How to Design Galilean Fall Experiments in Economics*

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In the social sciences we hardly can create laboratory conditions, we only can try to find out which kinds of experiments Nature has carried out. Knowledge about Nature's designs can be used to infer conditions for reliable predictions. This problem was explicitly dealt with in Haavelmo's (1944) discussion of autonomous relationships, Friedman's (1953) as-if methodology, and Simon's (1961) discussions of nearly-decomposable systems. All three accounts take Marshallian partitioning as starting point, however not with a sharp ceteris paribus razor but with the blunt knife of negligibility assumptions. As will be shown, in each account reflection on which influences are negligible, for what phenomena and for how long, played a central role.

1. Introduction.

> But even in mechanics long chains of deductive reasoning are directly applicable only to the occurrences of the laboratory. By themselves they are seldom a sufficient guide for dealing with the heterogeneous materials and the complex and uncertain combination of the forces of the real world. (Marshall 1920, 771)

The laws of nature are, usually, not found in the 'wild' but in laboratories by means of controlled experiments. The idea behind a controlled experiment is to create a specific environment, a laboratory, in which the rele-

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vant variables are manipulated in order to take measurements of particular parameters with the aim to discover the relationship, if any, between these variables. However, these laboratory conditions cannot be set up to investigate various relationships of interest in the social sciences. We can only be passive observers who have to unearth the lawful relationships by inferring from the data supplied by Nature the underlying 'designs' of the experiments Nature performs. This approach will always fall short of a controlled experiment. We can only observe experiments as they occur in the open air and are not able to manipulate any of the relevant objects.

The problem of gaining knowledge about lawful relationships without being able to do controlled experiments was explicitly dealt with in Tryvge Haavelmo's (1944) discussion of autonomous relationships, Milton Friedman's (1953) as-if methodology and Herbert Simon's (1962) approach to treat complex systems as nearly decomposable. This problem is analogous to the fall experiments that Galileo had carried out to find out about the lawful behavior of falling bodies without the usage of a vacuum pump. He was able to design his experiments such that 'other influences' were negligible, by choosing the most adequate object and a convenient height from which to drop.

The view that we need laboratories to detect laws of nature is most clearly expressed by Nancy Cartwright's account of laws. She defines a law of nature as "a necessary regular association between properties" (Cartwright 1999, 49). A consequence of her capacities account of laws of nature is that necessary regular associations hold only ceteris paribus, which means that "they hold only relative to the successful repeated operation of a nomological machine" (50). A nomological machine is "a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws" (50). Fixed patterns of association are a consequence of the operation of factors that have stable capacities arranged in the 'right' way in the 'right' kind of stable environment (138).

As a result of this account, the design of an experiment to discover laws is equal to the design ('blueprint' as she calls it) of a nomological machine. Although I agree with her that for laws we need a kind of a nomological machine: a right arrangement of stable capacities in the right kind of environment, we differ in opinion about the necessity of stability of the environment. In the social sciences we hardly can assume or arrange a stable environment. Therefore Cartwright is rather pessimistic about finding or using lawful relationships. But in my view we do not need to require stable environments for discovering them. The works of Haavelmo, Friedman and Simon show different strategies to model the data such that they function as designs for good experiments to uncover lawful relationships.

Before we explore these strategies, we first have to make explicit what in the social sciences usually is meant by lawful relationships. Philosophers have traditionally employed various standard criteria to distinguish laws from other types of generalizations. These criteria take the forms of: laws are said to be exceptionless generalizations and to make no reference to particular objects or spatio-temporal locations and to have a very wide scope. James Woodward (2000) shows that these criteria are not helpful either for understanding what is distinctive about laws of nature or for understanding the features that characterize explanatory generalizations in, for example, the social sciences. "In general, it is the range of interventions and changes over which a generalization is invariant and not the traditional criteria that are crucial both to whether or not it is a law and to its explanatory status" (Woodward 2000, 222).

Woodward's idea of invariance is this: a generalization describing a relationship between two or more variables is invariant if it would continue to hold—would remain stable or unchanged—as various other conditions change. The set or range of changes over which a relationship or generalization is invariant is its domain of invariance. So, invariance is a relative matter: a relationship is invariant with respect to a certain domain.

This paper supports Woodward's view that it is the notion of invariance that is useful for understanding explanatory practice in the social sciences and not the concept of a law of nature fulfilling the above-mentioned traditional criteria. However, it will be shown that the domain of invariance the social scientists are concerned with in finding explanatory generalizations is larger than the domain Woodward assumes to be crucial for their explanatory status.

Two sorts of changes can be distinguished that are relevant to the assessment of invariance. First, there are changes in the background conditions to a generalization, that is changes that affect other variables besides those that figure in the generalization itself. Second, there are changes in those variables that figure explicitly in the generalization itself. In his discussion of invariance, Woodward emphasizes that only a subclass of this latter sort of changes is important, namely changes that result from an intervention, that is changes that result from a causal process having the right causal characteristics as described in his paper. The reason he gives for this is that some background conditions are causally independent of the factors related by the generalization in question and therefore of no importance. However, other background conditions might be causally connected to some of the factors related by the generalization, and changes in these conditions might disrupt the relationship. A relationship that holds in certain specific background conditions and for a restricted range of interventions might break down outside of these.

The interesting questions for social scientists are not only whether a relationship is invariant under certain specific kinds of changes and interventions but also under which changes it remains invariant; they want to know the domain of changes for which it holds. Social scientists are faced with constantly changing background conditions and they would like to know whether the relationships on which they base their policy advice still hold tomorrow.

Although Woodward considers the notion of invariance under interventions as the key feature that a generalization must possess if it is to play an explanatory role, he admits implicitly that discussion of invariance in various sciences is broader than only in terms of intervention. He views an intervention as an idealization of an experimental manipulation—by human beings or Nature—and probably therefore de-emphasizes the role of unstable background conditions. However, when he discusses the idea that invariance comes with degrees, he uses the notion of Haavelmo's autonomy ("just another name for what we have been calling invariance" (Woodward 2000, 215)) to clarify the relativistic characteristic of invariance. In Haavelmo's account of autonomy, invariance is not only defined with respect to interventions but also to changes in background conditions, as we will see below.

In Woodward's account of explanation in relation to invariance, the difference between laws and invariant generalizations is considered as a matter of degree: laws are generalizations that are invariant under a large(r) and (more) important set of changes. So, any strategy to find laws outside a laboratory has to deal with the question: Invariant with respect to what domain? In economics (and econometrics) such an account is captured by the notion of autonomy, as will appear in all three research-strategies of finding explanatory generalizations discussed in this paper.

2. The Problem of Autonomy. Trygve Haavelmo discussed the problem of finding invariant relationships in economics in his 1944 paradigm paper "The Probability Approach in Econometrics"[1], published in a supplement of *Econometrica.* This problem, which he called 'the problem of autonomy' was worded as the problem of "judging the degree of persistence over time of relations between economic variables," or more generally speaking, "whether or not we might hope to find elements of invariance in economic life, upon which to establish permanent 'laws'" (Haavelmo 1944, 13). The problem we typically face in economics is that real economic phenomena cannot be investigated insulated from other potential influences falling

1. It was the manifesto of the so-called 'Probabilistic Revolution' in econometrics, in which economic data no longer were considered as results of the workings of deterministic mechanisms but as the outcomes of a system of simultaneous relations that are essentially stochastic (see De Marchi and Gilbert 1989 and Christ 1994).

outside the theoretical domain. We have to deal with passive observations, and these are influenced by a great many factors not accounted for in theory which cannot be eliminated by creating a ceteris paribus environment.

Haavelmo's approach of finding invariant relationships without being able to set up ceteris paribus conditions can be explicated by the following model: Let y be a economic variable whose behavior is determined by a function, F, of independent causal factors $x_1, x_2, \ldots$

$$y = F(x_1, x_2, \ldots) \tag{1}$$

The way in which the factors x_i might influence y can be represented by the following equation:

$$\Delta y = \Delta F(x_1, x_2, \ldots) = \partial F/\partial x_1 \cdot \Delta x_1 + \partial F/\partial x_2 \cdot \Delta x_2 + \ldots \tag{2}$$

The deltas, Δ, indicate a change in magnitude. The terms $\partial F/\partial x_i$ indicate how much y will change proportionally due to a change in magnitude of factor x_i.

Suppose we are trying to discover an invariant generalization that could be used to explain the phenomenon y. In principle, there are an infinite number of factors, $x_1, x_2, \ldots$, that could influence the behavior of y, but we hope that it may possible to establish a constant and relatively simple relation between the y and a relatively small number of explaining factors, x. In a laboratory, we would artificially isolate a selected set of factors from the other influences, in other words we would take care that ceteris paribus (CP) conditions are imposed: $\Delta x_{n+1} = \Delta x_{n+2} = \ldots = 0$, so that a simpler relationship can be investigated

$$\Delta y_{CP} = \partial F/\partial x_1 \cdot \Delta x_1 + \ldots + \partial F/\partial x_n \cdot \Delta x_n \tag{3}$$

Moreover, in a controlled experiment the remaining factors, x_i, can be changed in a systematic way to gain knowledge about the $\partial F/\partial x_i$'s and, so, establish the relationship between y and a limited number of factors $x_1, \ldots, x_n$.

However, in economics, we are not able to carry out "experiments that *we should like to make* to see if certain real economic phenomena—when *artificially isolated* from 'other influences'—would verify certain hypotheses" (Haavelmo 1944, 14). We can only passively observe "the stream of experiments that Nature is steadily turning out from her own enormous laboratory" (14). Having only passive observations available, Haavelmo's distinction between so-called 'potential' and 'factual' influences is fundamental to judge the degree of persistence over time. Taking into account that by definition:

$$\partial F/\partial x_i = \left[F(x_1,\ldots,x_i + \Delta x_i,\ldots,x_n) - F(x_1,\ldots,x_i,\ldots,x_n)\right]/\Delta x_i \qquad (4)$$

and for a fixed set of displacements, say $\Delta x_i = \Delta_i$, Haavelmo defined the potential influence of a factor x_i on y as

$$\Delta_i y = F(x_1,\ldots,x_i + \Delta_i,\ldots,x_n) - F(x_1,\ldots,x_i,\ldots,x_n) = \partial F/\partial x_i \cdot \Delta_i. \quad (5)$$

The factual influence was defined as $\partial F/\partial x_i \cdot \Delta x_i$.

We usually passively observe (PO) a limited number of factors that have a non-negligible factual influence:

$$\Delta y_{PO} \approx \partial F/\partial x_1 \cdot \Delta x_1 + \ldots + \partial F/\partial x_n \cdot \Delta x_n \qquad (6)$$

Thus, the relationship $y = F(x_1, \ldots, x_n)$ explains the actual observed values of y, provided that the factual influence of all the unspecified factors together are very small as compared with the factual influence of the specified factors $x_1, \ldots, x_n$.

The problem, however, is that it is not possible to identify the reason for the factual influence of a factor, say x_{n+1}, being negligible, $\partial F/\partial x_{n+1} \cdot \Delta x_{n+1} \approx 0$. We cannot distinguish whether its potential influence is very small, $\partial F/\partial x_{n+1} \approx 0$, or whether the factual variation of this factor over the period under consideration is too small, $\Delta x_{n+1} \approx 0$. We would like only to get rid of factors whose influence was not observed because their potential influence was negligible to start with. At the same time, we want to retain potential factors whose influence was not observed because they varied so little that their potential influence was veiled.

The variation of x_{n+1}, is determined by other relationships within the system. In some cases a, virtually, dormant factor may become active because of changes in the economic structure elsewhere. However, deciding whether a factor should be accounted for in the relationship under investigation should not depend on such changes. The relationship should be autonomous with respect to structural changes elsewhere.

In practice, the difficulty in economic research does not lie in establishing simple relations, but rather in the fact the empirically found relations, derived from observation over certain time intervals, are "still simpler than we expect them to be from theory, so that we are thereby led to *throw away* elements of a theory that would be sufficient to explain apparent 'breaks in structure' later" (Haavelmo 1944, 26). This is what Haavelmo called the problem of autonomy of economic relations. Some of these relations have very little autonomy because their existence depends upon the simultaneous fulfillment of a great many other relations. Highly autonomous relations are those that "describe the functioning of some parts of the mechanism *irrespective* of what happens in some *other* parts" (28).

Haavelmo called any relation that is derived by combining two or more relations a confluent relation. In general, a confluent relation has a lower degree of autonomy than the relations from which it is derived. This will rise to the problem that an infinity of systems of confluent equations can derived from a system built up of equations that have a certain degree of autonomy.

Autonomous relations are those relations that could be expected to have a great degree of invariance with respect to various changes in the economic structure. However, this kind of invariance should not be equated with the observable degree of constancy or persistence of a relation. The degree of autonomy refers to a class of hypothetical variations in structure, for which the relation would be invariant, while its actual persistence depends upon what variations actually occur.

3. Cowles Commission Approach. Haavelmo's design rules for econometrics were considered an alternative to the experimental method of science (Morgan 1990, 262). However, although researchers at the Cowles Commission[2] adopted Haavelmo's 'blueprint' for econometrics (Morgan 1990, 251), they scrapped the term 'autonomy' because it was believed that the theoretical relationships there were trying to measure were autonomous (see Aldrich 1989). The reason for believing this was that Haavelmo had pointed out the possibility that the empirically found relationships may be simpler than theory would suggest. This could lead researchers to discard potential influences that could explain shifts in these relationships (see above). This problem could be avoided by building models as comprehensive as possible, based on a priori theoretical specifications. As Christ (1994, 53) observes, the Cowles Commission's theoretical econometric work "did not have much to say about the process of specifying models, rather taking it for granted that economic theory would do that, or had already done it."[3]

The aim of the Cowles Commission's program was to build increasingly comprehensive models to improve their predictability so that they could be used as reliable instruments for economic policy. The implications of a policy change could then be forecasted. One of the early results was a monograph *Economic Fluctuations in the United States* (Klein 1950). It

2. The Cowles Commission for Research in Economics was set up in 1932 to undertake econometric research. The journal *Econometrica,* in which Haavelmo's paper appeared, was run by the Cowles Commission.

3. However this observation does not apply to Haavelmo 1944, in which the discussion of autonomy must be seen as dealing with specification problems.

presented three models of the United States economy, called model I, II and III, made up of from three to fifteen equations.

Andrew W. Marshall (1950a, 1950b) and Carl F. Christ (1951) conducted tests on Klein's fifteen-equation model III. "These two studies were among the first to act on the precept that econometric models, like any other theories, must be tested by their performance in making predictions" (Christ 1952, 49). An important part of Marshall and Christ's tests was a comparison of the predictive power of Klein's model against that of simple extrapolation models, the so-called 'naive models'. Marshall had tested Klein's model III for the post-sample period of 1946–47.

Two naive models were used for testing. The first, 'naive model I,' says that next year's value of any variable will equal this year's value plus a random normal disturbance ε^{I}_{t} with zero mean and constant variance: $y_{t+1} = y_t + \varepsilon^{I}_{t}$. The second, 'naive model II,' says it will equal this year's value plus the change from last year to this year plus a random normal disturbance, ε^{II}_{t} with zero mean and constant variance: $y_{t+1} = y_t + (y_t - y_{t-1}) + \varepsilon^{II}_{t}$. The results of these tests were that three equations were rejected on the basis of these naive model tests.

Christ (1951) revised Klein's model, estimated it with the data for 1921–47, and tested the results against 1948 data. He distinguished between two groups of tests: 'tests of internal consistency' and 'tests of success in extrapolation and prediction.' The first group comprised tests dependent only on data available for use in the estimation process; the second group comprised tests that used post-sample data and were therefore considered to be of higher authority.

The results of the naive model tests were remarkable. Each of the two naive models predicted seven out of thirteen endogenous variables better than did the reduced-form equations, as estimated by the ordinary least-squares method. Naive model I was better at predicting in fifteen cases out of twenty-one, and naive model II predicted better in thirteen cases out of twenty-one in comparison to the reduced form, as estimated by the restricted least-squares method. So, the econometric models in those days failed to be better predicting devices than the very simple naive models. In defense of this econometric modeling approach, Christ put forward the argument that econometric models are preferable to naive models because they are better at predicting the effects of alternative policy measures.

4. Milton Friedman's Methodology. An important critique to the Cowles Commission approach came from Milton Friedman. He doubted the validity of Cowles Commission method of econometric modeling on the basis of the poor results obtained by Marshall and Christ's post-model forecasting tests. Friedman's "Comment" (1951) on Christ's paper was very critical towards the Cowles Commission program but approved of

Marshall' and Christ's post-model tests, in particular the naive model tests.

Friedman considered naive models as standards of comparison, the 'natural' alternative hypotheses—or 'null' hypotheses—against which to test the hypothesis that the econometric model makes good predictions. On the basis of Christ's exercise, then, one should reject the latter hypothesis. Friedman opposes Christ's argument that these models are preferable to naive models because of their ability to predict consequences of alternative policy measures, by claiming that naive models can make such predictions, too. One can simply assert that a proposed change in policy will have no effect. The assertion that the econometric model can predict the consequences of policy changes, according to Friedman, is a 'pure act of faith.'

Friedman interpreted the disappointing test results as evidence that econometric modeling of an economy as a whole was premature, and cannot be achieved until dynamic models of parts of an economy are adequately developed. His lack of faith in the Cowles Commission program sent him in another research direction—namely, that of partitioning, the so-called 'Marshallian approach'[4] (see Hoover 1988, 218–225): "Man's powers are limited: almost every one of nature's riddles is complex. He breaks it up, studies one bit at a time, and at last combines his partial solutions with a supreme effort of his whole small strength into some sort of an attempt at a solution of the whole riddle" (Marshall [1898] 1925, 314; quoted in Friedman 1949, 469).

Marshall's approach of partitioning was based on an application of the ceteris paribus clause. The sentence immediately following his quote above shows how: "In breaking it up, he uses some adaptation of a primitive but effective prison, or pound, for segregating those disturbing causes, whose wanderings happen to be inconvenient, for the time: the pound is called *Cæteris Paribus*" (Marshall [1898] 1925, 314; see also 1920, 366).

For Friedman, the ability to predict was the quality of a model that should be evaluated, not its realisticness.[5] This methodological standpoint was spelled out in his well-known article "The Methodology of Positive Economics" (1953)[6] and is generally considered as an economic science version of 'instrumentalism'.

However, a 'lapse into instrumentalism' is unnecessary, as Alan Mus-

4. In economics, this refers to the better-known Alfred Marshall (1842–1924).

5. Mäki (1998) suggests using the term 'realisticness' instead of 'realism' if one argues about a property or a set of properties of theories and their constituent parts. I follow his suggestion.

6. See (Hirsch and De Marchi 1990) for an extensive discussion of Friedman's methodology and its background.

grave (1981) has shown in his discussion of the different kind of assumptions that could be distinguished in Friedman's paper. According to Musgrave, Friedman's instrumentalist position stems from his failure to distinguish three different types of assumption: negligibility, domain and heuristic assumptions. A negligibility assumption is the assumption that a factor that could be expected to affect the phenomenon under investigation actually has no effect upon it, or at least no detectable effect. A domain assumption is the assumption that an expected factor is absent and so is used to specify the domain of applicability of the theory concerned. A heuristic assumption is made if a factor is considered to be absent or negligible in order to simplify the logical development of the theory.

In fact, there is also a fourth type of assumption in Friedman's paper, although this is only mentioned in a footnote and not labeled separately by Musgrave. They belong to the kind of 'as if *p*'-assumptions where *p* is an analogous mechanism, and not an idealization in the sense of one of the other three assumptions. In other words, *p* is a simulacrum: "something having merely the form or appearance of a certain thing, without possessing its substance or proper qualities" (*Oxford English Dictionary*, 1933). This definition is used by Cartwright (1983) to denote what models are, stressing the 'anti-realist' aspect of models. She could have used the term 'simulation', but probably didn't because it refers to the assumption of false appearances for the sake of deception. But in the social sciences today the term is employed without this connotation of deception: "the assumption of the appearance of something without having its reality" (Dawson 1962, 1–2).

To clarify Friedman's methodological anti-realisticness position, let us consider an example he used, namely a Galilean fall experiment. The starting point is the same problem as in Haavelmo's 'Probability Approach', namely the problem of not being able to carry out controlled experiments, and so being dependent on passive observations alone:

> Unfortunately, we can seldom test particular predictions in the social sciences by experiments explicitly designed to eliminate what are judged to be the most important disturbing influences. Generally, we must rely on evidence cast up by the 'experiments' that happen to occur. The inability to conduct so-called 'controlled experiments' does not, in my view, reflect a basic difference between the social and physical sciences both because it is not peculiar to the social sciences—witness astronomy—and because the distinction between a controlled experiment and uncontrolled experience is at best one of degree. No experiment can be completely controlled, and every experience is partly controlled, in the sense that some disturbing influences are relatively constant in the course of it. (Friedman 1953, 10)

The empirical regularity Galileo found by his fall experiments is a very simple one: Distance (s) is proportional to time (t) squared, $s \propto t^2$. From this empirical finding he inferred a law of falling bodies that states that the acceleration of a body dropped in a vacuum is a constant and independent of the mass, composition and shape of the body, the manner of dropping it, etc.

The question is to what extent can the law of falling bodies be applied outside a vacuum. According to Friedman, to answer this question one has to take into account the kind of object that is to be dropped. Galileo's law works well if applied to compact balls. "The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves *as if* it were falling in a vacuum" (Friedman 1953, 16). Air resistance is negligible for compact balls falling relatively short distances, so they behave approximately as described by Galileo's law. In other words, for compact balls we can apply the negligibility assumption.

The problem, now, is to decide for which objects the air-resistance is negligible. Apparently, this is the case for a compact ball falling from the roof of a building, but what if the object is a feather or the object is dropped from an airplane at an altitude of thirty thousand feet? One of the traditional criteria on laws is that they must contain no essential reference to particular objects or systems. In contrast to this traditional view, Friedman argues that a specification of the domain of objects and systems for which a generalization applies—the scope of the relationship, as Woodward has called it—should be attached to the generalization.

To deal with this problem of specification, two options are possible. One is to use a more comprehensive theory—the Cowles Commission approach—"from which the influence of some of the possible disturbing factors can be calculated and of which the simple theory is a special case" (Friedman 1953, 18). However, the extra accuracy it yields may not justify the extra costs of achieving it, "so the question under what circumstances the simpler theory works 'well enough' remains important" (18). The second option is to select the phenomena for which the theory works. That is to say, to indicate the domain for which the formula holds, for example, the law of falling bodies (outside a vacuum) holds for compact balls and not for feathers. This means that one should specify the domain for which a generalization holds, but this should be done independently of this generalization. Thus, one should not incorporate this specification into the generalization itself, as the Cowles Commission program aimed at. Having a generalization that has been successfully used to model and explain certain phenomena, it is a separate empirical question what the full range of phenomena is that can be explained by it and of which the answer thus can not already been built into this generalization (see Woodward 2000, 231 where this issue is extensively discussed). For which previously unexplained phenomena the generalization holds must be discovered empirically.

> The important problem in connection with the hypothesis is to specify the circumstances under which the formula works or, more precisely, the general magnitude of the error in its predictions under various circumstances. Indeed, . . . such a specification is not one thing and the hypothesis another. The specification is itself an essential part of the hypothesis, and it is a part that is peculiarly likely to be revised and extended as experience accumulates. (Friedman 1953, 18)

Summarizing Friedman's strategy of finding explanations, a hypothesis or theory should consist of three parts: first, a model containing only those forces that are assumed to be important—in other words each model implies negligibility assumptions; second, a set of rules defining the class of phenomena for which the model can be taken to be an adequate representation—these are (independent) domain specifications; and third, specifications of the correspondence between the variables or entities in the model and observable phenomena.

Friedman is not an antirealist, he only opposes the approach in which models are aimed as 'photographic reproductions', which he unfortunately labels as 'the realism of its assumptions.' By a realistic assumption he means a comprehensive as possible description of reality. The uselessness of such striving for realisticness was illustrated in a hyperbole:

> A completely 'realistic' theory of the wheat market would have to include not only the conditions directly underlying the supply and demand for wheat but also the kind of coins or credit instruments used to make exchanges; the personal characteristics of wheat-traders such as the color of each trader's hair and eyes, his antecedents and education, the number of members of his family, their characteristics, antecedents, and education, etc.; the kind of soil on which the wheat was grown, its physical and chemical characteristics , the weather prevailing during the growing season; the personal characteristics of the farmers growing the wheat and of the consumers who will ultimately use it; and so on indefinitely. (Friedman 1953, 32)

So, "the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic', for they never are, but whether they are sufficiently good approximations for the purpose in hand" (15).

To clarify Friedman's position, a comprehensive explanation of the motion of a falling body can be represented by the above-mentioned equation (*2*):

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$$\Delta y = \Delta F(x_1, x_2, \ldots) = \partial F/\partial x_1 \cdot \Delta x_1 + \partial F/\partial x_2 \cdot \Delta x_2 + \ldots \quad (2)$$

Suppose that y is the motion of a body, x_1 is gravity, x_2 air pressure, x_3, x_4, . . . are other specifications of the circumstances (e.g. temperature, magnetic forces). The law of falling bodies says that in vacuum ($x_2 = 0$, but the notion of 'vacuum' in this law in fact also imposes that interference by other disturbing causes are absent: $x_3 = x_4 = \ldots = 0$) all bodies fall with the same acceleration, regardless of mass, shape or composition: $\partial F/\partial x_1$ is equal for all bodies. However, in the open air, the shape and the substance of the falling body determine which of the interfering factors can be considered as having negligible potential influence (i.e. $\partial F/\partial x_i \approx 0$). For example, air resistance is negligible for compact balls falling relatively short distances, so they behave as if they are falling in vacuum. However, for feathers the air pressure does interfere. Similarly, magnetic forces act on steel balls and not on wooden balls, etc. To conclude, one has to define the class of phenomena for which a specific model is an adequate representation.

Musgrave (1981) conjectured a chronological ranking in the use of the assumptions: "what began as a negligibility assumption may be changed under the impact of criticism first into a domain assumption, then into a mere heuristic assumption; and that these important changes will go unnoticed if the different types are not clearly distinguished from one another" (386). In contrast to this view, my reading of Friedman's methodology is that the model based on negligibility assumptions should be maintained and that it is the domain of phenomena for which this model holds that should be explored empirically. Friedman advocated a Marshallian partitioning, however not on the basis of ceteris paribus assumptions as generally is assumed, but according to a combination of negligibility assumptions and domain specifications.

5. Herbert Simon's Hierarchical System Approach. While Friedman avoided the problem of the complexity by Marshallian partitioning and only focusing on some parts, Herbert Simon dealt explicitly with complexity. Although he used the same method of partitioning—as will be shown below—the interaction between the subsystems was an essential part of his analysis.

Very early in his career Simon already found that the description of a very complex system can be simplified by considering them as hierarchic, a strategy that can be found in several of Simon's articles dealing with complexity. The first mention of the idea of hierarchical systems as representations of complex systems, like the human mind, was in a comment on John von Neumann's talk, "General Theory of Automata", at the

Harvard Meeting of the Econometric Society in 1950. In his talk Von Neumann warned against taking the brain-computer analogy too literally. Simon (1951) who was discussant in this session on the theory of automata, observed that "the significant analogy was not between the hardware of computer and brain, respectively, but between the hierarchic organizations of computing and thinking systems" (Simon 1977, 180).

The paper presented by Von Neumann was his paradigm paper, "The General and Logical Theory of Automata" (1951), in which the top-down approach of Artificial Intelligence to deal with complexity for the first time was introduced. In that paper, he explicated what he labeled as the Axiomatic Procedure:

> The natural systems are of enormous complexity, and it is clearly necessary to subdivide the problem that they represent into several parts. One method of subdivision, which is particularly significant in the present context, is this: The organisms can be viewed as made up of parts which to a certain extent are independent, elementary units. We may, therefore, to this extent, view as the first part of the problem the structure and functioning of such elementary units individually. The second part of the problem consists of understanding how these elements are organized into a whole, and how the functioning of the whole is expressed in terms of these elements. (Von Neumann [1951] 1963, 289)

The first part of the problem belonging to the relating discipline, in this case physiology, could be removed by the 'process of axiomatization':

> We assume that the elements have certain well-defined, outside, functional characteristics; that is, they are to be treated as 'black boxes.' They are viewed as automatisms, the inner structure of which need not to be disclosed, but which are assumed to react to certain unambiguously defined stimuli, by certain unambiguously defined responses. (Von Neumann [1951] 1963, 289)

Simon's (1962) "The Architecture of Complexity" is an elaboration of Von Neumann's Axiomatic Procedure. The central thesis of this article is that complex systems frequently take the form of hierarchic systems. A hierarchic system is a system that is composed of interrelated subsystems each, in turn, hierarchic in structure right down to the lowest level of elementary subsystems. Each subsystem can be treated as a 'black box' where the internal structure is irrelevant and only the inputs and outputs are of interest. Therefore, there is inevitably some arbitrariness, related to the researcher's interest, as to when partitioning is necessary and what subsystems are assumed to be elementary.

If one distinguishes between weak and strong interactions and partitions the complex system where interactions are weakest, the analysis of that system can be tremendously simplified. This result was found in a paper, which dealt with the problem of aggregation, entitled "Aggregation of Variables in Dynamic Systems," co-authored by Alfred Ando, and published in 1961. The aim of this paper was "to determine conditions that, if satisfied by a (linear) dynamic system, will permit approximate aggregation of variables" (114). It appeared that aggregation could be performed in 'nearly decomposable' systems. The notion of 'near decomposability' was clarified by the definition of a decomposable matrix. When a matrix can be arranged in the following form:

$$P^* = \left\| \begin{matrix} P_1^* & & & & \\ & \ddots & & & \\ & & P_i^* & & \\ & & & \ddots & \\ & & & & P_n^* \end{matrix} \right\| \tag{7}$$

where the P_i^*'s are square submatrices and the remaining elements, not displayed, are all zero, then the matrix is said to be completely decomposable. A nearly decomposable matrix is the slightly altered matrix P:

$$P = P^* + \varepsilon C \tag{8}$$

where ε is a very small real number, and C is an arbitrary matrix of the same dimension as P^*.

The main theoretical findings of the analysis of the structure of dynamic systems represented by nearly-decomposable matrices were summed up in two propositions, which were also mentioned in slightly more general terms in "The Architecture of Complexity":

> (a) in a nearly decomposable system, the short-run behavior of each of the component subsystems is approximately independent of the short-run behavior of the other components; (b) in the long run, the behavior of any one of the components depends in only an aggregate way on the behavior of the other components. (Simon 1962, 474)

By considering a complex system as nearly-decomposable the description of the system can be simplified: only aggregative properties of its parts enter into the description of the interactions of those parts (Simon 1962, 478).

Simon's approach of simplifying the analysis of complex systems by treating them—whenever possible—as nearly-decomposable systems can be considered as a Marshallian partitioning, but not by using a sharp ceteris paribus razor but a blunt knife of negligibility assumptions. This kind of partitioning can be very helpful in exploring the kind of issues that interest social scientists, but which are not suitable for controlled experiments, "social scientists must use the data generated by a single, complex, uncontrolled experiment that is the history of society in its entirety" (Ando 1963, 1). In Simon's terminology, laboratory experiments are a way to artificially create a decomposable structure: one is able to control the ε of equation (*8*) and to fix them at zero. Unfortunately, this is not applicable to most social phenomena. "However, nature is not completely unkind to social science" (2). Many of the situations can be represented by nearly-decomposable systems.

The question about this approach is: In what sense are the results valid when one of such 'nearly' unrelated subsystem is analyzed as if it exists in complete isolation? How negligible is the environment of this subsystem? Simon and Ando's (1961) analysis shows that for predictions of the behavior of that subsystem within a given degree of accuracy there is a trade-off between time interval over which the accuracy of prediction will be maintained and the degree of nearness of the system to a really isolated system. For example, the shorter the time a ball falls, the more air resistance can be neglected.

To use macroeconometric models for policy evaluation, one has to know the properties of the model that are invariant under policy changes. Simon located invariance at the decomposed elementary level. Simon expected each small box to contain only a simple relationship. And, more importantly, "a simple hypothesis that fits data to a reasonable approximation should be entertained, for it probably reveals an underlying law of nature" (Simon 1968, 448). In other words, simple correlations have a higher probability of being autonomous. This expectation was supported by Harold Jeffreys' (1948) simplicity postulate in Bayesian reasoning. Simon argued the following. If one attaches a high a priori probability to the hypothesis that the world is simple (i.e., that the facts of the world, properly viewed, are susceptible to simple summarization and interpretation): P(simple law) is high; and if one assumes that simple configurations of data are sparsely distributed among all logically possible configurations of data: P(simple configuration of data) is low; then a high posterior probability must be placed on the hypothesis that a simple configuration of data in fact reflects approximations to conditions under which a simple law of nature holds:

$$\mathrm{P}\left(\text{simple law}\,|\,\text{simple conf}\right) = \mathrm{P}\left(\text{simple conf}\,|\,\text{simple law}\right) \cdot \mathrm{P}(\text{simple law})/\mathrm{P}(\text{simple conf}) \tag{9}$$

where P(simple conf | simple law) = 1.

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Jeffreys' strategy of starting with simple models was built on by the econometrician Arnold Zellner. Like Friedman he was dissatisfied with the poor ability of large-scale macroeconometric models to explain and predict, in comparison to the naive models. His strategy is to start with models as simple as possible and improve the model each time in the direction indicated by all kinds of diagnostic checks on the properties of the model. Because naive models perform better in prediction than complicated models, he suggested starting with this kind of simple models. According to Jeffreys the choice of the simplest form is not a matter of convention, but "because it is the most likely to give correct predictions" (Jeffreys 1948, 4). Zellner's approach was based on Jeffreys' suggestion that if there are no effective models available to explain a phenomenon, a sophisticatedly simple initial model is that all variation is random (naive model I) unless shown otherwise (Zellner 1988). In an unstable environment, a relationship that predicts better than another relationship is more autonomous than the other one. Thus, when simple relationships are more likely to give correct predictions in unstable environments, then simple relationships have a higher probability of being more autonomous than comprehensive relationships.

When a scientist "finds that the 'facts' summarized by a simple, powerful generalization do not fit the data exactly, his first reaction is *not* to throw away the generalization, or even to complicate it by incorporating additional terms" (Simon 1968, 442). Instead, his explorations would move in two directions: "(1) toward investigations of his measurement procedures as possible sources of the discrepancies and (2) toward the identification of other variables associated with the deviations" (442). However, in contrast to Haavelmo's method of incorporating these other variables into the model, Simon describes the modeling process in a similar way to the strategy expounded by Friedman: the scientist would narrow the empirical generalization by stating the limiting conditions under which it is supposed to hold.

But this process of inference from the facts does not stop with these two stages of (1) finding simple generalizations that describe the facts to some degree of approximation; and (2) finding limiting conditions under which the deviations of facts from the simple generalization might be expected to decrease; but continues to (3) explaining why the simple generalization should fit the facts, e.g., Newton's gravitational explanation for Galileo's law.

According to Simon, one should stick to the simple generalization, even when an object's behavior is complex, and not make the generalization more complex accordingly. A simple generalization is more likely to reveal a lawful relationship. In the example of the falling feather, the environment should hold the explanation for the complex behavior of the feather.

In other words, the complexity of the feather's fall is a reflection of the complexity of the environment—turbulence—and not of the law of gravity. To support this claim he used the metaphor of the route an ant has to take to cross a beach on the way to its home:

> We watch an ant make his laborious way across a wind- and wave-molded beach. He moves ahead, angles to the right to ease his climb up a steep dunelet, detours around a pebble, stops for a moment to exchange information with a compatriot. Thus he makes his weaving, halting way back to his home. (Simon 1969, 23)

The ant has a general sense of where home lies, but he cannot foresee all the obstacles that he will encounter on the way. Thus, the ant's path is irregular, complex and hard to describe, but its complexity is a reflection of the complexity of the surface of the beach, not of a complexity in the ant.

> An ant viewed as a behaving system, is quite simple. The apparent complexity of its behavior over time is largely a reflection of the complexity of the environment in which it finds itself. (Simon 1969, 24)

The kind of partitioning Simon aimed at was to decompose the system into a hierarchical system until a level is reached where the elementary units ('axioms' in Von Neumann's terminology) are bound only by simple relationships. Their simplicity implies that they probably represent autonomous relationships.

6. Nomological Machines. According to Cartwright, for fixed patterns we need stable environments: nomological machines. To build them we must be able to control the circumstances, which is possible (always only to a certain extent) in physics but highly problematic in economics. However, invariant relationships (in both economics and physics) are not always the result of ceteris paribus environments but could also occur because the influence of the environment is negligible, in other words invariant relationships could also be *ceteris neglectis* regularities, empirical relations that are as far as possible autonomous.

The ceteris neglectis condition can be clarified by the same equation (*2*) used before to discuss autonomy:

$$\Delta y = \Delta F(x_1, x_2, \ldots) = \partial F/\partial x_1 \cdot \Delta x_1 + \partial F/\partial x_2 \cdot \Delta x_2 + \ldots \qquad (2)$$

where x_i denotes a causal factor of y. Suppose we would like to investigate whether there is an invariant relationship between y and x_1. The machine should be designed such that the behavior of y is sensitive to changes in x_1 and at the same time insensitive to changes in the other circumstances (OC).

$$\Delta y = \Delta F(x_1, OC) = \partial F/\partial x_1 \cdot \Delta x_1 + \partial F/\partial OC \cdot \Delta OC \tag{10}$$

where OC is a collective noun of all the other factors, x_2, x_3, . . . , that should have a negligible influence on y. This condition implies requirements on $\partial F/\partial x_1$ and $\partial F/\partial OC$, namely $\partial F/\partial OC$ must be negligible compared to $\partial F/\partial x_1$. In other words, the empirical relation should be autonomous as possible. If we can design a machine based on an autonomous relationship, we do not have to worry about the extent to which the other circumstances are changing (ΔOC). We can allow the other circumstances to change; they do not have to be controlled as is assumed in the conventional ceteris paribus ($\Delta OC = 0$) and ceteris absentibus ($OC = 0$) conditions.

Haavelmo did not discuss autonomy in terms of nomological machines but in terms of designing experiments, but there are two kinds of experiments: the controlled experiments carried out by us, and the others by Nature which we only can passively observe. The relevant question here is: Are the experiments turned out by Nature good experiments, in the sense that we can infer from them invariant generalizations? In nature, everything is connected and constantly changing. Fortunately, not every connection is of relevance, many are negligible. Because invariance is a domain-related feature of empirical relationships, the problem is: How autonomous are the empirically founded relationships consisting of a limited number of non-negligible connections. Exceptionless generalizations like 'All men are mortal' are highly autonomous, but they are scarce (Cartwright 1983, 46; see also Woodward 2000, 228).

7. Autonomy versus Precision. Economists use models to evaluate different kinds of policy measures. Therefore, they require their models to predict well. Every evaluation of a policy measure is a kind of prediction. They would like to use models for counterfactual analyses, therefore they require that their models contain autonomous relationships, that is the model equations should be invariant for a range of policy interventions. At the same time, using models for policy evaluations means that economists aspire to preciseness of the models' predictions. However, there is a tension between autonomy and preciseness. As long as generalizations like 'If I drop it, it falls' remain imprecise they are almost exceptionless, and thus highly autonomous (Hoover 1997, 14). However, if we would specify the 'it' as a feather or a bank note—Neurath's example of a thousand dollar bill swept by the wind on Saint Stephen's Square—as Cartwright (1999, 27) did, it becomes clear that any relation that is used to predict when or where it hit the ground can hardly be autonomous. Environmental conditions such as turbulence are very significant. So-called exceptions (when the dropped object does not fall to the ground, because for example it gets caught in a bush) are caused by the environment of the object and does not contradict the generalization 'If I drop it, it falls' itself. Precise predictions are based on combinations of relationships like the above imprecise, autonomous relation and those that describe the specific circumstances. As a result, these combined relationships are more confluent, and thus less autonomous, but more precise.

Haavelmo's advice was to incorporate as many potential influences as possible into the model to achieve the highest degree of autonomy. The researchers at the Cowles Commission, assuming that their model equations were autonomous, strived for more comprehensiveness to achieve more precise predictions. They recommended building models in which turbulence is taken account of. The result was that the model's equations became lesser autonomous. Their striving for preciseness went at the cost of autonomy.

Friedman propagated an opposite strategy by recommending to start with modeling those phenomena in which the environmental circumstances are less influential. When and where a very heavy ball hits the ground when thrown from the tower of Pisa can be predicted quite precisely. Turbulence, wind or even a bush standing in the way, do not matter. A good (fall) experiment is one carried out with a heavy ball and not with a feather. But are the equivalents of heavy balls to be found in economics? They are scarce. That is, there are probably only a few examples of phenomena for which the influential factors are so dominant that they push other influences aside.

According to Simon, the simplicity of a relationship is an indication for its lawfulness. His advice is to 'decompose' the falling object from its environment to simplify the analysis. The shorter the period that a prediction applies to, the less influence the environment has and thus the more accurate the prediction becomes.

8. Conclusions. The different cases of economic practice discussed in this paper show that economists have developed different strategies to infer invariant relationships from passive observations. With the notion of autonomy, Haavelmo provided the framework for dealing with invariance outside the laboratory. He came to the conclusion that the problem of autonomy could be solved by economic theory. As a result, autonomy disappeared from the Cowles Commission research agenda. Members of the Cowles Commission strived for comprehensiveness to attain preciseness.

Friedman reintroduced invariance on the agenda by criticizing the belief that more autonomy could be achieved by more comprehensiveness. He showed that the problem of autonomy was an empirical problem. Invariant generalizations should not be assessed by exploring the domain of changes for which they hold, but by investigating for which phenomena they hold. However, these domain specifications should not be incorporated into the model equations, like in the Cowles Commission program, but should be specified independently.

Simon showed how from comprehensive models invariance could be inferred. Decomposing a system where the interactions between subsystems are negligible in the short run might lead to simple relationships that have a high probability of being invariant.

In general, philosophers link the possibility of finding lawful relationships with the ability of doing controlled experiments and therefore tend to be pessimistic about finding these relationships in the social sciences. This paper has discussed different strategies that question the presupposition of the necessity of laboratories. The first strategy replaces this presupposition with the presupposition that theory will solve the problem of autonomy, the second with the presupposition of the existence of phenomena that can be described by simple invariant relationships, and the third with the presupposition that laws of nature are simple.

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