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Implementing the Popper-Samuelson Demarcation in Economics

> Every genuine *test* of a theory is an attempt to falsify it, or to refute it.
>
> Karl R. Popper [1965, p. 36]

In the construction of an economic model the model builder must make methodological decisions about the following: (1) what variables are to be included in the models, (2) which of these variables are to be specified as exogenously determined, and (3) what should be the form of the relationship between the exogenous and endogenous variables. Throughout this book I am concerned with the formal 'benefits and costs' of every aspect of decisions concerning the construction of models in economics. From the standpoint of 'benefits' one would expect that: (a) the larger the number of variables included, the better, (b) the greater the proportion of endogenous variables (i.e. variables to be explained by the model) over exogenous variables, the better, and (c) the more complex (less linear) the form of the relationships, the better. These expectations are held because one suspects that this is the way the 'real world' is. These factors, or attributes, are then the formal 'benefits' of the model constructed. Unfortunately, the increase in any one of these factors, or attributes, usually comes at a 'cost', namely, a decrease in the testability of the model constructed.

Why do some economists consider a reduction of testability a cost? For some optimistic proponents of the Popper-Socrates view of science, 'progress' in science is precluded if we fail to replace theories which have been shown to be false (and/or inadequate) with 'better' theories. New theories must explain why the old theories went wrong as well as what the old could explain. For these proponents of the optimistic Popper-Socrates view, true science is a progressive sequence of models [e.g. Koopmans 1957, Weintraub 1985].

If a new theory or model is constructed in a manner that makes it logically difficult to test (e.g. it has a very high Popper-dimension or it is a tautology), then one reduces the chances for progress by reducing the necessity of replacing it, i.e. by reducing the appearance of failures. In one sense then the reduction in testability is a long-run cost. By a reduction in testability I mean an increase in the Popper-dimension which results from the particular methodological decisions of the model construction discussed above. An increase in Popper-dimension by virtue of a new model (or theory) is then an increase in the long-run theoretical cost of a particular set of methodological decisions (e.g. the cost of changing from a linear model to a non-linear model).

In the short run – that is for the purposes of immediate practical use of the economic theories or models – one may not be as concerned about the testability or even the general truth status of one's models so long as they do their intended practical job. For the applied economic theorists, the question of the model's truth status is not immediately important. However, it may become important should the model or theory fail to assist in the practical job!

1. Calculating the Popper-dimension of explicit models

In light of my discussion of the Popper-Socrates view of science and the implementation of the Popper-Samuelson demarcation criterion expressed in degrees of falsifiability (i.e. the Popper-dimension), I now examine a few specific economic models, some of which can be found in the literature. What will be most interesting is the rather high Popper-dimension of rather small models. In most cases I consider the determinants of the Popper-dimension and how the dimension can be reduced in hopes of making the models more falsifiable. It must be stressed here that I am concerned with the *form* of the solution of a model, thus I will treat all solutions as if we do not know the actual values of the parameters. This amounts to treating the parameters as exogenous *variables*. I will examine the 'methodological sensitivity' of all the parameters and forms of relations. For the eleven simple models

used as illustrations, it will be found that the Popper-dimension varies from a low of zero to a high of over 475,000.

Let us begin with a simple macro model from the end of Chapter 2. As with all models discussed there, I will use bold capital letters for the endogenous variables and lower case letters for both parameters and exogenous variables. Initially, I consider only the following variables to be endogenous:

Y ″ GNP or aggregate spending,
C ″ aggregate consumption.

Model 3

$$\mathbf{Y} = \mathbf{C} + k \qquad [3.1]$$
$$\mathbf{C} = b\mathbf{Y} \qquad [3.2]$$

where *k* and *b* are considered exogenous.

The solution of this model is:

$$\mathbf{Y} = k/(1 - b) \qquad [3.3]$$
$$\mathbf{C} = bk/(1 - b) \qquad [3.4]$$

Equation [3.3] may be rewritten as:

$$\mathbf{Y} - b\mathbf{Y} - k = 0 \qquad [3.3a]$$

I shall now interpret [3.3a] to be a special case of a second-degree surface in 3-space – i.e. a special case of:

$$\mathsf{F}(\mathbf{Y},b,k) = 0 \qquad [3.3b]$$

In general, the algebraic representation of our second-degree surface in 3-space is:

$$0 = \mathrm{A}\mathbf{Y}^2 + \mathrm{B}b^2 + \mathrm{D}k^2 + \mathrm{E}bk + \mathrm{F}\mathbf{Y}k + \mathrm{G}\mathbf{Y}b + \mathrm{H}\mathbf{Y} + \mathrm{J}b + \mathrm{L}k + \mathrm{M} \qquad [3.5]$$

where non-bold capital letters denote coefficients. Note that in general there are nine free coefficients. We are not free to select the value of the tenth coefficient because, except for a special case, that would lead to a contradiction. By *not* specifying the tenth coefficient we are describing a family or system of equations (or surfaces) of a particular *form* – rather than *one* particular equation of a particular form. The emphasis here is with the form. For example, we may be interested in the truth status of *any* linear model not just the truth status of *one* specific linear model.

Considering equation [3.3a], if we let G = –1, H = 1, and L = –1, we are left with the following:

$$0 = \mathrm{A}\mathbf{Y}^2 + \mathrm{B}b^2 + \mathrm{D}k^2 + \mathrm{E}bk + \mathrm{F}\mathbf{Y}k + \mathrm{J}b + \mathrm{M} \qquad [3.6]$$

Again, I wish to focus attention on the *form* of the solution and in particular on the question of whether or not the surface [3.3b] (or [3.5]) describes the solution of the model. I stress that I am concerned only with a methodological problem: Can a second-degree surface be found that is compatible with [3.3a], i.e. a surface which satisfies both [3.3a] and [3.6]? Since there are six free coefficients, it should be easy to find such a surface. Thus the Popper-dimension of this model is six because it is not *necessarily* compatible with *any* seven coefficients. Specifically, it would take a basic statement consisting of seven observations of **Y** *and* **C** in order to refute the solution – each observation yields a *b* and a *k* which must satisfy both [3.1] and [3.2] as well as [3.6] such that:

$$0 = \mathbf{Y}^2\mathrm{A} + (\mathbf{Y}/\mathbf{C})^2\mathrm{B} + (\mathbf{Y} - \mathbf{C})^2\mathrm{D} + (\mathbf{Y}/\mathbf{C})(\mathbf{Y} - \mathbf{C})\mathrm{E} + \mathbf{Y}(\mathbf{Y} - \mathbf{C})\mathrm{F} + (\mathbf{Y}/\mathbf{C})\mathrm{J} + \mathrm{M}$$

In other words, although it may be possible to find coefficients of equation [3.6] which render [3.6] compatible with six observations or points, it is not *necessarily* possible to find a second-degree surface which will ‘pass through’ any arbitrary seven ‘points’ [see also Samuelson 1947-8, pp. 88-90].

In this example, the Popper-dimension can be determined by observing the general form of the surface because the number of terms are relatively few and the surface is in only 3-space. In general, the Popper-dimension of a solution equation depends on three properties of that equation: its degree, the number of unknowns implicit in it, and the number of terms in it. If we let the degree be d and the number of unknowns be w (hence our surface is in ‘w-space’), we can determine the number of terms, ***N***, that would be free to be chosen in the complete general form equation of the solution surface as follows:

$$\boldsymbol{N} + 1 = (\mathsf{d}+1)(\mathsf{d}+2)(\mathsf{d}+3)\ldots(\mathsf{d}+\mathsf{w}) / \mathsf{w}! \qquad [3.7]$$

or in symbolic notation:

$$\boldsymbol{N} + 1 = \mathsf{S}_1\ \mathsf{S}_2\ \mathsf{S}_3 \ldots \mathsf{S}_{\mathsf{w}}\ r \qquad [3.7a]$$

where the summation S is over $r = 1$ to $r = \mathsf{d}+1$. This is a generalization which follows by means of mathematical induction such that letting $\mathsf{w} = 1$ yields:

$$0 = \mathrm{A}_1 + \mathrm{A}_2x^1 + \mathrm{A}_3x^2 + \ldots + \mathrm{A}_{\mathsf{d}+1}x^{\mathsf{d}}$$

When $w = 1$ the list of indices will be the following series: $(1,2,3,4,5,\ldots,d+1)$. And letting $w = 2$ will yield the following:

$$\begin{aligned} 0 = \ & A_1 + \\ & A_2 x_1 + A_3 x_2 + \\ & A_4 x_1 x_2 + A_5 x_1 + A_6 x_2 + \\ & A_7 x_1 x_2 + A_8 x_1 x_2 + A_9 x_1 + A_{10} x_2 + \\ & A_{11} x_1 x_2 + \ldots \qquad + A_{15} x_2 + \\ & \text{etc.} \end{aligned}$$

Note here that the list of the indices for the last coefficient in each line is the following series (1,3,6,10,15, etc.). Also note that each term of this series can be further analysed: $1 = 1$, $3 = 1+2$; $6 = 1+2+3$; $10 = 1+2+3+4$; $15 = 1+2+3+4+5$; or generally, $S_1 S_2 r$. Thus by mathematical induction, equation [3.7a] can be obtained.

1.1. More testable is not always more falsifiable

It could be inductively concluded that [3.7a] is equivalent to [3.7] by substituting various values for d and w and obtaining the same values for $\boldsymbol{N}$ [see Salmon 1928, Woods 1961]. This possibility raises two problems that require some adjustment to the concept of the Popper-dimension of a theory if it is going to be applied to specific economic models.

First, if we let the number of terms given by the solution equation be $\boldsymbol{t}$ then the Popper-dimension equals $(\boldsymbol{N} - \boldsymbol{t})$. This consideration requires no adjustment since it seems to correspond to what Popper had in mind when he compared the testability of circles and conics as noted in Chapter 2. To illustrate, consider Model 3 (p. 66). The degree of [3.3a] is 2. The number of unknowns in [3.3a] is 3 hence the model is in 3-space and the number of terms given in [3.3a] is 3. Thus using [3.7] yields:

$$\boldsymbol{N} + 1 = (2+1)(2+2)(2+3)/(1 \cdot 2 \cdot 3) = 10 = 9 + 1$$

Therefore $\boldsymbol{N} = 9$, but the Popper-dimension of Model 3 with respect to $\mathbf{Y}$ is 6, i.e. $(9 - 3)$. Note that the Popper-dimension of Model 3 with respect to $\mathbf{C}$ is also 6.

Second, by expressing Popper's idea of the field of application as being the list of parameters and variables, we see how Popper's view that equates the degree of testability and the degree of falsifiability may lead to apparent contradictions. In Chapter 2, I noted that a theory which explained more variables was, in Popper's eyes, more falsifiable because it was more at risk of being refuted. Here I am saying that since it raises the Popper-dimension of the theory, more variables means less testable. Actually by more variables Popper was referring to a greater domain of application for the same list of variables, such as going from prices of all agricultural products to prices of all products. Here we are talking about changing the list of variables and we are concerned with the practicalities of defining a successful test. Since in economics we are typically not concerned with the question of a domain of application for a variable, rather than entering into a technical argument over whether my definition of a Popper-dimension of a theory accurately represents what Popper had in mind, I will henceforth call it the 'P-dimension'. Here it will always be the case that the testability of an explicit model goes down (because the P-dimension goes up) whenever the number of variables or the maximum degree of the equations increases *ceteris paribus*. Whether a more testable explicit model is necessarily more falsifiable will not be a concern here since the question of more or less testability better represents the methodological concern of modern economists who define a successful test as a refutation (e.g. Samuelson and the followers of his methodology).

1.2. The P-dimension as a measure of testability

Now consider Model 1, previously discussed in Chapter 2 (p. 55), which differs from Model 3 in that the former includes 'autonomous consumption'.

Model 1:

$$\mathbf{Y} = \mathbf{C} + k \qquad [3.8]$$

$$\mathbf{C} = a + b\mathbf{Y} \qquad [3.9]$$

where a, b, and k are considered exogenous variables.

First, let us determine this model's P-dimension. The solution for $\mathbf{Y}$ is implicit in the following:

$$\mathbf{Y} - b\mathbf{Y} - a - k = 0 \qquad [3.10]$$

As before I shall interpret [3.10] to represent a surface in 4-space:

$$F(\mathbf{Y}, a, b, k) = 0 \qquad [3.11]$$

Note that the degree of [3.10] (and hence [3.11]) is 2 and the number of terms in the algebraic representation of [3.11] is:

$$\boldsymbol{N} = [(3 \cdot 4 \cdot 5 \cdot 6)/(1 \cdot 2 \cdot 3 \cdot 4)] - 1 = 14$$

Since [3.10] gives us four coefficients, the P-dimension of Model 1 with respect to **Y** is 14 – 4 or 10. Here again the P-dimension with respect to **C** is the same as the P-dimension with respect to **Y**. It can be seen directly that Model 1 results from adding a single autonomous term to Model 3, thus Model 1 is a little more difficult to falsify.

Let us attempt to reduce the P-dimension of Model 3 even further by considering five cases. First, for Case 1, consider a 'material' reduction, i.e. specifying that the solution surfaces must pass through a particular pair of values for **Y** and **C** (without first specifying the values of *b* or *k*, since we do not know them). For example, we may know that this year's GNP is 100 and the level of consumption is 80, thus we can expect the surface to pass through $\mathbf{Y} = 100$ and $\mathbf{C} = 80$. Therefore, using [3.3a] and [3.4] yields:

$$0 = 100 - b100 - k$$
$$0 = 80 - b80 - bk$$

Hence at $\mathbf{Y} = 100$ and $\mathbf{C} = 80$ we have $b = 0.8$ and $k = 20$. I stress here that these values for *b* and *k* may only hold at $\mathbf{Y} = 100$ and $\mathbf{C} = 80$. So far we do not *know* anything about *b* and *k* – i.e. they are being considered *exogenous* variables. Now using equation [3.6] with these values of **Y**, *b* and *k*, yields:

$$\mathrm{A}(100)^2 + \mathrm{B}(0.8)^2 + \mathrm{D}(20)^2 + \mathrm{E}(16) + \mathrm{F}(2000) + \mathrm{J}(0.8) + \mathrm{M} = 0 \qquad [3.12]$$

Equation [3.12] can be used to eliminate one of the coefficients – i.e. solve for one coefficient in terms of the others. This means that there is now one less free coefficient, thus the P-dimension falls to five.

For Case 2, let us add to the model the common assumption that *b* remains the same for all observations although we still do not *know* its value. Constant parameters are almost always presumed in economic theories and my discussion already shows that such an extra assumption is not always necessary for testability. Since I am concerned exclusively with the *form* of models, in subsequent discussions remember that parameters (such as *b*) are never presumed to be fixed over all observations. However, for the purpose of discussing Case 2, we do assume *b* is fixed and thus the solution surface in terms of endogenous and exogenous variables becomes:

$$\mathsf{F}(\mathbf{Y},k) = 0 \qquad [3.13]$$

that is, a 'surface' in 2-space. The general form is:

$$\mathrm{A}\mathbf{Y} + \mathrm{B}k + \mathrm{D} = 0 \qquad [3.14]$$

Since the solution [3.3] can be expressed as:

$$\mathbf{Y} - [1/(1-b)]k = 0 \qquad [3.3c]$$

we have values for two coefficients of equation [3.14] – the value for A and an expression of unknown value for B. This leaves D free to be chosen, thus we conclude that the P-dimension is now 1.

For Case 3, let us consider the additional assumption that $0 < b < 1$. This assumption can be combined with either Case 1 or Case 2. In each case the P-dimension will not change, however on the basis of Popper's subclass relation described in Chapter 2, we would say that this additional assumption makes the model 'more falsifiable' since it prohibits certain solutions. Nevertheless, it does not change the P-dimension!

Finally, for Case 4, let us consider an additional assumption that *b* is a particular value. For example, let us assume that $b = 0.5$. Thus equation [3.3c] becomes:

$$\mathrm{Y} - 0.5k = 0 \qquad [3.3d]$$

Therefore, we have values for both free coefficients of [3.14]. Hence the P-dimension is now zero and thus its testability is now maximum. This means that whenever this model is false, one observation is sufficient to refute the model. For example, the observation from Case 1 is not compatible with [3.3d] and hence if it was observed that $\mathbf{Y} = 100$ and $\mathbf{C} = 80$, the model would be falsified or refuted. By my formula, since $\mathsf{d} = 1$, $\mathsf{w} = 2$ and [3.3d] yields two terms (i.e. $\boldsymbol{t} = 2$), the P-dimension can be determined as follows:

$$\boldsymbol{N} = [(2 \cdot 3)/(1 \cdot 2)] - 1 = 2$$

hence the P-dimension equals (2–2) = 0.

Now let us turn to Model 2, which was also discussed in Chapter 2, but here I eliminate the identity [2.9]. As modified, Model 2 uses the following endogenous variables:

Y ″ aggregate spending (i.e. GNP),
C ″ aggregate consumption spending,
K ″ aggregate spending on new capital.

Model 2

$$\mathbf{Y} = \mathbf{C} + \mathbf{K} \qquad [3.15]$$
$$\mathbf{C} = a + b\mathbf{Y} \qquad [3.16]$$
$$\mathbf{K} = d/r \qquad [3.17]$$

where *a*, *b*, *d* and *r* are considered exogenous variables.

The solutions for **Y** and **C** are implicit in the two following equations:

$$r\mathbf{Y} - br\mathbf{Y} - ra - d = 0 \qquad [3.18]$$
$$r\mathbf{C} - br\mathbf{C} - bd - ar = 0 \qquad [3.19]$$

Equation [3.18] represents the surface:

$$\mathsf{F}(\mathbf{Y},a,b,d,r) = 0 \qquad [3.20]$$

and likewise, [3.19] represents:

$$\mathsf{F}(\mathbf{C},a,b,d,r) = 0 \qquad [3.21]$$

These surfaces are in 5-space, their degree is 3, and for both there are four coefficients determined by equations [3.18] and [3.19] respectively. Thus the P-dimension can be calculated with respect to **Y** or **C** as follows:

$$\boldsymbol{N} = [(4 \cdot 5 \cdot 6 \cdot 7 \cdot 8)/(1 \cdot 2 \cdot 3 \cdot 4 \cdot 5)] - 1 = 55$$

Hence the P-dimension is 55–4 = 51. However, the P-dimension with respect to **K** is not 51. Equation [3.17] itself is the solution for **K** and represents the surface:

$$\mathsf{F}(\mathbf{K},d,r) = 0 \qquad [3.22]$$

Thus for **K**, the surface is in 3-space, and according to [3.17] the degree is 2. Two coefficients are thus determined and the P-dimension is calculated with respect to **K** as follows:

$$\boldsymbol{N} = [(3 \cdot 4 \cdot 5)/(1 \cdot 2 \cdot 3)] - 1 = 9$$

hence the P-dimension is 9 – 2 = 7.

As I stated earlier, a theory (or model) as a whole is falsified when any statement deduced from it is falsified. Thus it can be concluded that the P-dimension of a model as a whole is the minimum P-dimension for each of its solution surfaces. Thus the P-dimension for Model 2 is 7, which is the P-dimension with respect to **K**.

It is interesting to note that not only is Model 2 considered ‘better’ than Model 1 by Popper’s subclass relation, but also that the P-dimension is lower. Thus, judged by both criteria it can be concluded that Model 2 is ‘better’.

2. Examples of the P-dimension in economic models

Let us form Model 4 by altering Model 2 somewhat by changing [3.17] to make the determination of **K** more interrelated as follows:

Model 4

$$\mathbf{Y} = \mathbf{C} + \mathbf{K} \qquad [3.23]$$
$$\mathbf{C} = a + b\mathbf{Y} \qquad [3.24]$$
$$\mathbf{K} = d/r + e\mathbf{Y} \qquad [3.25]$$

which merely adds another exogenous parametric variable, *e*, to Model 2.

The solution for **Y** is implicit in the following:

$$\mathrm{r}\mathbf{Y} - \mathrm{br}\mathbf{Y} - \mathrm{er}\mathbf{Y} - \mathrm{ar} - \mathrm{d} = 0 \qquad [3.26]$$

This represents a surface in 6-space of degree 3:

$$\mathsf{F}(\mathbf{Y},a,b,d,e,r) = 0 \qquad [3.27]$$

Equation [3.26] gives us 5 terms or coefficients, thus $\boldsymbol{N} = 83$ and the P-dimension with respect to **Y** is 78.

The solution for **C** is implicit in:

$$r\mathbf{C} - br\mathbf{C} - er\mathbf{C} - ar - aer - bd = 0 \qquad [3.28]$$

which represents:

$$\mathsf{F}(\mathbf{C},a,b,d,e,r) = 0 \qquad [3.29]$$

Since this is also in 6-space with degree 3 and [3.28] gives us 6 terms, the P-dimension with respect to **C** is 77. Likewise, the solution for **K** is implicit in:

$$r\mathbf{K} - br\mathbf{K} - er\mathbf{K} - d + bd + aer = 0 \qquad [3.30]$$

which represents a surface of the same *form* as [3.29] thus the P-dimension with respect to **K** is also 77. I would conclude then that the P-dimension for the entire model is 77.

Next let us form Model 5 by altering Model 4 with a change in [3.24] as follows.

Model 5

$$\mathbf{Y} = \mathbf{C} + \mathbf{K} \qquad [3.31]$$
$$\mathbf{C} = a + b\mathbf{Y} + f\mathbf{K} \qquad [3.32]$$
$$\mathbf{K} = d/r + e\mathbf{Y} \qquad [3.33]$$

which adds still another exogenous parametric variable, *f*.

The solution for **Y** is an equation with 7 terms whose highest degree is 4 and is in 7-space. Thus the P-dimension with respect to **Y** is 322. The solution for **C** is also in 7-space with degree 4 but has 12 terms, thus the P-dimension with respect to **C** is 317. And the solution for **K** yields 9 terms, thus the P-dimension with respect to **K** is 320. Thus I conclude that the P-dimension for Model 5 as a whole is 317.

Note how the interdependence of the relations of a model affects the P-dimension of the entire model. This leads to a consideration of one more alternative which is obtained by modifying Model 5 in a similar manner.

Model 6

$$\mathbf{Y} = \mathbf{C} + \mathbf{K} \qquad [3.34]$$
$$\mathbf{C} = a + b\mathbf{Y} + f\mathbf{K} \qquad [3.35]$$
$$\mathbf{K} = d/r + e\mathbf{Y} + g\mathbf{C} \qquad [3.36]$$

The implicit solution for **C** is a surface in 8-space:

$$\mathsf{F}(\mathbf{C},a,b,d,e,f,g,r) = 0 \qquad [3.37]$$

The solution yields 10 terms and is of degree 4. Thus the P-dimension with respect to **C** is 484. Similarly, the solutions for **K** or **Y** yield the same P-dimension. Thus the P-dimension for Model 6 as a whole is 484.

Comparing Models 2, 4, 5 and 6 shows how the ‘degree of inter-dependence’ influences the P-dimension of a theory (or model). While the P-dimension of the rather simple Model 2 is 7, the P-dimension of the more complex Model 6 is 484. Consequently, there is a 65-fold increase in the number of observations necessary to form a sufficient falsification. To obtain some perspective on these numbers, let us consider how long it would take to make enough observations to construct a counter-example. If it takes one whole day to generate a new observation of all the variables in Model 6, then it would take over a year to construct a falsification!

Perhaps the six models considered so far are rather general if not outright dull. To spice things up a little, let us examine three of the many models developed by Jan Tinbergen [1956/67, appendix 3]. I have simplified them by eliminating the identities (which are definitions and hence contribute nothing of interest). These models will draw from the following list of variables:

Y ″ aggregate spending (i.e. GNP),
P ″ price level (index),
W ″ wage bill,
N ″ level of employment,
C ″ aggregate consumption spending,
T ″ total tax receipts.

Model 7 (closed, static, macro, money and product flow model)

$$\mathbf{Y} = a\mathbf{P} + b\mathbf{Y} \qquad [3.38]$$
$$\mathbf{P} = \mathsf{a} + \mathsf{b}(\mathbf{Y}/\mathbf{P}) \qquad [3.39]$$

where *a*, *b*, a and b are considered exogenous parametric variables [Tinbergen 1956/67, p. 231].

The unusual aspect of this model is that it involves fixed prices. The P-dimension with respect to **Y** is 49, and with respect to **P** it is 15. So this model’s P-dimension is 15.

Model 8 (closed, static, macro, money, product and factor flow model)

$$\mathbf{Y} = a + b\mathbf{Y} + c\mathbf{W} \qquad [3.40]$$
$$\mathbf{W} = w\mathbf{N} \qquad [3.41]$$
$$\mathbf{N} = m + n\mathbf{Y} \qquad [3.42]$$

where *a*, *b*, *c*, *m*, *n* and *w* are considered exogenous parametric variables [p. 232].

For this model, the P-dimension with respect to **W** is 783, with respect to **Y** it is 324, and with respect to **N** it is 321.

Model 9 (closed, static, macro, money flow and public finance model)

$$\mathbf{Y} = \mathbf{C} + g \qquad [3.43]$$
$$\mathbf{C} = a + b(\mathbf{Y} - \mathbf{T}) \qquad [3.44]$$
$$\mathbf{T} = r + s\mathbf{Y} \qquad [3.45]$$

where *a*, *b*, *g*, *r* and *s* are considered exogenous parametric variables [p. 233].

For this model the P-dimension with respect to **Y** is 77, with respect to **C** it is 76, and with respect to **T** it is also 76.

There is not a lot that can be done to compare these three models since their respective fields of application vary widely. The P-dimension of Model 8, as well as Model 6, may seem large but as we shall see subsequently when we examine some more involved models these dimensions are not very high. It should be stressed that the P-dimension of a theory or model can be calculated only when all the relations are in an explicit form rather than an abstract form such as $\mathbf{C} = f(\mathbf{Y})$. In fact, it could be argued that the P-dimension of a model involving abstract functions with no restrictions should be considered infinite (I will return to this consideration in Chapter 7).

Let us consider now a model which is usually presented with only abstract functions, but this time we will use explicit relations. (The model is a version of a Keynesian model originally presented by my thesis supervisor, Hans Brems [1959, pp. 34-47] but in which I have eliminated the one identity.) The endogenous variables for this model are as follows:

$\mathbf{Y}$ ″ aggregate net spending,
$\mathbf{C}$ ″ aggregate consumption spending,
$\mathbf{I}$ ″ aggregate net investment spending,
$\mathbf{L}_T$ ″ aggregate volume of ‘transactions’ cash balances,
$\mathbf{L}_L$ ″ aggregate volume of ‘assets’ cash balances,
$\mathbf{R}$ ″ the market interest rate.

Model 10

$$\mathbf{Y} = \mathbf{C} + \mathbf{I} \qquad [3.46]$$
$$\mathbf{C} = a + b\mathbf{Y} \qquad [3.47]$$
$$\mathbf{I}^2 = c - e\mathbf{R}^2 \qquad [3.48]$$
$$\mathbf{L}_T = f\mathbf{Y} \qquad [3.49]$$
$$\mathbf{L}_L = g/(\mathbf{R} - h) \qquad [3.50]$$
$$m = \mathbf{L}_T + \mathbf{L}_L \qquad [3.51]$$

According to Brems [1959, p. 41], the solution of this model for **Y** is a fourth-degree equation *in* **Y**. The solution represents the following surface:

$$F(\mathbf{Y},a,b,c,e,f,g,h,m) = 0 \qquad [3.52]$$

The leading term in his solution [p. 41] is $b^2f^2\mathbf{Y}^4$, therefore the degree of the solution surface is 8. The solution has 27 terms and the surface is in 9-space. Thus the P-dimension with respect to **Y** is 24,282. I stress that this means that if the model is false, it will take 24,283 observations to refute it whenever we do not know the values of the parameters. Again, if it would take one entire day to generate a complete set of observations (i.e. one for each variable) then it would take over 66 years to construct a falsification. Stated another way, to be able to construct a falsification in one year would require the assembly of a complete set of observations every 21 minutes!

Note that the relations which contribute most to the high P-dimension are the equations [3.48] and [3.50]. Thus let us consider a couple of ways of altering them in order to reduce the P-dimension. First, let us change [3.48] to the following:

$$\mathbf{I} = c - e\mathbf{R} \qquad [3.48a]$$

This change causes the degree of equation [3.52] to fall to 4 and the number of terms to fall to 11. The P-dimension with respect to **Y** in turn falls to 703.

To go further, in addition to equation [3.48a], let us substitute a different relation for [3.50] (unfortunately this deviates considerably from the Keynesian ‘liquidity preference’ relation that Brems wished to model):

$$\mathbf{L}_L = g - h\mathbf{R} \qquad [3.50a]$$

With these two substitutions the P-dimension with respect to **Y** falls to 212.

Again, this shows how the *form* of the relations contribute to the P-dimension of a model. And again, while the P-dimension of Model 10 even before the modification seems rather high, I will subsequently show a model with an even higher dimension yet with fewer endogenous variables.

Consider a model which uses the well-known and popular Cobb-Douglas production function. We will see that one difficulty with models that use this function is that their P-dimensions become a function of the true (but unknown) value of one of their parameters.

Model 11

$$\mathbf{X} = a\mathbf{N}^{\lambda}k^{1-\lambda}, \qquad 0<\lambda<1 \qquad [3.53]$$

$$\mathbf{N} = b\,(\mathbf{W}/p), \qquad b>0,\ p>0 \qquad [3.54]$$

$$\partial\mathbf{X}/\partial\mathbf{N} = \mathbf{W}/p \qquad [3.55]$$

$$\mathbf{Y} = p\mathbf{X}, \qquad \text{(a definition)} \qquad [3.56]$$

where a, b, p and λ are parameters and thus the only endogenous variables are **X** (the level of real output), **N** (the level of employment), **W** (the money wage rate), and **Y** (the dollar value of the output).

The solution for Y is implicit in the following:

$$\mathbf{Y}^{2-\lambda} - a^2 b^{\lambda} p^{2-\lambda} \lambda^{\lambda} k^{2-2\lambda} = 0 \qquad [3.57]$$

Now the 'degree' of this relation turns out to vary with λ. To facilitate the analysis let me assume that λ is a rational number, i.e. $\lambda = r/q$. Hence, the degree of [3.57] is $6q - r$. The P-dimension can thus be estimated as follows:

$$[(6q-r+1)(6q-r+2)\ \dots\ (6q-r+6)\,/\,6!] - 3 \qquad [3.58]$$

since [3.57] is in 6-space and it has two terms.

The P-dimension with respect to **Y** can be calculated for several values of λ using expression [3.58] for each value.

Let us begin with an extreme case. If $\lambda = 0$ when $r = 0$ and $q = 1$, then the degree is 6. By expression [3.58] the P-dimension with respect to **Y** is:

$$[(7 \cdot 8 \cdot 9 \cdot 10 \cdot 11 \cdot 12)/(1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6)] - 3 = 921$$

The other extreme case is $\lambda = 1$, and thus letting $r = 1$ and $q = 1$ yields a degree of 5. Therefore, in a similar manner, the calculated P-dimension is 459.

In the following five cases I will simply list the results. The cases are more reasonable values for λ if we are to maintain the form of [3.53].

if $\lambda = 1/2$, the degree is 11 and the P-dimension is 12,373,
if $\lambda = 2/3$, the degree is 16 and the P-dimension is 74,610,
if $\lambda = 1/3$, the degree is 17 and the P-dimension is 100,944,
if $\lambda = 3/4$, the degree is 21 and the P-dimension is 296,007,
if $\lambda = 1/4$, the degree is 23 and the P-dimension is 475,017.

Generally I note that the degree varies mostly with the denominator q. It may be concluded that the more complicated the rational number used to approximate λ, the less falsifiable is the solution. I also note that although when $\lambda = 3/4$ or $\lambda = 2/3$ is used to calculate **Y**, the results do not differ very much, however, the P-dimension does differ greatly.

I think I have shown enough examples to illustrate that some methodological decisions that model builders might make for the convenience of their mathematical analysis can lead to extremely unfortunate methodological consequences whenever one also is concerned with the requirement of testability. Of course, it will be noted that my discussion has been concerned only with explicit non-stochastic equilibrium models and thus may have a limited applicability. I would agree that my discussion and my concept of a P-dimension is seen in the clearest light in terms of these types of models, but it can be noted also that stochastic econometric models are typically constructed from similar non-stochastic equilibrium models. The degree of testability of any econometric model is not independent of the P-dimension of its underlying exact model. Such an interdependence, I claim, is implicit in the econometrician's consideration of the so-called 'Identification Problem'.

3. The identification problem and the P-dimension

Econometricians have long recognized that the problem of identification is logically prior to the problem of estimation of the parameters of a model [e.g. Johnston 1963, Goldberger 1964, Fisher 1966]. One of the purposes of this chapter is to support the claim that the methodological problem concerning truth status, which is connected with our discussion of the P-dimension, is logically prior to the problem of identification. There might appear to be an obvious objection to the relevance of this claim – namely that the econometrician is concerned with stochastic models and I have been dealing with only non-stochastic models so far. It can, however, be pointed out that the problem of identification exists quite apart from the stochastic nature of econometric models [Johnston 1963, p. 243].

In order to support adequately my claim, I will first outline the problem of identification since today it is too often taken for granted. To discuss this problem, I have at my disposal the well-developed language of the econometrician. So far I have chosen to avoid using such terminology so as to avoid suggesting an econometric methodology. Here may be a good place to start, so I note that the methodology of econometric model building is concerned with the following concepts: a

structure, a model, and a property called ‘identification’. By a structure (of a non-stochastic model) I mean ‘a specific set of structural equations’ such as is obtained by giving specific numerical values to the parameters of a model. By a (non-stochastic) model I mean ‘only a specification of the form of the structural equations (for instance, their linearity and a designation of the variables occurring in each equation).… More abstractly, a model can be defined as a set of structures’ [Koopmans 1953, p. 29]. Identification refers to the property of a specific model which assures that, if the model is posited as being the hypothetical ‘generator’ of the observed data, a *unique* structure can be deduced (or identified) from the observed data. By hypothetical generator I mean that if one is given the true values of the parameters then whenever the observed values of the exogenous variables are put into the model, the resulting values for the endogenous variables are said to be ‘generated’ by the structure of the model.

There are two ways in which a model may fail to possess the identification property. Either the model is such that no structure can be deduced, or the model is such that more than one structure can be deduced from the same data. Attempting to avoid the possibility of these difficulties is called the ‘problem of identification’.

First I wish to pursue the significance of the claim made by many econometricians that the problem of identification is logically prior to the estimation problem; that it would exist even if our samples were infinitely large; and that it would exist even with non-stochastic models. The task of the econometrician is to determine the particular structure of a specified model (usually a linear model) which would generate the given data. A possible methodological problem, usually called the ‘problem of estimation’, is that the data given is stochastic and hence the structure cannot be exactly determined. But before the structure (i.e. the parameters) of a model can be estimated, the form of the model must have been specified such that the problem of identification is avoided. Thus we can see that the problem of identification is ‘logically prior to the estimation problem’. I note further here that the consideration of the property of identification implies (or is predicated on the assumption) that the model in question is known (or assumed) to be true. Hence the solution statements are true statements, and although there exists a finite P-dimension, no set of observations could ever be found which would contradict the model.

It is this ‘assumed truth status’ of the form which is the moot point of this section. This is the epistemological problem concerning truth status that I mentioned at the beginning of Chapter 2. Since the form of the model must be assumed to be true before the problem of identification is considered, it can be concluded that the epistemological problem concerning truth status (the truth status of the form of a model) is logically prior to the problem of identification.

Note that the solution to the identification problem amounts to the avoidance of ‘generally’ (in an algebraic sense) in a solution statement of a model. A solution is general when it remains invariant under transformations of the coordinates (i.e. of the model). Thus the uniqueness property of identification implies a lack of invariance.

Identifiability is dependent upon the form of the model. As most texts on econometrics indicate, what can determine the identifiability of a model is the relationship between the number of endogenous variables and the number of exogenous variables. Avoidance of the identification problem requires that the relationship be of a nature which will assure us that with a finite number of observations we can (if the model is true) deduce the values of the parameters (i.e. the structure). This requirement holds even for non-stochastic models. If for a particular model this requirement were not met then it may be possible that even if both the number of observations were infinite (or unlimited) and the model were true, we still could not deduce the unique structure of the model (i.e. distinguish between the possible sets of parameter values). The finite number of observations that are necessary to deduce a structure of a non-stochastic model is analogous to the concept which I have called the P-dimension.

Although in this chapter the discussion has been limited to non-stochastic models (hence avoiding the problem of estimation) it was not limited to only directly or indirectly linear models (as would most econometric methodology). The discussion applies to all linear and non-linear models or relations between the endogenous and exogenous variables, although the specific formula for calculating the P-dimension has only been worked out for polynomial solutions.

One of the implications of the priority of the methodological problem concerning truth status over the identification problem is that econometric studies are not substitutes for research in pure theory. Clearly, econometrics is very useful for practical applications of

economic theories, particularly in the area of economic planning and forecasting. Many economists unfortunately confuse the sophistication of the statistical theory of econometrics with the sophistication of the economic theory upon which the econometric model is based. The fact is that the economic theory used in econometric studies is usually very primitive. If progress is to be made in pure theory, it will be more through our efforts to deal with the methodological problem concerning truth status than the problems of econometrics.

4. Concluding remarks about the P-dimension

The most important statement to be deduced from a model is the solution. The solution, or its form, specifies that the endogenous variables are related to the parameters and exogenous variables in a specific manner. Models which differ will specify different relations – i.e. different solutions. Methodologically speaking, theories can be compared by comparing the form of the solution statements deduced from their representative models. I have suggested that the operative comparative question is: What is required to show that the *form* of a model (indicated by the solution) is false?

If the solution statement is false, then in principle (or conceivably) we can show the solution to be false. Using the P-dimension criterion one can state exactly what it takes to refute the solution statement (i.e. the minimum but sufficient number of observations). If the solution statement is true, a similar quantitative criterion *cannot* be established to specify the finite number of observations which would be sufficient to *assure* that whenever the values of the parameters are unknown, the solution statement is indeed true. One cannot distinguish between false or true solution statements on the basis of their form if the number of observations is less than the P-dimension.

From the illustrations presented earlier in this chapter, it is easy to see that the two factors which most influence the P-dimension are the degree of, and the number of variables or parameters in, the solution equation. The degree is influenced by the form of every relation in the model and the ‘degree of interdependence’ of the relations in the model. The ‘degree of interdependence’ also influences the number of parameters since as variables were added to a relation we usually added extra parameters (see the discussion of Models 2, 4, 5 and 6 on pp. 72-4). I suggest that what is usually called ‘simplicity’ might be interpreted in terms of my illustrations [see also Popper 1959/61, Ch. 7]. Certainly, reducing the degree of interdependence makes the model ‘more simple’. Likewise, the paucity of parameters is usually an indication of simplicity. I say ‘usually’ because, as Model 11 (p. 78) illustrates, this is not necessarily the case.

In this sense the desire for simplicity can be translated into a desire for higher degrees of testability or falsifiability. I would like to be able to make such a translation of the concept of algebraic or mathematical simplicity of the forms of the relations of a model but as can be seen with Model 11, although the ‘form’ does not change with changing values of the exponents of equation [3.53] the P-dimension (and hence falsifiability) can change. Therefore one cannot always be sure that the form alone indicates anything. However, the desire for pragmatic simplicity (i.e. the ease with which one can solve for the endogenous variables) can be translated into the desire for lower P-dimensions. As was seen in the case of Model 10 (p. 76), by reducing the degree of the solution equation, the equation is made easier to solve and at the same time more falsifiable.

It might be argued that the proposed criteria (i.e. Popper’s subclass relation and my P-dimension) is useful only to compare *comparable* models rather than evaluate a single model. One answer to this objection is that although I did not (and probably cannot) develop an absolute criterion using the P-dimension concept, one does get the strong impression that a model with a P-dimension in the order of 100,000, or even 10,000, is approaching what some economists might call a ‘tautological’ model since it would be very difficult to refute such a model convincingly. Of course, if economists adopt the aim of science that is posited by the Popper-Socrates view, then the concepts of falsifiability and P-dimension are crucial. If, on the other hand, economists maintain that the aim of science is to provide verifiable theories and models [e.g. Machlup 1955, Rotwein 1959] then the necessity to investigate the P-dimension of their economic models and theories is less obvious. However, I noted at the beginning of Chapter 2 that verificationists may not wish to waste time on tautologies so it might be suggested that the P-dimension is important for the verificationists as well. Although a model with a P-dimension of 100,000 is in principle falsifiable, in some crude sense it is ‘more tautological’ than a model with a P-dimension of only 10.

In terms of the Popper-Socrates view of science, one can hardly *know* when one has made a mistake (i.e. when one has put forth a false theory) if it takes over 100,000 observations in order to show that the theory is false. This observation is even more significant when it is recognized that all of these models are non-stochastic and thus in principle refutable with a finite set of observations. Further consideration of the methodological problems introduced by stochasticism is postponed until Chapters 7 and 8.

Based on the discussion in Chapters 1 and 2 and on the illustrations of the present chapter, it might still be argued that the truth status of some non-stochastic models can in principle be determined. But unfortunately, it is only the falsity that can be demonstrated. However, the Popper-Socrates philosophy of science would have us being very pleased that at least some models are falsifiable and that we can potentially make progress in science by learning from our 'mistakes' – that is, from the refutations of our models and theories. Despite the lofty platitudes embodied in this optimistic philosophy, many economists may still see the worship of falsifiability as a misplaced desire for the hole instead of the donut!

In Chapters 4 and 5, I deal with how economists maintain a more positive interest in the truth status of models. While it is widely recognized that it is usually impossible to prove that a model or theory is true – even a non-stochastic model – economists still wish their theories to achieve some sort of truth status. In Chapter 4, I examine how theorists accommodate questions of the truth status of theories by employing more conventional standards than those embodied in the Popper-Socrates view of science. Of particular concern will be the nature of methodological disputes in the absence of a means of demonstrating the truth status of competing models or theories. In Chapter 5, I explain how an economist might view questions of theory choice when truth status cannot be directly demonstrated. Of particular concern will be the failure of conventional substitutes to overcome the problem of demonstrating the truth status of models (or theories) when they are true.

PART II

Popper-Samuelson Demarcation vs. the Truth Status of Models

4

Conventionalism and Economic Theory: Methodological Controversy in the 1960s

> If perfect competition is the best simple theory in town, that is no excuse for saying we should regard it as a good theory if it is not a good theory.… We must not impose a regularity – or approximate regularity – in the complex facts which is not there. Good science discerns regularities and simplicities that are there in reality … psychological usefulness should not be confused with empirical validity.
>
> Paul A. Samuelson [1963, p. 236]

> It would be highly desirable to have a more general theory than Marshall's.… The theory of imperfect or monopolistic competition developed by Chamberlin and Robinson is an attempt to construct such a more general theory. Unfortunately, it possesses none of the attributes that would make it a truly useful general theory.
>
> Milton Friedman [1953, p. 38]

While theorists and applied economists may dutifully assure themselves that their models are indeed falsifiable, the truth status of their models continues to present an ongoing methodological problem. The model builder must make decisions regarding three things: (1) the question to be answered by his or her model, (2) the list of variables and the specification which are endogenous and which are exogenous, and (3) how the truth status of the answers will be determined.

Within any model or theory an individual idea is usually represented by a verbal, or mathematical, statement. A statement is true only if it corresponds to facts, that is, only if there will never be a fact which contradicts it. Of course, for a model or theory to be true it is necessary, but not sufficient, that it be internally consistent. There is a popular school of thought in the philosophy of science which would equate truth status with internal consistency, since it would appear that truth status is only a matter of convention [see Agassi 1963, 1966a; Boland 1982, Ch. 1]. This view of science – often called 'conventionalism' – is a rather sophisticated version of an old theory of knowledge. The old theory – sometimes called 'empiricism' and other times 'inductivism' – said that knowledge is (or represents) the accumulation of empirical facts and as such is necessarily true. The newer conventionalism says that all knowledge is accumulated facts but true theoretical knowledge is impossible. Conventionalism says that truth status is at best a matter of convention because we can never know that a theory is true.

It is important to distinguish conventionalism from an equally popular doctrine that sounds similar – specifically, Milton Friedman's 'instrumentalism' [Friedman 1953, see also Boland 1979a]. Fortunately, it is easy to distinguish these two methodological doctrines. Conventionalism is concerned with the *status* of theories and models and instrumentalism is concerned with the *role* of theories and models. Where conventionalism asserts that theories are neither true nor false but only better or worse, instrumentalism asserts that if the theory or model works its truth status does not matter. According to instrumentalism, theories and models should be judged on the basis of whether or not they are useful. This distinction gets rather blurred in practice, since it is possible for someone to embrace both views. One can advocate conventionalism when asked about the truth status of a model and advocate instrumentalism when asked about the role of models in economic science. It is possible to advocate instrumentalism and reject conventionalism. However, most economists seem to embrace both. When they say Theory A is 'better' than Theory B, it is not always clear whether they mean Theory A is better as measured on some scale of truth-likeness or as measured on some scale of usefulness. When advocates of conventionalism argue methodology with followers of Friedman's instrumentalism, it is not always clear what the argument is about.

When I began publishing articles in economics journals in the 1960s, the number of methodologists in our profession was very small. Among this small group there was one remaining apriorist, the late Ludwig von Mises, and a few empiricists struggling against apriorism. Everyone else battled empiricism by endorsing some form of conventionalism and/or Friedman's instrumentalism. In 1970 I reported that the battle was over. Conventionalism and instrumentalism had won out over empiricism. The last remaining General for the empiricists, Eugene Rotwein, was by then pleading for toleration and 'pluralism' [Rotwein 1966]. Although the number of methodologists is now much larger and growing, things have not changed very much. Most methodological disputes still disgorge very little substance. What the victory over empiricism means for economic theory and the prospects for a continued occupation in the face of rising interest in the Popper-Socrates view of science will be the topic of this chapter.

The major outcome of the victory of conventionalism and instrumentalism over empiricism in the late 1960s was that methodological controversy in economics was reduced to nit-picking over which must come first – simplicity or generality. The popular 1960s controversy was a pseudo-argument. Both sides argued from the same methodological position, namely, conventionalism.

1. Robinsonian conventionalism

Except for a small group of economists whose credentials are probably suspect anyway, all economists can still be divided into two groups: those who say they agree with Milton Friedman's famous methodology essay [1953] and those who do not. Closer examination shows, however, that both groups espouse the conventionalist view which sees science as a series of approximations to a demonstrated accord with reality. Their dispute in fact is simply a conventionalist family disagreement over conventionalist criteria for judging theories. Namely, it is a dispute between the conservative followers of Friedman who advocate simplicity as the more important criterion for judging theories, and the would-be liberal conventionalists who argue in favour of generality as the more important criterion. The generalists are perhaps inspired by Paul Samuelson's views [1952, 1963, 1965] of economic methodology and his apparent success at demonstrating the logical validity of a proposition by generalizing it.

Being a beginner in the 1960s, I did not understand *why* simplicity or generality was considered desirable. Later I was to learn that the dispute was, more specifically, a dispute between those who wished to promote mathematical interest in economics vs those concerned with promoting the application of economics to real-world problems and phenomena [see also Grubel and Boland 1986]. Real-world applications of economics are almost always facilitated by simplifications. Those guided by the aesthetic tastes of mathematics departments were more interested in the generalization of economic theories. For example, rather than explaining the choice between two specific goods (apples vs bananas), we should explain the choice between *n* different goods. Unfortunately, I was misled by the political environment of the 1960s. I assumed that the conservative-liberal schism was a reflection of politics and ideology and thus would be expressed in terms of theoretical disputes between the so-called 'Chicago School' and more liberal views of theory such as those found in Cambridge (England or Massachusetts).

The dispute over methodology in the 1960s appeared to me to have its genesis in Edward Chamberlin's and Joan Robinson's attempts to modify the old Marshallian theory of the firm [Chamberlin 1933, Robinson 1933]. The question for the 'Robinsonian conventionalist', as I shall call the disputants, is the following: Should we (1) stick to the Marshallian 'perfect competition' model of the firm (or perhaps a 'pure monopoly' model or a 'mixture of the two' [Friedman 1953, p. 36] – whichever 'works'), or (2) adopt the more modern 'imperfect competition' model of the firm?

Perfect competition, of course, is characterized by very small independent firms in a very large open (i.e. free enterprise) market such that any one firm cannot affect the price for the good it produces. By contrast, imperfect competition is characterized by the ability of any firm to affect its price by varying the amount of its good it supplies in the market. One model is more simple, the other is more general.

I have called the parties of this dispute Robinsonian conventionalists because it was Joan Robinson's attempted modification which makes the dispute more clear-cut. Her efforts made it possible for us to understand better the old perfect competition theory of the firm.

2. Pareto optimality

Now, it is a well-known principle of welfare economics that if all markets were cases of perfect competition and all firms were to maximize their profit as well as everyone maximizing their personal satisfaction, then in the long run we would have achieved an economic optimum, often called a 'Pareto optimum' – namely, an optimum where no one can gain by any redistribution of resources between firms without someone else losing. Sometimes this is called a '*laissez-faire* optimum' because here everyone is given leave to independently pursue his or her own business whether it be profit or self-satisfaction.

The theory of the firm in this case is very simple. We ask: Why is the firm producing at its present level of output? We answer: Because, given the going prices in the market and its technical capabilities, that is the one level of output which maximizes its profit. And the truth status of this answer is determined merely by assuming perfect competition and that costs facing the firm are guided by diminishing marginal returns to all factors of production. That is all there is to that!

The appeal of this simple theory to the conservative economist should be obvious. It says, if we can satisfy the marginal conditions of maximization with perfect competition, the best of all possible worlds will be achieved both in terms of ends and of means. In terms of ends we have the Pareto welfare optimum (no one can gain without others losing) and in terms of means we have complete independence (i.e. classical Liberalism).

The case of imperfect competition, which everyone agrees is 'more realistic', is, however, much more complicated. It is more complicated merely because we cannot say 'given the going prices in the market' since the firm's decision affects its prices as well as its level of output. And so the prices are not 'given'. Instead, the answer would be: Given the behaviour of the prices in the market as well as the firm's technical capabilities, the firm produces the one level of output which maximizes its profit. Clearly, this is not much of a change in the theory, but to determine the truth status of this modified theory of the firm we must add assumptions about the behaviour of prices and the firm's ability to affect its prices, as well as assumptions about its technical cost constraints. There are many ways to do this, thus any way we choose is likely to be *ad hoc*. Moreover, the *ad hoc* conditions usually place more demands on our concept of the market than most economists admit [see Arrow 1959, Clower 1959, Richardson 1959, Boland 1986]. And to satisfy these demands we must make behavioural assumptions about factors outside the firm which increases both the complexities and the difficulties in deducing testable predictions. In mathematical terms, we can see that the complexities are created by allowing for the variability of prices. That is, we now have one more dependent variable, in addition to output level, which affects the decision criterion, i.e. profit. In terms of an old jargon, there has been an increase in the degrees of freedom.

3. Welfare implications of imperfect competition

Now what about the welfare implications of the modified theory? These turn out to be another problem. The modified theory does not necessarily lead in the long run to a Pareto optimum. Furthermore, it allows for interdependence between firms and customers. However, virtually everyone agrees that such non-optimality and such complexity is the nature of the 'real world'. For all its potential drawbacks, this theory of imperfect competition has, as a limiting case, the other theory – namely, the perfectly competitive theory of the firm. Thus imperfect competition is claimed to be 'more general'. The price of generality is the reduction of simplicity – there are additional variables and *ad hoc* assumptions to deal with.

4. The conventionalist controversy

I think I can now clinch my point that the 1960s methodological controversy did not amount to much. On the one side we have the pro-perfect competition theorists, the methodological conservatives who believe in simplicity, and on the other side we have the would-be methodological liberals who believe in generality. Surely the conservatives could never convince the pro-imperfect competition theorists of the virtue of simplicity by merely asserting that simplicity is more virtuous than generality. Such, however, would seem to be the case. When Friedman, in his famous 1953 essay on methodology, argues that greater generality of a set of assumptions does not matter if the alternative set of simple assumptions leads to positive results and predictions, he is merely reaffirming his methodological position [see Boland 1979a]. Moreover, he tells us that assumptions, and hence the results, are only approximations anyway, and thus we should stick to our perfect competition models of the firm because they are capable of providing more positive results [Friedman 1953, p. 38]. Followers of Friedman go on to argue that with the complex imperfect competition theory and its degrees of freedom and *ad hoc* conditions, it is difficult to come up with any results or predictions, positive or otherwise [see Stigler 1963, Archibald 1961].

After all this, what can be argued by the Robinsonian conventionalists who advocate generality and its embodiment in the idea of imperfect competition? While Samuelson says that the task of an economic theory of the firm is 'describing and summarizing empirical reality' [Samuelson 1952, 1967] he nevertheless argues that all 'scientists accept some degree of approximation' [Samuelson 1964] and thus he admits that all theories are approximations. And some followers of Friedman continue to say that imperfect competition is empty or arbitrary, and since it is only an approximation, we should be guided only by simplicity [e.g. Stigler 1963] and thereby see that the perfectly competitive theory is to be preferred.

Since both schools are thus immersed in the same kind of difficulty when they attempt to criticize either theory, the issue becomes which is a 'better approximation' – a simplifying approximation which gives more positive results, or a generalizing approximation which allows for a better description of what firms (in fact) do? From the standpoint of the Robinsonian conventionalists, it is not sufficient merely to assert that simplicity is more important than generality or vice versa – and so the conventionalist controversy must continue.

5. Limitations of approximating *laissez-faire*

I do not propose to resolve this methodological dispute here, mainly because I think the simple perfect competition theory of the firm is simply false and the imperfect competition theory is at best pointless. The perfect competition theory is false, if for no other reason, because above all it assumes that all firms are so small relative to the market that they individually cannot affect their prices. The imperfect competition theory is pointless because it is far from complete and unlikely to be completed as a 'partial equilibrium' theory of the firm [see Boland 1986]. So, I will turn to the broader methodological question of approximation underlying both positions in the controversy. In this regard I will try to draw an analogy between this question of approximation and the 1960s theory of approximate free trade (which was called the theory of the Second Best [see Meade 1955, Lipsey and Lancaster 1956-7, Mishan 1960 and the bibliography in Bohm 1967] and is about the possible outcomes of approximating the goals of any economic system).

Let me explain the theory of the Second Best in terms appropriate for the construction of my analogy. The 'first best' is the *laissez-faire* optimum where everyone is individually optimizing (i.e. maximizing utility or profit) and hence society as a whole is at an optimum. If the 'first best' cannot be reached because there is a constraining obstacle to satisfying all the optimizing conditions that are necessary for the achievement of a Pareto optimum (e.g. a constraint preventing the firm from setting its marginal cost to exactly equal its marginal revenue), then Second Best Theory says that (1) between the resulting 'constrained' outcome reached by approximating the completion of the optimizing conditions and the 'first best' outcome, there exists a 'second best' outcome, but (2) in order to reach the 'second best' outcome we must give up the method followed to reach the 'first best'.

For example, if for any reason some of the firms in the economy cannot satisfy all its marginal conditions for maximization because of some legal constraints (such as union constraints and/or price controls), the economy as a whole will not be at a Pareto optimum. That is, there would be the possibility that without the legal constraints someone could gain without anyone else losing. Now, if we require that all other firms (those not restricted by the legal constraints) must still satisfy the marginal conditions of (profit) maximization, then (it is argued) there will necessarily be the possibility of choosing some other set of possible decisions on the part of the non-restricted firms which will leave the economy as a whole better off than would be the case if they attempted to satisfy all the conditions for individual maximization of profit. The asserted existence of the possibility of improving over the approximation of economic optimum is the central idea of the 'theory of the Second Best' which asserts that approximation in this case is less than 'second best'. The argument supporting this theory, which unfortunately is somewhat mathematically involved, can be found in Lipsey and Lancaster [1956-7].

On the basis of the implications of this theory it turns out that one of the virtues of the perfect competition optimum was that it involved a unique method for reaching the optimum. That is, there is one and only one allocation of resources, and one and only one distribution of commodities between consumers and between producers. Giving up the perfect competition model (and its optimum) involves giving up the uniqueness of the choice regarding methods (criteria, conditions, etc.) for allocation and distribution. For this reason, McManus [1959] states a conventionalist argument that the approximated optimum is a desirable 'third best' because at least we know how to reach it while we only know of the possibility of the 'second best'.

The theory of the existence and method of the 'second best' arose in the 1950s when someone was considering traditional free trade arguments [see Meade 1955]. The issue then was the following dilemma: We all agree that if every country pursued a free trade policy (e.g. no tariffs or quotas) then we would have the best of all possible economic worlds. The question arose, if one country can gain by instituting tariffs (such as an import duty on all foreign-produced goods), what should other countries do? The free trade people maintained that the best we can do is approximate free trade. That is, every other country should not respond by also instituting tariffs, but should behave as if every country did not have tariffs. Now this argument is merely one of many possible theories, and Second Best Theory was offered as a counter-theory that there does exist a better way of trading although it will not be the 'first best' way.

6. Second best theory vs approximationism

At first it seemed curious to me that I could not find much in the writings of the Robinsonian conventionalists such as Friedman or Samuelson about the theory of the Second Best – particularly since the issue is approximation. I think the reason for this lacuna is that Robinsonian conventionalism and Second Best Theory are not compatible. To see this we have to raise the theory of the Second Best to a meta-theoretical level. Robinsonian conventionalism of either school (liberal or conservative) says that approximating an ideal or optimum (such as simplicity or generality) is the best we can do, and therefore is satisfactory. My meta-theoretical formulation of the well-known Second Best Theory is: There does exist something better than the approximation of the 'ideal' theory (i.e. of the exact representation of empirical reality) but to find it we are required to give up the old method (namely, Robinsonian conventionalism).

What are the implications of this meta-theory for the dispute over the theory of the firm? I have already noted that there are obstacles to the construction of competitive theories of the firm (one is false, the other is arbitrary) thus my second best meta-theory says that there must exist a better theory than the two discussed, but it must be something other than a mixture of the two or a modification of one of them.

7. The simplicity-generality trade-off

What will be the Robinsonian conventionalists' argument against my meta-theoretical formulation? It may be something of the form: Yes, there may be a better way, but the current version of conventionalism is the best we can do! And we begin an infinite regress. Or they may argue (as some followers of Friedman do) that the 'first best' has not been reached because there are obstacles to perfect competition and so, they say, get rid of the obstacles. In other words, they might argue that people and firms, in short the whole world, should behave in accordance with the 'ideal' theory in order that the theory will be true.

It is more likely that the methodological conservatives will argue that my meta-theoretical formulation is simply wrong because (1) Second Best Theory is about welfare with commodities and my second best meta-theory is about knowledge with theories, and (2) the perfect vs imperfect competition dispute does not have its analogue at the meta-theoretical level. In response to such an argument I would have to say that it is much easier for conventionalists to see an analogy between welfare and knowledge and between commodities and theories than it would for other methodologists. This is because the conventionalists' approach to meta-theoretical questions resembles the economists' approach to welfare questions (as I will explain in Chapter 5).

It is clearly a conventionalist characterization that the methodological problem of choosing between theories involves some sort of 'trade-off' between generality and simplicity. Such a statement of the methodological problem as an economic problem is precisely what Martin Bronfenbrenner argued when he told us that to improve a theory by one criterion (e.g. simplicity) we must give up some of the other criterion (e.g. generality, which he sees as a hallmark of an opposing 'realistic' school) at an increasing rate [Bronfenbrenner 1966]. It is as if we are constrained along a methodological possibilities frontier analogous to a standard economic concept of the production possibilities frontier. Furthermore, Bronfenbrenner tells us that the choice reduces to a matter of 'subjective preference', that is, subjective optimization. In other words, the conventionalists themselves have raised the economic argument to the meta-theoretical level.

As far as the second possible major objection goes, I simply note that at the meta-theoretical level the conventionalists reduce the original dispute to an argument concerning criteria for judging theories. To pick the perfect competition theory is to optimize simplicity, and to pick the imperfect competition theory is to optimize generality. Here there is no compromise. Given that the choice of theories leads to combinations of levels of simplicity and generality which are constrained along an efficiency frontier, as Bronfenbrenner suggests, the 'ideal' theory, i.e. the methodological optimum (if it exists), is somehow an intermediate between the two extremes, a 'half-way house' as he calls it. I would like to extend this analysis further. The choice of the 'ideal' theory for a compromising Robinsonian conventionalist such as Bronfenbrenner implies the satisfaction of some Pareto-like conditions. Such a condition in this case would be the equality between the relative subjective preference for simplicity vs generality and the marginal rate of methodological substitution of generality for simplicity within the constraint of the methodological possibilities frontier. To reach the half-way house 'ideal' within the Robinsonian conventionalist dispute requires the 'ideal' theory to be a mixture of the two extreme theories (such as allowing imperfection in the factor market but assuming perfect competition in the product market, or the other way around, etc.). I think this optimization methodology is wrong-headed. The solution to this dispute is to get rid of the meta-theoretical constraint in order to be able to increase *both* simplicity and generality. The meta-theoretical constraint results from attempting to resolve the dispute within Robinsonian conventionalism. My suggestion is, of course, what my second best meta-theory suggests. It is also what I think Popper suggests in that it is what underlies his saying that simplicity and generality go together (see Chapter 2).

8. Concluding remarks on Robinsonian conventionalism

To conclude, the primary methodological dispute of the 1960s, which I have called the Robinsonian conventionalist dispute, forced the argument over theories of the firm to be concerned with conventionalist criteria for judging theories. Furthermore, the dispute constrained the choice of theories within the Robinsonian dispute (i.e. perfect vs imperfect competition). Optimization within this meta-theoretical constraint would lead to a mixture of the two theories but there exist obstacles to the success of such a mixture – the ingredients are one false theory and one *ad hoc* and unnecessarily arbitrary theory. By means of the theory of the Second Best I have suggested that the pursuit of a theory of the firm within the constraint of the Robinsonian conventionalist methodology leads to a 'third best' – a mixture of the two disputed theories. And that there exists a 'second best' which can only be achieved by stepping outside (Robinsonian) conventionalist methodology.

5

Methodology as an Exercise in Economic Analysis

> the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line … between the statements, or systems of statements, of the empirical sciences, and all other statements.
>
> Karl R. Popper [1965, p. 39]

Methodology has always occupied a precarious position in academic circles. It is neither only a study of applied logic nor is it only a sociological matter. For me, methodology is the study of the relationships between problems and solutions and between questions and answers. Clearly, logic plays an important role in any methodology, but logic alone would not be sufficient for a complete study. There are many methodological decisions to be made which are neither arbitrary nor irrational. Such decisions occupy a central place in the domain of any methodological study. Of particular interest, it seems to me, is the distinction between intended and unintended consequences of a decision to use a particular method to solve a particular problem or to answer a particular question.

This view of methodology is intended to avoid all the linguistic philosophy that usually goes under the name of methodology. I want to avoid this primarily because I find it unilluminating and uninteresting. I would also like to avoid all the authoritarian appeals to 'rules to promote scientific progress' which individuals, whom we might call 'pseudo-Popperians', might claim to offer with their demands for 'refutability'.

In this chapter, I attempt to illustrate a different view of the Popper-Socrates philosophy of science by presenting a methodological critique of the conventionalist methodology discussed in Chapter 4. I continue to study conventionalist methodology because, though it is quite uninteresting, it remains popular. While conventionalist methodology may, for the reason of popularity, be applied to economics, I think it is uninteresting precisely because it attempts to solve such uninteresting philosophical problems as choosing the 'best' theory from a set of competing theories where 'best' is interpreted as a status representing the highest attainable point on a truth-likeness scale. I want to show that the conventionalist methodological problem, if specified completely, will be found to be unsolvable on its own terms. Again, my argument will be that the conventionalist methodological problem is, methodologically speaking, the same problem that welfare economics theory attempts to solve. The welfare problem is to choose the optimum among competing alternatives. In economics it would be to choose the optimum (possible) state of the economy by means of criteria applied to the allocation of resources. In conventionalist methodology it is to choose the optimum (conceivable) theory by means of criteria applied to the selection of hypotheses or models.

1. Conventionalist methodology

Now let me briefly describe and explain the practice of conventionalist methodology among economists in terms more general than the discussion in Chapter 4. As I noted at the beginning of Chapter 4, economists when confronted by practical problems of applied economics will follow the methodological doctrine of instrumentalism [see Boland 1979a]. In effect, instrumentalism asserts primarily that theories merely play a role as tools or instruments used to solve practical problems or as learning (i.e. heuristic) devices so as to get at the facts more efficiently, and so on. With tools or heuristic devices the question of truth status is considered of less importance than questions about usefulness. Economists when confronting the philosophical problems of economic theory will follow the doctrine of conventionalism [see Boland 1979a, 1982]. Conventionalism asserts primarily that theories are merely catalogues or files of facts. That is, theories are neither (absolutely) true nor false. When viewing theories, both doctrines will pose the methodological problem of choosing the 'better' of any two available theories. Each doctrine seeks a set of criteria or principles to use as a guide for making the (optimum) choice but the choice criteria may be very different.

In economics there are two different approaches to the problem of choice, both of which may be applied to both doctrines. When I was a student in the 1960s, one approach was called the Theory of Choice and was attributed to Walras, Pareto, Hicks and maybe even Marshall. The other was called the Theory of Revealed Preference and was widely attributed to Samuelson [1938, 1948] (see also Georgescu-Roegen [1954]). In the Theory of Choice one would attempt to explain why a particular choice logically follows from a specified set of principles regarding the behaviour of the person(s) making the choice. In the Theory of Revealed Preference one would attempt to infer from the fact that a person has made a particular choice what his or her reasons must have been for that choice. (Actually, the latter approach can only infer the specifications of the principles assumed in a Theory of Choice [see Samuelson 1950b].)

In methodology the analogue of this distinction between approaches is found in the difference between some popular versions of Popper's philosophy of science [e.g. Blaug 1968, 1980; Tarascio and Caldwell 1979; Caldwell 1982] and Thomas Kuhn's philosophy of science [1962/70]. The popular (mis)readings of Popper to which I refer are analogous to a Theory of Choice. It is alleged that certain choices follow from the Popper-Socrates theory of learning and Popper's view of the logic of theories (see also Chapter 2). Kuhn's view is that a particular choice is revealed as preferred by the contents of the standard textbooks of any discipline [Agassi 1971b]. Of course, followers of Kuhn would attempt to infer the significance of the choice of a textbook based on the methodological principles of conventionalism which I noted above. Ultimately, however, the two approaches boil down to the same thing – a set of specified principles for making a choice.

2. Choice in welfare economics

Like my argument in Chapter 4, I wish to draw an analogy between ideas in economic theories of social choice and a basic idea of conventionalist methodology of theory choice. To set up the analogy, I will discuss the economist's theoretical view of social choice with respect to choosing how society allocates its resources. The problem of choice is obviously central in economic theory. The overall nature of the intended and unintended consequences of a particular economic choice is the primary concern of any theory of welfare economics. One particular welfare theory attempts to show that the unintended consequence of particular individual actions (self-interest) leads necessarily to desirable (intended?) social consequences [cf. Pigou 1962]. Arguments have arisen over which social actions lead to unintended consequences that are undesirable [cf. Arrow 1951/63, Mishan 1964] but they will not concern us here.

In the 1960s when welfare economics was a popular topic of discussion, the typical welfare analysis went as follows. If we have an economy of two persons, two scarce factors (i.e. productive resources including labour), two consumption goods (which are to be produced), then the resources will have been optimally allocated only when there is no opportunity for any person, or any producer, to gain without someone else losing. These situations are the so-called ‘Pareto optima’. Usually, many such optima are possible [cf. Samuelson 1950b]. At any one of these optima each individual is choosing a combination of quantities of the two goods which maximizes his or her personal satisfaction within his or her earned income (it is assumed that such maximization is the sole intended consequence of the choice). Also, each producer is choosing the one combination of factors which maximizes its own profit within its productive constraints (it is assumed that such maximization is its sole intended consequence of its choice of productive factors). The necessary conditions for such maximization can be easily established [cf. Lerner 1944] and used as guides for (personal and public) welfare judgements. There still would remain the question of when the available resources between the two producers have been allocated optimally, which depends on whether the relative levels of satisfaction for the two persons have been optimally distributed. At least we would want the allocation to be a Pareto optimum but there may be many such possible allocations. To choose one we need a means of comparing the levels of satisfaction of the individuals concerned. Specifically, we can complete a theory of choice if we have a ‘social welfare function’ which would be used to evaluate each of the various allocations of resources. Since the intended social consequence of allocating resources is assumed to be to maximize social welfare, the society (by means of government action if necessary) would choose the allocation which ranks ‘best’ of all the possible allocations based on that social welfare function. It should be pointed out that without some sort of welfare function, the choice between Pareto optima becomes rather arbitrary.

The conventionalist’s view of this social choice is to see it as an instance of the ‘index number problem’. That is, the problem of computing a single number, an ‘index number’, which will represent (or measure) a multidimensional aggregate. This viewpoint is a ‘conventionalist ploy’ since it assumes the (questionable) possibility of a common measure. This conventionalist ploy has a well-known variant called ‘the identification problem’ which assumes the truth of a model that is used to ‘identify’ variables in a particular set of observations (see Chapter 3). One alternative to the conventionalist ploy is to view the choice in question as a political choice [e.g. Samuelson 1950b]. My use of welfare economics as a means of illustrating the choice problem is by no means arbitrary. As I have attempted to show elsewhere [Boland 1982, Ch. 3], there is a lot of conventionalist philosophy built into the ‘problems’ of economics.

The analysis of the social choice problem concerns the optimal allocation of resources that I have described so far and can be neatly represented by three interrelated diagrams [cf. Samuelson 1950b, Bator 1957]. The major ideas represented in these diagrams are as follows.

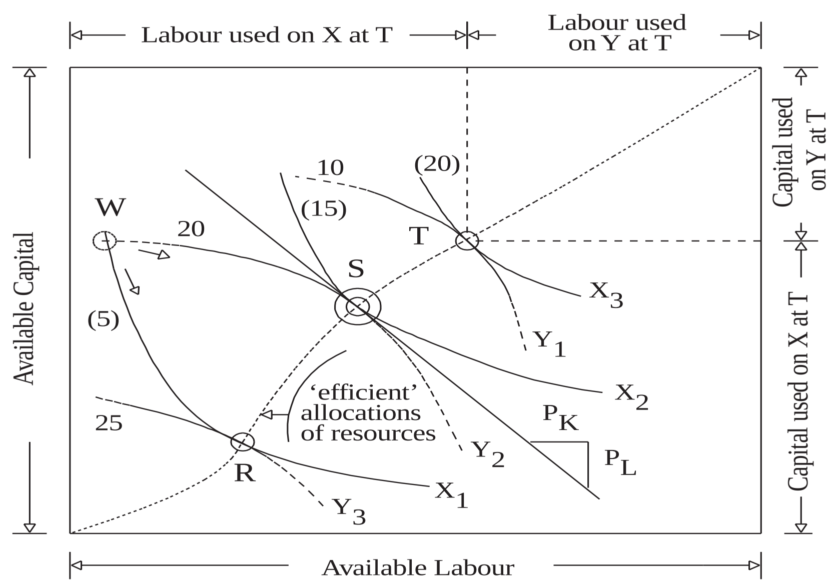

Figure 5.1 *Efficient resource allocation model*

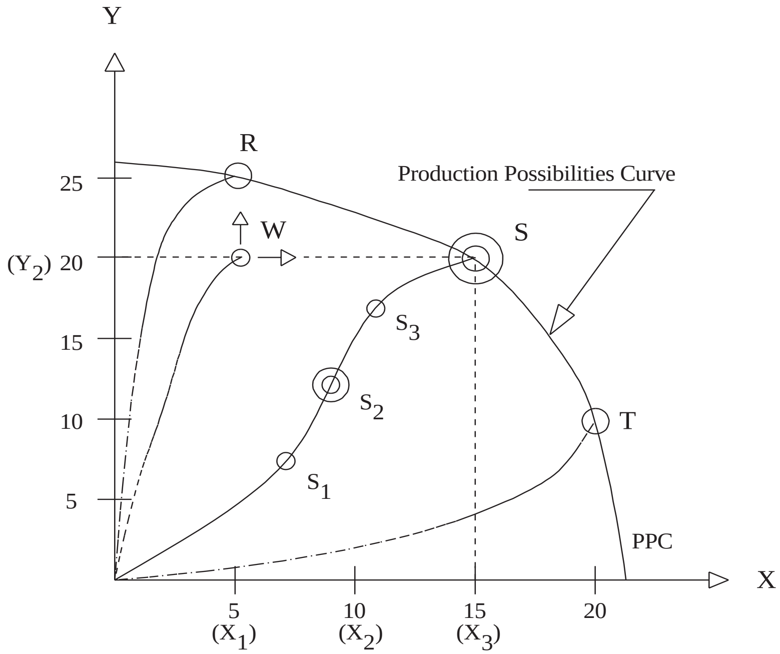

Figure 5.2 *Product-mix*

(1) Society's resources are limited. The limitation is represented in Figure 5.1 by a box with fixed dimensions: one side represents the available amount of labour, the other side represents the available amount of the other resource, that which we traditionally call capital. Any point in this diagram represents one allocation of these resources between the production of the two goods (**X** and **Y**). The respective production functions are represented by two opposing sets of 'iso-quants'. Each iso-quant represents all combinations of labour and capital that will produce one level of output of a good. Given the box and the production functions, we deduce that there is a set of allocations which if any allocation is chosen, it is impossible to increase the output of one good without necessarily reducing the output of the other good (because of the scarcity implied by the fixed dimensions of the box). This set of allocations represents all the 'efficient' allocations of the available resources. It is the locus of points of the tangency between opposing iso-quant curves that represent levels of respective outputs. In Figure 5.2 this same set of allocations is represented by possible quantities of the two goods produced. Here this set is called the Production Possibilities Curve (PPC). Any combinations of outputs to the left or below this curve are producible with the given quantities of available resources and given technology (i.e. production functions).

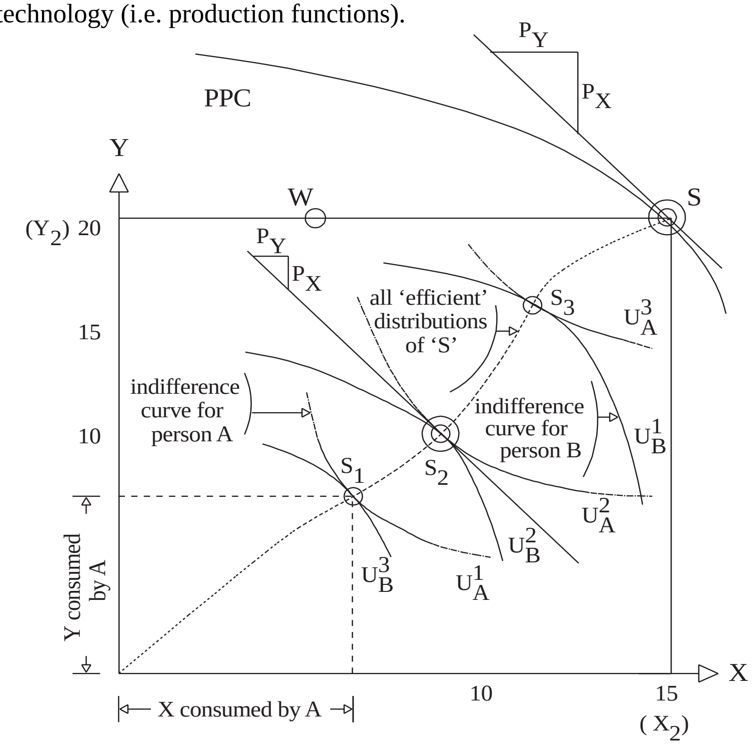

Figure 5.3 *Distribution of mix 'S'*

(2) These two diagrams are interconnected. If we happen to choose any point on the PPC in Figure 5.2, we have also implicitly chosen a point in Figure 5.1. Presumably, society in order to maximize social welfare will necessarily have to pick a point on its PPC. That is exactly what the Pareto optimum conditions assert. Moreover, all points on the PPC are potentially Pareto optima.

(3) Given that the society would want to avoid wasting its resources and thus operate its production efficiently (i.e. choose a 'product-mix' on its PPC), whether a Pareto optimum is achieved depends on how the quantities of goods produced are distributed between the two persons. In this case, we may draw a box whose dimensions are fixed by the choice

of a point on PPC (see Figure 5.2 or Figure 5.3). Any point inside the box of Figure 5.3 (or Figure 5.2) represents one distribution of the available products between the two persons. We can also evaluate the various distributions by considering how the individuals evaluate them. If each individual considers combinations of goods that give the same utility to be equivalent and combinations that give more utility as better, then we can represent the implied evaluation with an iso-utility map or what is more commonly called ‘an indifference map’. As with the other box (by placing one person’s map starting from the lower left-hand corner and the other person’s starting from the upper right-hand corner pointing downward), there is a set of efficient distributions of which if any one is chosen, it is impossible to increase one person’s utility without decreasing the other’s. Choosing one of these distributions is also necessary for Pareto optimality with regard to the relative distribution of utilities between individual members of society.

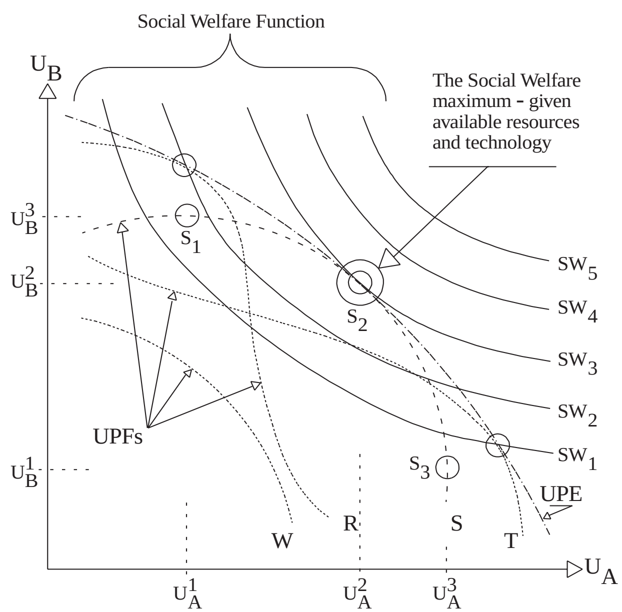

Figure 5.4 *Distributions of personal satisfaction*

(4) Given any particular choice regarding ‘product-mix’ there is an upper limit on both individuals’ levels of utility. Analogous to the PPC of Figure 5.2, following Samuelson [1950b], this upper limit can be represented in Figure 5.4 as the Utilities Possibilities Function (UPF) where for each point on the curve representing an efficient distribution of utilities, there is one point on the locus of tangencies between opposing indifference maps. If we chose a different ‘product-mix’, we would generally have a different set of possible utility distributions as shown in Figure 5.4. If we consider all the possible ‘product-mixes’ and hence all the possible efficient allocations of resources, there is still a gross limit on all the various Utilities Possibilities Functions. This gross limit is represented by an ‘envelope’ which we call the Utility Possibilities Envelope (UPE). Distributions of the individuals’ levels of utility on this envelope (UPE) can be reached only by choosing an allocation of resource which is efficient (i.e. producing a mix on the PPC) and choosing an optimal distribution of utilities. Furthermore, not any such optimal point will do. There will be only one such point, namely the one point on the chosen UPF which is also on the UPE.

(5) Now there are still many possible gross optimal points – i.e. Pareto optima represented by the UPE. We still need a criterion to choose between them. If we had a way of ordering all the individual distributions of utility, i.e. if we had a social welfare function such as SW in Figure 5.4 (where each curve represents a different level of social welfare), by that criterion only one such point would maximize the social welfare. In Figure 5.4 we see that the one social optimum is represented by the one distribution (**S_2**) on the UPE which is also on the highest iso-social welfare curve. If such a social welfare function is describable, we can thereby deduce the necessary conditions for reaching (by choice) that social optimum. Without such a function we have no reason to choose, or change, between one point on the UPE (and all its implications for resource allocations) and any other point on the UPE. However, we can give reasons for not choosing any point not on the UPE.

The question then arises: Can we ever determine such a social welfare function so as to verify the existence of a chosen social optimum? On the basis of what everyone seems to mean by an *acceptable* social welfare function, Arrow [1951/63] has ‘proven’ the impossibility of determining such an acceptable function. The operative concept here is the ‘acceptability’ of the (social welfare) function. Arrow argues that the criteria of acceptability are insufficient to rule out important logical problems of deciding unambiguously what the social welfare optimum is by any acceptable function. Note that Arrow's 'proof' does not deny the existence of a social welfare optimum, only the impossibility of rationalizing it on the basis of the 'reasonable conditions' by which the optimum is determined. A consequence of Arrow's proof is that we can never know (necessarily) when the economy has reached a social optimum, even if it has (although we may still know when it has not by showing that it has not reached at least a Pareto optimum).

The purpose of this somewhat tedious exposition is to show that welfare theory has been quite successful in working out the *criteria* for all the intermediate decisions needed to reach at least a Pareto optimum point – i.e. a point where all individual intentions are realized. Such a point is reached when all individuals are choosing combinations of goods they can afford such that they are maximizing their personal utility within the constraints of their own income. They will know they are accomplishing such maximization when the slope of their respective indifference curves equals the slope of their respective budget lines (which is determined by the market-determined relative prices). Likewise, at a Pareto optimum point each producer is choosing the combination of factor inputs such that it is maximizing its profit given the (market-determined) price of its product. The producer will know profit is maximum when the relative marginal productivity equals the (market-determined) relative prices of the factors such that costs are being minimized for the level of output.

3. Conventionalist methodological criteria

Now let me turn again to conventionalist methodology and consider the criteria used to choose one theory among competing theories. By 'competing theories' I mean only those theories which attempt to explain the same things – e.g. the traditional alternative theories of the firm (see Chapter 4). While I list here a few of the commonly suggested criteria, I want to point out that I am not trying to include all of them or even to identify particularly significant criteria. My major concern is to examine why any criterion might be seriously entertained as an essential part of a methodological study.

Suggestions concerning the choice of one theory over another include admonitions to choose the theory which is:

(a) more simple,
(b) more general,
(c) more verifiable,
(d) more falsifiable,
(e) more confirmed,
(f) less disconfirmed.

Representative examples can be found in Eddington [1958], Friedman [1953], Koopmans [1957], Poincaré [1905/52], Popper [1959/61], Samuelson [1947/65] and various essays in Krupp [1966].

For economists interested only in solving practical problems, the followers of Friedman's instrumentalism, the confirmation criterion, (e), is probably more important. Generally instrumentalists must try each theory (only) until one is found which works regardless of these criteria. Therefore I will drop further consideration of instrumentalism since it raises no interesting philosophical problems, or at least none which would interest an instrumentalist [see Boland 1979a, 1980, 1981a, 1984].

4. Choice theory in conventionalist methodology

Now I will outline a conventionalist's methodology for the choice of a theory from a set of competing theories. Let us say that with any one theory (i.e. one set of assumptions about the behaviour of people and/or institutions), say $\mathbf{T_1}$, we can build many different models which differ only in how we represent each of the constituent assumptions of the theory. After specifying which variables are to be endogenous and which are to be exogenous, by specifying, say, linear behavioural relations and linear constraints, we have a model (say, $\mathbf{M_{11}}$) which may be very simple but which lacks widespread applicability, i.e. generality. By increasing the number of endogenous variables (giving $\mathbf{M_{12}}$) or by increasing the 'degree' of the relationships (giving $\mathbf{M_{13}}$), we may build a model which is more general, but less simple. As shown in Chapter 3, changing the number of variables or the 'degree' of the relationships changes the testability of the theory. When we choose a theory we choose the things to be explained or described and thereby we face certain limits on the testability of the chosen theory. Moreover, the more we try to explain the more difficult our theory is to test (if for no other reason, we need to observe more things).

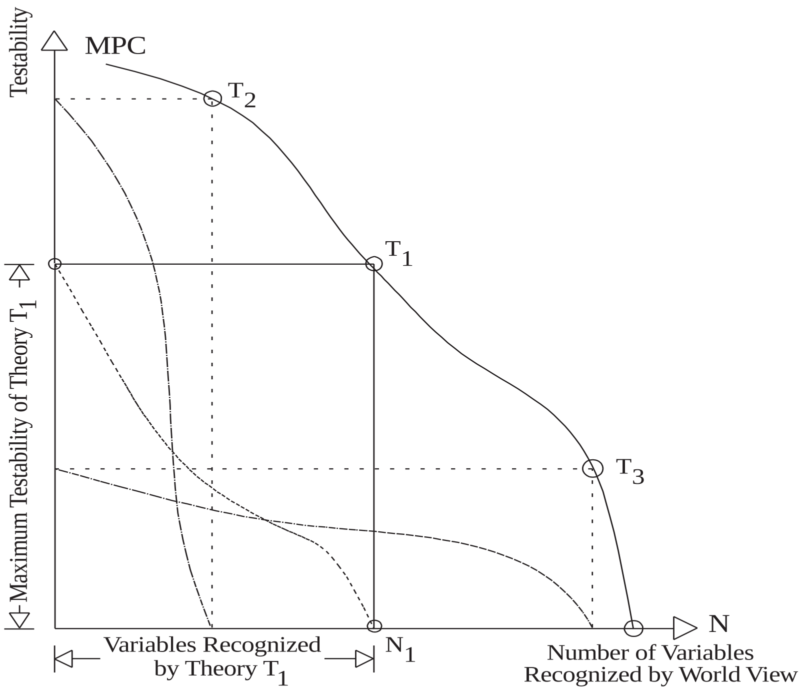

Figure 5.5 *Conceivable theories*

I have represented my understanding of models and theories with a diagram similar to the ‘product-mix’ diagram of welfare economics. In Figures 5.5 and 5.6 a Metaphysical Possibilities Curve (MPC) represents the limitations on possible theories within a particular ‘world view’, as Thomas Kuhn [1962/70] might call it. The negative slope of this curve reflects the view that as we attempt to explain more, our explanation becomes less testable.

The properties of possible models of any chosen theory are also represented within this diagram. In choosing any theory – i.e. choosing a horizontal size representing the number of variables in my diagram – there is a set of models of any particular theory which are efficient with respect to the two criteria of simplicity and generality. This set is represented by the locus of tangencies between iso-generality and iso-simplicity curves. For models off this locus it may be possible to choose other models. There may exist models off this locus which are more simple without being less general (or vice versa). This would not be

possible once we have chosen a model on the locus. There may be many such ‘efficient’ models of a theory. The problem (of choice) is still to choose one of them.

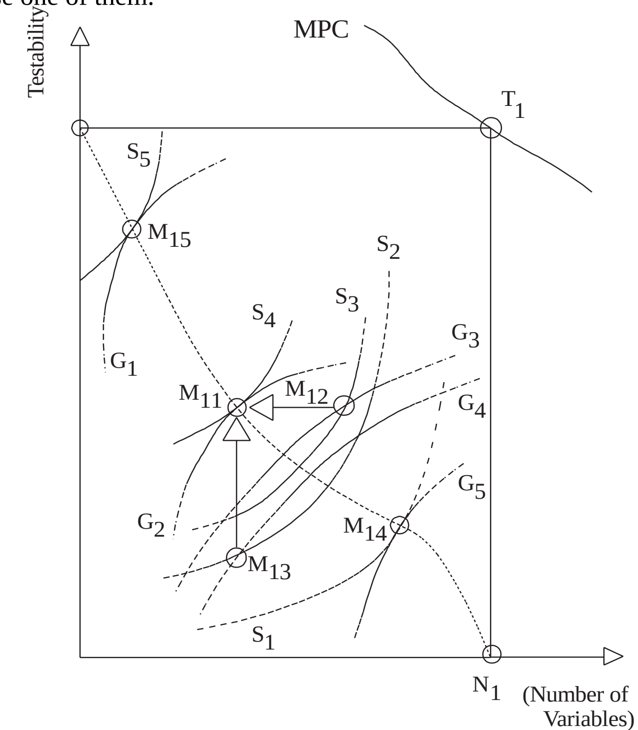

Figure 5.6 *Models of a theory*

For any theory (i.e. a set of specific models) there is an upper bound on the possible combinations of simplicity and generality. By analogy with Figure 5.4, I shall call this the Methodological Possibilities Function (MPF) and label it with the theory it represents (see Figure 5.7). Within our world view’s Metaphysical Possibilities Curve, if one should choose a different theory (i.e. a different number of variables and thus a different limit on testability) one can also determine its Methodological Possibilities Function by considering all of the possible ‘efficient’ models of that theory.

For our current world view there are limitations on the set of all possible MPFs which are represented by the envelope of the individual MPFs. I shall call this the Methodological Frontier (MF). With this I am asserting the existence of a set of models, each of a different theory, any of which, if chosen, makes it impossible to choose another possible model or theory which has greater simplicity and no less generality (or greater generality and no less simplicity). These are, by analogy with welfare economics, the Pareto optima of our particular world view.

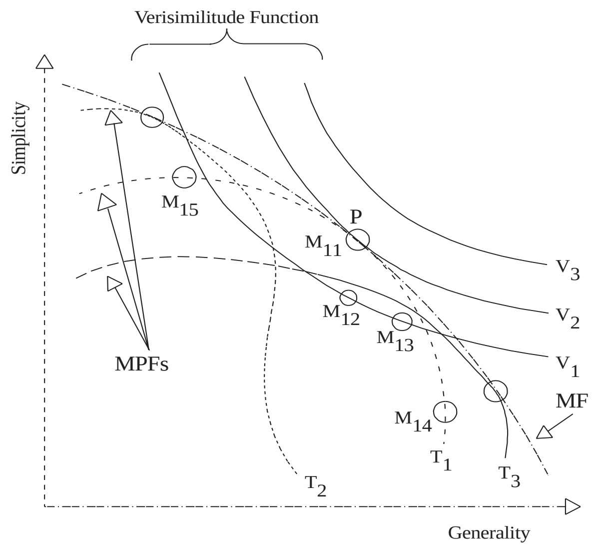

Figure 5.7 *Choice of paradigm*

The problem of choosing one of these Pareto optimal models (hence one of the theories) still remains. With the allocations of resources and distributions of personal satisfaction represented on the UPE of welfare economics (see Figure 5.4), if we do not have another means of comparing points on the UPE (e.g. if we do not have a welfare function), we cannot choose one Pareto optimum over another. In short, the criterion of Pareto optimality is necessary but not sufficient for a complete choice theory. In the case of my Methodological Frontier, in order to compare the Pareto optimal models of our world view we need another criterion in addition to those of explanatory power, simplicity, generality or testability. The most obvious criterion to use is the degree of corroboration or confirmation or what we might call a 'verisimilitude function'. The more a theory is confirmed (or confirmable because it is a good approximation), the 'better' the theory is. With such a criterion we

can complete an explanation of the choice of a paradigm theory or model. Namely, we might posit that the paradigm is chosen to maximize the degree of corroboration or confirmation. I have represented this criterion in Figure 5.7 where the paradigm is the one Pareto optimal model with the highest possible degree of corroboration within our world view. Some might even say that movement towards this paradigm is 'scientific progress' and set about laying down rules (as in welfare economics) for finding it.

This then completes my presentation of the conventionalist theory choice where the choice of a theory among competing theories is based on the choice of the 'best' model within our world view. I think it is a natural outcome of the conventionalist methodology – i.e. a methodological outcome which presents methodology as a problem of choosing the 'best theory' among competing theories.

5. The failures of welfare theory

Since I want to criticize conventionalist methodology by showing, as I have, that it presents, formally, the same problem-solution situation found in welfare theory, I will now outline what I think are the major failures of welfare analysis.

(1) If we were to accept only operational definitions of a social welfare function, as Samuelson and Arrow seem to require, then we must conclude that the construction of such a welfare theory of economic choice is impossible. If Samuelson and Arrow are correct, we can never explain (within economics at least) why one Pareto optimum will be chosen over another. Those who argue this way conclude that beyond the Pareto criteria, welfare analysis is irrelevant.

(2) The problem which welfare economics seems to be solving (when we allow that social welfare functions are possible) is one of choosing between allocations of resources within the constraints of existing production technology and institutions. In other words, the problem of choice is necessarily a 'static' problem and welfare analysis is appropriately a 'static' solution. Clearly, the more interesting (and pressing) problem is to find the means of expanding the various possibilities frontiers. Many would say that most of the economic activity in the real world seems to be directed to that problem.

(3) The welfare problem is approached as a variation of some form of a calculus problem, as a problem of maximizing some quantity (welfare). Such a view would do the job of explaining the social choice but there is no reason why every individual (let alone the society) should have a single peaked or even a monotonic welfare function. If we recognize that absence of a welfare function means that welfare does not relate to economic situations in a 'smooth and continuous way', then we must also recognize that all such forms of welfare economics fail to explain any observed social choice. Being able to draw a picture representing what we need (e.g. Figure 5.4) does not mean we are representing a realistic possibility.

6. The failures of conventionalist methodology

My critical study of conventionalist methodology is now complete. The same objections that I have listed for welfare analysis can be brought to bear against conventionalist methodology.

(1) If we accept the Popper-Socrates view that there is no operational way of knowing when a theory or model is true (even if it is true), then it is impossible to construct the necessary 'verisimilitude function' which would enable us to compare two competing theories. We may accept some econometric convention (based, for example, on R^2s, *t*-tests, etc.) to assign degrees of confirmation, but there is no reason why one convention would be accepted over another (unless we are only concerned with practical problems [see Agassi 1966b, 1967]). Thus the problem of choosing between theories is raised to a choice between conventions of confirmation. Thus, there is no way to resolve methodological disputes over the choice of theories on the Methodological Frontier since all theories on the Frontier are, at least, Pareto optimal by the other criteria. In the absence of a 'verisimilitude function', once we are faced with choosing between the Pareto optimal models of theories, conventionalist methodology becomes irrelevant.

(2) Philosophers such as Karl Popper [1959/61, 1965] and Joseph Agassi [1963, 1966a] have argued that conventionalist methodology, in attempting to solve the choice problem, is pursuing an uninteresting (because unsolvable) problem. The more interesting problem might be understanding and even promoting revolutions in our world views. The efforts of science as a learning process are directed at learning through criticizing our world view, especially the limitations on our world view. Hopefully, we can expand our world view and thereby free ourselves of the constraints imposed by our current world view. This is a dynamic problem on which conventionalist methodology, even allowing for a verisimilitude function, can throw little light.

(3) Finally, as with welfare economics, the solution of the choice problems of conventionalist methodology relies on certain calculus-type situations – e.g. maximizing corroboration, maximizing expected probability, etc. To use such a method requires that the relationship between variables of a model or theory and the real world (i.e. the correspondence between theories and facts) be of a continuous, single-peaked or monotonic nature [see Boland 1986, Chs 3 and 4]. There is no reason to expect this to be the case. In fact, only one theory among *available* competitors can be true (and there is no reason why even one should be true). All others will be false. Attempts to disguise this with calculus can only increase the difficulty in criticizing our world view and thereby increase the difficulty in learning about the real world.

PART III

Exploring the Limits of the Popper-Samuelson Demarcation