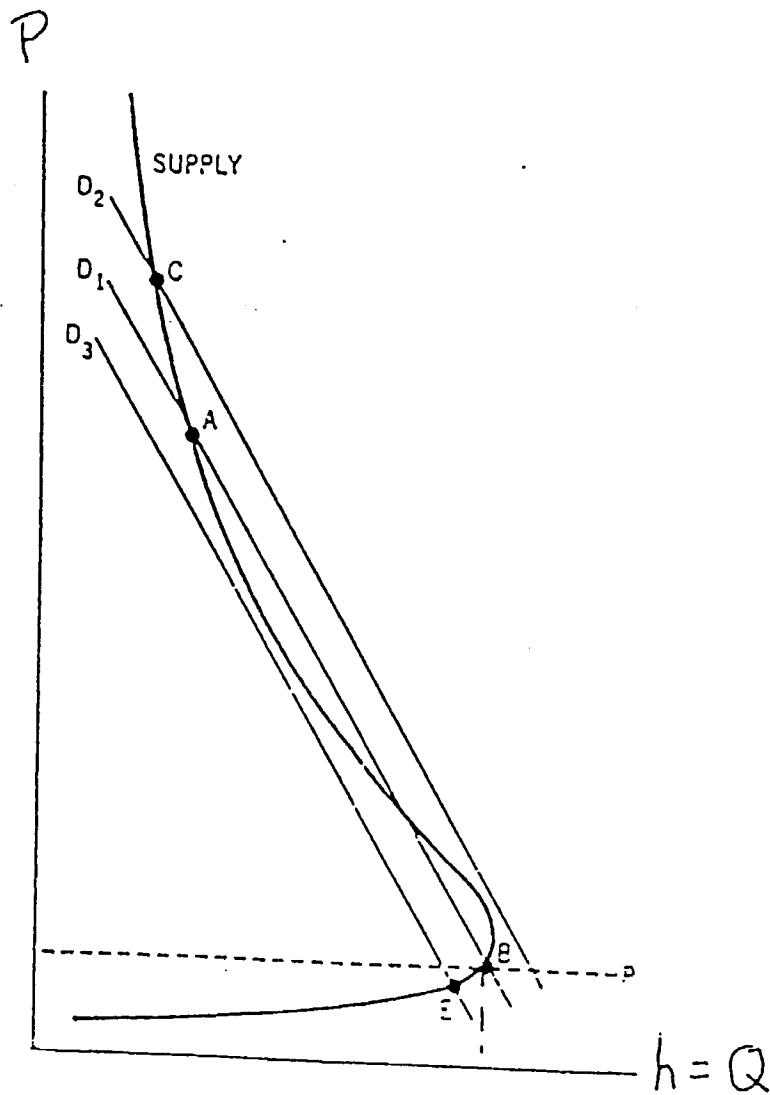Nonlinear Investment Function

Figure 4

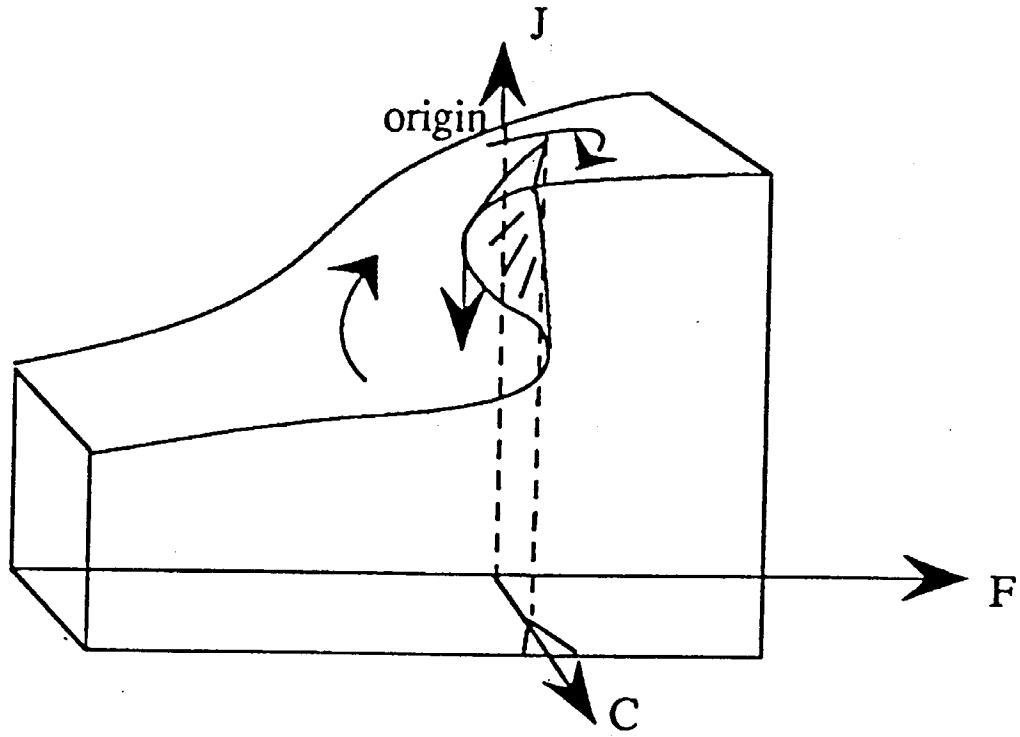Business Cycle as Fold Catastrophe

Figure 5

Business Cycle as Cusp Catastrophe

Figure 6

Overfishing Catastrophe with
Backward-Bending Supply

Figure 7

Bubble and Crash as Cusp Catastrophe

Figure 8