Criticism of causal-scientific social research

Investigations into the foundations of sociology and economics

Hans-Hermann Hoppe

This work, which is primarily committed to the work of the economist L. v. Mises, attempts, on the one hand, to provide a non-empirical justification for the thesis that **causal scientific social research is impossible**.

Central to the justification of this thesis is the proof that the statement that one conducts causal research as a scientist of acting is logically incompatible with the statement that one can learn, which is implicitly recognized as valid by every scientist and cannot be disputed without argument.

In addition, the attempt is made to present the double consequences that arise from this evidence for the logic (and also the research practice) of the science of acting:

- As an empirical discipline, social research must be understood as a reconstructive science that, in analogy to the linguistic analysis of language, breaks down actions into the logical components on which they are based, which are in principle recognizable as such by the actor.
- In addition, social research can be conducted as a non-empirical discipline (as a pattern of which economics, illustrated by some of its theorems, is presented), the statements of which are not verified on the basis of experience data, but, starting from a priori given premises, solely on the basis of logical-conceptual analyses.

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Preliminary remarks

The present work was originally intended to look different from what it now looks like:

The plan was to conduct a scientifically logical evaluation of more complex social science statistical techniques for theory and model construction. It should be examined how different strategies of "causal modeling" are to be assessed in view of the scientific-theoretical desideratum of the strictest possible testability of theories.

It was assumed that these strategies differed in some cases significantly with regard to testability levels - but it was assumed that what they all have in common was unproblematic: the assumption that there could be causal research in the social sciences.

However, in the course of the work, for reasons detailed in the present study, the epistemological unsustainability of this assumption emerged.

The original plan had to be abandoned.[1]

-- Instead, the following three treatises, each self-contained but forming a thematic unit, are shown:

(1) empirical-causal-scientific social research is logically impossible, i.e. it implies an unavoidable logical contradiction to claim that one conducts causal research as a social scientist;
(2) empirical social research must rather be justified as a reconstructive science that has its paradigm in grammar;
(3) economics is not an empirical social science, but must be understood as an a priori science of action, whose statements are not verified by empirical data, but are logically derived.

All three treatises attempt to justify thesis (1) from different perspectives. At the same time, each of the three treatises also attempts to present and reject the unified scientific program, which thesis (1) implicitly opposes, in what we believe to be a novel form as a logically contradictory approach.
Thesis (2) and (3), which formulate the double consequences resulting from the rejection of the unified science program for the logic of the sciences of acting, are separately substantiated in Chapter 2 and 3:
Chapter 2 deals with the consequences of the impossibility of empirical-causal-scientific social research for a science of action in terms of empirical science, and shows to what extent empirical social research can only be reconstructive (and not: predictive) research.
Chapter 3 demonstrates the extent to which the proof of impossibility with regard to empirical-causal-scientific social research leaves room for the possibility of a social science qua

[1] My treatises give an impression of the 'type' of work that was originally envisaged: On How Not To Make Inferences About Measurement Error, Quality and Quantity, 14, 1980; On the use of unmeasured variables in causal models. An epistemological critique, Zeitschrift für Soziologie, Heft 3, 1981; About unmeasured variables. Of a False Conclusion and Two Unanswered Questions, Zeitschrift für Soziologie, Heft 1, 1982

aprioristic (logical-analytical, pure) science of action, discusses its logic, and analyzes economic theorems as an example of this type of science.
To avoid misunderstandings: In connection with thesis (3), it is not claimed that everything that operates under the title 'Economics' is actually statements of pure action theory or even just statements that could be reformulated as such.

On the contrary, much of what economics means is 'only' empirical, i.e. reconstructive social research and cannot be more than that in principle. Rather, it is argued that of the social science disciplines, it is above all economics that, as a rule unconsciously to the economists themselves, represents an indeed impressive fundus, pure (non-empirical) or indeed action theory that can be reformulated as such; that the often observed higher level of scientific development of economics, especially in comparison to sociology, can be explained exclusively from this status of economics qua pure action science and can be secured and expanded solely by recognizing this fact; and that, on the other hand, any attempt to justify the relative superiority of economics by referring to an empirical-causal-scientific research methodology that is carried out particularly consistently within its framework (e.g. econometrics) must necessarily fail, because the conception of an empirical-causal-scientific action science is contradictory in itself.)[2]

The method of the entire work is, as far as I was able, that of a strict, logical argumentation: discursive reasoning and deductive derivation.
I would not have been able to find the time required to prepare this study without the support I have received from several sides: I would like to thank the German Academic Exchange Service and the University of Michigan, the German Research Foundation and, above all, Prof. Hondrich.

[2] Psychological insights of outstanding importance in gaining this insight were the works of L. v. Mises, as well as of K. R. Popper. From Mises comes the insight into the double consequences for the action sciences resulting from the rejection of the unified science program: in terms of empirical discipline, they are only (reconstructive) history, and in terms of "explanatory theory", they are logic of action ("praxeology"). However, Mises himself gives no irrefutable reason for the decisive first step in the argument: the rejection of the unified science program. In our opinion, Popper presents this reason, as it were against his will. He, in turn, does not recognize the far-reaching consequences resulting from it with regard to the logic of the sciences of acting. (The psychological sources of insight indicated here can best be rediscovered in the following systematic studies in the explanations on pages 41-49!)

Chapter 1

On the Impossibility of Causal Scientific Social Research

I

Promoted by the widespread belief that 'the more mathematics, the more scientific', there has been a tendency for some time in economics, psychology and finally sociology to increasingly use and complicate statistical techniques within the framework of social scientific practice.

Countering this tendency must seem more and more like don quixotry, especially since the increasing specialization of pure methodologists and the prestige they have gained have led to a situation in which it is increasingly difficult for the critic to prove that he understands what he is criticizing in detail at all.

In view of whether one is satisfied or not, one must not only prepare oneself for a protracted struggle with countless individual attacks, but each criticism must also, according to the rules of the game defined by the methodologists, take its starting point from a concrete presentation of approved techniques that proves an understanding, only then - with their assumed internal consistency - to demonstrate that their application requires assumptions that can in principle be understood by everyone as logically nonsensical.

In the following, such a criticism will be presented.
As it were, in a general attack, the method of social-scientific causal analysis will be targeted: we are concerned with proving that the application of the technique of causal research requires assumptions that, if only recognized as such, would inevitably lead to the admission that this technique is actually not applicable in a precisely definable area (that of the social sciences) in which it has hitherto been used uncritically, since the necessary prerequisites for its application are lacking.

This applies, as will be shown, to all techniques that allow constants to be calculated from a given set of data, by means of which a given variable, regarded as dependent, is placed in a lawful (functional) relationship with other variables.

Whether this relationship is of a linear or non-linear nature, whether there are one or more independent variables, whether - as in time series analyses - the dependent variable itself (time-shifted) also functions as an independent variable, whether the relationship is of a recursive or non-recursive, deterministic or statistical nature, etc., is indifferent: the criticism refers to all techniques (from simple linear regression to the comparatively complex methods of time series analysis), provided that *constants* (including those that assume variable values according to a constant pattern) are calculated there.

The fact that the use of such techniques is on the rise hardly needs to be emphasized any more: within the framework of economics, econometrics as the home of all these techniques[1] is constantly gaining ground - despite the criticism of representatives of pure economic theory[2] and

also in sociology, one increasingly expects - greatly encouraged above all by the work of Blalock and Duncan[3] - all salvation through thorough econometricization.

II

For the purpose of greater illustration of the following argumentation, it may be assumed that, on the basis of a given set of data, the constants b1 and b2 of the multiple regression equation

$$Y = a + b1X1 + b2X2 + e$$

where Y - the dependent variable - is understood as a linear function of the independent variables X1 and X2 as well as an error variable e with an average value of 0.
This equation or, better, the b-constants appearing in it can be interpreted in two ways:
You can give them a harmless, but completely unusual interpretation, or you can give them the 'normal' interpretation, which is no longer harmless, but must rather implicitly presuppose assumptions whose inappropriateness will have to be demonstrated.

The harmless interpretation states:
The partial regression coefficients (as well as the a constant, of course) represent no more than a (verifiable) determination of how, in a given, completed, i.e. historical set of data, the Y values can best be predicted on the basis of the X1 and X2 values (as well as the linearity assumption, plus the assumption that they are additive effects).
The constants are historical-mathematical facts. Beyond the historical data on which they were calculated, they have no meaning whatsoever:
neither does their calculation imply the (falsifiable) general hypothesis of a corresponding universal relationship between Y on the one hand
and X1 and X2 on the other, nor that there is any constant, lawful relationship between these variables at all.

It is difficult to object in principle to such a modest interpretation;
however, according to it, the establishment of an equation such as the one given above also becomes an event of highly subordinate scientific importance: this was not the establishment of a theoretical theorem, but merely a description of facts, even more so of a kind that - with a given data set, varying assumptions about function types and with computer support - can be generated in any quantity in the blink of an eye.

In fact, there is probably no one who has carried out e.g. a regression analysis, and does not think it can do more than this harmless interpretation provides for.

Without exception, it can be confidently asserted that a different interpretation is used instead:
The setting up of the above equation is interpreted as a formulation of a general hypothesis that can of course be falsified on the basis of new data entering the horizon of experience, but not: verifiable, which asserts the relationship between Y and X1 and X2 as universally valid, specifically determined by the constants of the equation.

III

The problematic (implicit) assumptions that this interpretation requires can be reconstructed from the following situation: An attempt has been made to replicate the results obtained on the basis of the analysis of a first data set and recorded in the form of the above equation with precisely defined constants on new or different data. This experiment ends with the fact that the multiple linear regression analysis carried out for the Y and the X1 and X2 variables of this second dataset yields b constants that deviate significantly from those determined for the first dataset.

According to the harmless interpretation of such equations, such an event would have no consequences: first one historical-mathematical fact was determined, now another, both are different - that's all! According to the normal interpretation, however, the original hypothesis has been falsified with the failed replication: it has been proven that it cannot be universally valid.

What is the condition of the possibility that one can say 'falsified'?
Such a statement necessarily presupposes the intellectual principle of 'equal cause, equal effect' or 'unequal effect, unequal cause', by means of which the possibility of contingency (variability) in the effectiveness of causes is categorically excluded, and instead - just as categorically - lawfulness (constancy) is assumed for observable processes.
This principle cannot be falsified by experience, because it can never be ruled out in principle that one cannot indeed find an unequal' cause in principle for an effect determined to be unequal; and the principle cannot be confirmed by experience, because the conclusion of the determination of two equal effects on the same causes is only consistent if one assumes the constancy principle as valid beforehand; but if one does not assume it, but allows the possibility of contingencies in the effectiveness of causes, then logically nothing follows from the determination of equal effects with regard to the causes: and the proof now required to confirm the constancy principle that actually all the events preceding the two equal' effects in time were also the same, since this required the search of the entire universe, in principle irrecoverable.

The principle of constancy is thus independent of any experience, and one can call it "a priori" insofar as words are of course not important. However, although it cannot itself come from experience and can be refuted by it, it is itself a condition of the possibility of experiences that, in contrast to isolated and randomly juxtaposed experiences via empirical (historical) facts, can be related to each other as mutually confirming or contradictory.[4]

Only the previous assumption of constants in the course of observable phenomena leads to the fact that there is or can be such a thing as 'events requiring explanation' and, as a result, possible learning (i.e. a possible overhaul of old by new experiences); because only if one categorically assumes 'constancy' can an event sequence be interpreted as an apparent contradiction to this principle and a question about the unequal' causes logically necessarily arise.

So, to return to the starting point of the discussion, if the failed replication, according to the 'normal' interpretation of the regression equation, is understood as a falsification' of the original hypothesis; and if one is consequently prompted to explain the different size of the b constants by the fact that in one sample one or more factors F were (implicitly) effective in causing Y, which were not, or not equally effective in the other sample; and if one is finally prompted to explain these factors F (hypothetically) and to incorporate them into the original hypothesis, which only assumed a systematic effectiveness of X1 and X2, in order to replace in this way an old hypothesis with a (hopefully) improved new hypothesis - all this is only possible if one has already assumed the principle of constancy for Y and all factors causing Y.

IV

We now claim that there is an object area that cannot be constituted as an object area in which laws' prevail, and that consequently no behavioral equations (such as the above regression equation) that could be given a 'normal' interpretation can be formulated for this area.

Since the validity of the constancy principle cannot be questioned due to external, sensory experience, as shown, it can only be internal' experience facts, i.e. logical reasons, that are responsible for the inapplicability of the constancy principle in this area. However, there can only be logical reasons for the inapplicability of a logical principle (i.e. a pure principle of the mind) with regard to mind operations themselves. It is indeed the area of pure or applied cognitive performance, i.e. the area of linguistically formulated scientific and pre-scientific findings or experiences and the area of human actions, which in terms of intentional actions[5] make use of such (explicable) cognitive performance, for which the validity of the constancy principle cannot be assumed without being involved in inevitable logical contradictions: while the behavior of the objects of external experience can never contradict the constancy principle, its validity in the area of human recognition and action is inconceivable.

Assuming the application of the principle of constancy to the field of human cognition and action, this implied, as will soon become even clearer, (logically) the following assertion:

> I (and every other person) know exactly and fully what I will ever know at any given time; and my actions, which make use of this already known knowledge, are carried out according to a precisely anticipatable pattern; no new situations that cannot already be described in the present occur, no new goals of knowledge or action that are not already known, and the same means are always used in the same temporal pattern of repeating, in always the same situations with always the same goals.[6]

Everyone knows and knows this with greater certainty than the knowledge of even the most elementary logical and mathematical sentences can ever convey that this assertion is incorrect with regard to human cognition and action.[7] Here, like the truth of logical and mathematical theorems, the truth of the theorem that people do not learn (that they do not learn is assumed in the state presented above) is independent of contingent empirical experiences: If they could not learn, then of course the claim that they do not learn could never be falsified, because for this one would have to be able to learn from experience, but one can unfold the claim that people cannot learn with regard to all its implications and determine its validity or non-fitness in this way - as done here - so this already shows that the claim is false and one can indeed learn.

The proposition that people learn applies a priori: one cannot dispute it argumentatively without having already presupposed it; because to defend such a thesis as an argument meant to admit the possibility of a discussion (whether with oneself or with other people) with regard to the validity of the statement, i.e. the possibility of answers understood as contingent, which would be nothing other than the concession of the fact of learning ability.

This proves by way of an argumentum a contrario that the principle of constancy cannot apply in the context of human action: If it were to apply, this would mean that one cannot learn - but that one can learn, that one cannot learn, cannot be claimed without having already contradicted oneself.

V

The same result can also be achieved directly: from the premise that people can learn and the development of its implications. It follows from the mere recognition of this sentence, which is completely indisputable on the basis of empirical arguments, that my knowledge (both about external nature, other persons, and myself) and my actions (insofar as they make use of this my knowledge in terms of intentional actions) differ or do not differ at a given point in time t1 from that or those at a second, later point in time t2 in such a way that it is logically impossible for me to predict at point in time t1 whether, and if so, in what respects my knowledge or my actions will have changed at point in time t2.

If I can learn, it means that I do not already know now what I will know later, and just as I will not act later, with my then given knowledge.[8] If, on the other hand, I were able to predict today what I will only know tomorrow, and if I knew today how I will act tomorrow (with the knowledge of tomorrow), the changes to which my knowledge and actions are subject over time would follow a constant pattern that I already knew in the present and could anticipate exactly, i.e. I did not learn - rather, I would always know what I will ever know.

What applies to me applies mutatis mutandis to other persons as well as to me in relation to them, as well as to them in relation to me:

If I impute learning ability to other persons (and vice versa to me) - and this is necessarily done reciprocally whenever one speaks and argues with each other or when one assumes that one can speak and argue with each other - their future states of knowledge and the corresponding actions are logically unpredictable for me, because they could learn from what I have learned (which I cannot predict); and conversely, they cannot predict my future knowledge and my corresponding actions, because I could learn from what they have learned (which, in turn, they cannot predict).

It is therefore generally true that a society of learning subjects - even if one assumes that they have interconnected their brains and know, each for themselves, at any given time, what everyone else knows[9] - cannot predict their own future states of knowledge and the corresponding social interaction of actions for logical reasons.

From the proposition that future states of knowledge and corresponding processes of action - whether with regard to one's own person or with regard to other persons - cannot be predicted (and not for practical reasons, but for logical reasons), it immediately follows that, of course, the current state of knowledge and action was not predictable from the point of view of the past (even if it was from the immediate past). Our present knowledge is explicable only at the moment in which we have it, and our intentional actions are explicable only at the moment in which they are actually carried out, and in the same breath: our future knowledge can only be explicable in the future, and our intentional actions that have not yet been carried out can only be explained or understood when they actually take place; and finally also: the transition from one state of knowledge to another and, quite correspondingly, the transition from one intentional action to another, following in time, as well as the constants or changes that are documented in such transitions, can only be reconstructed subsequently, after a transition that has actually already been carried out. Anyone who claimed that he could 'do more' (i.e.: predict), claimed the possibility of the logically impossible.[10]

To say: states of knowledge and actions (in which certain states of knowledge become manifest) or the change or the constancy of such states and the corresponding actions, if we have to consider ourselves - and this necessarily - as subjects capable of learning, cannot be predicted, but only explained or reconstructed ex post, is now equivalent to saying:

The principle of constancy does not apply in the field of human cognition and action; we cannot claim without putting ourselves in logical contradiction to the a priori valid proposition that we can learn that constant, time-invariant causes can be found for a state of knowledge or an intentional action, which made the same predictable;

and we likewise cannot claim that it is possible to explain a change in knowledge or action (according to the principle of constancy) ex ante by showing certain 'unequal' causes.

VI

Constituting oneself as capable of learning logically implies that one ceases to regard one's thoughts and actions as 'caused' in the sense of constituting the objects of nature as caused. The assumption of learning ability rather implies the recognition of the proposition that - contrary to the statement contained in the constancy principle - one can recognize or act differently under the same conditions, and, mutatis mutandis, under unequal conditions. To think of oneself as capable of learning means having to assume a contingency in the effectiveness of causes with regard to one's own recognition and action:
who is capable of learning, whose behavior (recognition and action) cannot (for purely logical reasons) be regarded as determined by constants or by a causal complex of constant (be it statistical or deterministic) effectiveness.
Constants with regard to the causation of events can logically only exist where one is dealing with an object area of non-learning objects or, more correctly, where one first constitutes an object area as an area of non-learning objects.
However, one cannot even think of oneself as non-learning: not only is a mind operating according to the scheme expressed in the principle of constancy necessarily a learning mind (we learn about the behavior of ourselves as non-learningly designed objects)[11], the assertion that one does not learn cannot even be presented as an argument without having already implicitly given it up.

No advance in knowledge, however dramatic it may seem, can ever change the fact that one must regard oneself, one's recognition and action, as 'uncaused'.
One may consider this idea of 'freedom' to be an illusion, and from the point of view of a 'scientist' with a cognitive ability clearly superior to human intelligence, from the point of view of God, for example, such a characterization may also be quite correct:
he may be able to indicate causes of constant effectiveness for each state of knowledge, for each action, and for each transition from state to state or from action to action
- only: we are not God, and even if our 'freedom' is an illusion from his point of view, for us humans it is in any case a (mentally) necessary illusion[12]:
We cannot predict our knowledge and our knowledge manifesting action due to previous conditions.

VII

Let us now return to the regression equation at the beginning of the current discussion,

$Y = a + b1X1 + b2X2 + e$

and in this way close the circle of reasoning:
The dependent variable, Y, occurring in this equation is an arbitrary intentional action (or also, if it is an aggregate variable, a variable that is logically necessary based on intentional actions), i.e. an action that, starting from the inner and/or outer perception of a certain initial situation, tries to realize a specific, imagined goal that is preferred to the initial situation and to other alternatives by using certain means (successful or not), and which in this respect manifests 'knowledge' in three respects:

Knowledge of the existence of a certain initial situation,
Knowledge by the idea of a specific action goal that is considered feasible, and
Knowledge of the means considered suitable for achieving the goals.

With the establishment of such an equation or with the calculation of the constants occurring in it on the basis of a certain set of data, it is now asserted, at least according to the 'normal' interpretation:

"There is a complex of causes that produces effect Y with constant effectiveness, and it is possible, based on knowledge of this complex and knowledge of the nature of its effectiveness (i.e., the type of function), to predict the occurrence or non-occurrence of the intentional action Y (thus understood as a dichotomous 0/1 variable).
Based on the experience gained in view of a certain amount of data, the Y-explanatory and predictable causal relationship as well as the form of its effectiveness with regard to Y is provisionally, hypothetically determined as stated in the above equation (with constants calculated with regard to size). New experiences may reveal the need for revision of this concrete assumption about cause variables and function type:
In place of the above equation (induced by the experience of non-replicability of the results postulated on the basis of the original hypothesis), other equations with different assumptions in this regard can occur -- perhaps one of the following (or any other):

$$Y = a + b_1X_1 + b_2X_2 + b_3X_3 + e \quad (1)$$
$$Y = a + b_1 Z + b_2X_2 + e \quad (2)$$
$$Y = a + b_1\log(X_1) + b_2X_2 + e \quad (3)$$
$$Y = a + b_1X_1 + b_2X_2 + b_3X_1X_2 + e \quad (4)$$

Here, equation (1) assumes that, in addition to X_1 and X_2, X_3 is also one of the cause variables;
Equation (2) assumes that instead of X_1 as originally assumed, a variable Z (correlated with X_1) is the cause of Y;
Equation (3) no longer assumes a linear relationship with respect to the effect of X_1;
and equation (4) finally assumes an interactive effect of the two variables in addition to the additive effects of X_1 and X_2.

But no matter which equation replaces the original one in the course of new experiences, or whether the original one can be confirmed again and again - that Y can be predicted applies unconditionally, however much one may argue about the concrete prediction equation.'

Our previous statements have provided the in our opinion undeniable logical proof that this assertion is to be classified as 'nonsense' from the first to the last sentence:

it involves an inevitable logical contradiction to claim that a cause complex of constant effectiveness can be found for Y, and if Y cannot be thought of as 'caused' without contradiction, then, of course, it cannot be said that the claim about a specifically determined cause complex and about an equally specifically determined mode of effectiveness can be confirmed or falsified by experience.
Where there are no constant relationships at all, two experiences with regard to such related variables cannot mutually confirm or contradict each other.

This verdict results from the argument summarized here again as follows:

(1) I and - in terms of possible discussion or argumentation partners: - other people can learn.
(This statement cannot be disputed by argumentation without already admitting its correctness implicitly. To deny it would mean to defend the logically nonsensical or contradictory thesis that one could learn that one could not learn.
- The sentence (1) is therefore valid a priori.)
(2) If one can learn, one cannot know at any time what one will know at any future time and how this knowledge will be acted upon.
(If you always knew at a given time what you will know later, you did not learn - but in this regard see theorem (1).)[13]
(3) The assertion that one can predict one's own and/or third-party states of knowledge and the corresponding actions that manifest knowledge (i.e. find variables that can be interpreted as a 'cause') involves a logical contradiction. If the subject of a state of knowledge or an intentional action can learn, then there are no 'causes' for this; but if there are 'causes', then he cannot learn - but for this, see theorem (1) again.

The supposed causes of Y by intentional action and the supposed constants by means of which Y and 'causes' are related to each other are in truth nothing else, nothing more significant, than variables that have been found at a certain historical point in time in contingent covariant relationships with Y.[14]

And it is also a purely contingent historical-mathematical fact (but not a hypothesis confirmation!) if these covariative relationships can be determined again in (approximately) the same way on new or other data:

The fact that people act in the same way in a certain situation defined by specified variables, i.e. apply the same knowledge, is in principle 'accidental', since, if one can learn, it is of course also logically impossible to predict whether a person is actually learning or not learning from one point in time to another, but this can always only be established post festum as an already established fact.

And finally, a change in such covariative contexts (i.e. -- in the expression of the constancy principle: an 'unequal effect') must in principle be understood as a 'random' fact (and not as hypothesis falsification); because if one can learn, it is not only a logical impossibility to predict

whether one will actually learn in a certain period of time, but it is equally impossible to state what one will learn if one should actually learn.[15]

Any attempt to deny the principally contingent, random character of established (or postulated) relations, as well as their change or constancy over time, and to give them a causal interpretation, is logically nonsensical. There are no causes in the field of human cognition and action - in any case, our minds cannot think of their existence without becoming involved in a contradiction - and consequently there is no social-scientific causal analysis.[16]

Chapter 2

On the Impossibility of Causal Scientific Social Research

An investigation into the justification of sociology as a reconstructive science of action ("action grammar")

I

There is probably only a minority of professional social scientists left, and their number is likely to continue to decline, who would not be willing to sign the following statement as a programmatic sentence about the 'logic of the social sciences':
The main task of the social sciences, as well as the sciences as a whole, is the 'explanation'.
In the social sciences, in particular, it is a matter of explaining human actions on the basis of internal and/or external causes (variables), whether in the deterministic or in the statistical sense. The explanatory hypotheses must be formulated in such a way that conditional forecasts can be derived logically from them, the occurrence or non-occurrence of which must be regarded as confirmation or refutation (falsification) of the hypothesis. If empirical experience confirms a hypothesis, this is not proof of its truth - in principle, later experiences can always falsify it.
If, on the other hand, the hypothesis is refuted by empirical experience, a new explanation with changed assumptions about the corresponding cause complex must be developed with regard to the action classified as requiring explanation: (social) scientific progress develops along the path of negative, disappointing experiences, and due to a corresponding elimination of certain hypotheses from the reservoir of all conceivable hypotheses.

As reasonable as this position now seems not only to the scientist, but also to the layman, that one must almost fear to be regarded as astonished if, on the contrary, one were to assert that the social sciences are not about causal explanatory hypotheses and their systematic test on empirical experience; and if one should even go further to say that such hypotheses and hypothesis developments stimulated by falsifying events are even impossible in the social sciences, then one will probably immediately be enveloped in the ring of irrationalism, obscurantism, essentialism or dogmatism. From the position presented at the beginning, the answer to such 'accusations' also seems too obvious and striking to shy away from such characterizations: whether or not causal explanations and empirical tests are possible in the social sciences, the answer can be anticipated, that is after all a question of experience, that fundamental, categorical discussions about it are inappropriate in the first place.

It shall now

(1) be shown that this answer does not withstand a thorough analysis: one does not learn from experience that causality exists or does not exist, but, conversely, an experience-independent rule of mind is the prerequisite for being able to speak of learning from experience, of hypothesis confirmation and falsification at all. Afterwards,

(2) when the question 'Are there causal explanations in a specific subject area?' has been worked out as a non-empirical, rather as an epistemological question, it should be

demonstrated that it follows from the logical analysis of the aforementioned rule of reasoning itself that this principle cannot apply in the field of the social sciences. And finally

(3) it is important to show which revisions with regard to the self-understanding of the empirical social sciences necessarily result from the recognition of the correctness of what has been demonstrated under (1) and (2).

II

Classical empiricism, particularly D. Hume, has proven that the impression of a necessary connection between two or more events cannot originate from experience: Hume states that there is no bond that observably connects events.
For empiricism, which only recognizes concepts that can be derived in some form from direct experience, this insight implies that the concept of causality qua certainty of a necessary connection does not and cannot exist.
Justifiable on the basis of experience and thus permissible in the system of scientific terms, Hume further argues, is alone a much more modest concept of causality. Causality as a temporal succession of events, whereby the repeated observation of such a succession can transform the initial experience step by step into a more and more proven experience and ultimately also lead to the creation of the subjective impression that the event chain in question actually represents a 'necessary' link.
However, since a bond that visibly links events cannot be observed in principle, this impression of necessity is never a guarantee that an experience will not ultimately be refuted. In short: certainty regarding the experience of causal relations is unattainable and falsification is always possible.

Obviously, this 'modest' concept of causality is identical to the one from which the position on the logic of the social sciences presented at the beginning also proceeds. - Is it correct, as repeatedly assumed by empiricists, that this term actually comes from experience and can be justified by it? Has classical empiricism indeed provided evidence that the question of whether or not there is causality in any subject area can only be decided by experience (and not by logical analysis!)?

We owe Kant the insight that both questions must be answered in the negative.
His performance is sometimes understood to mean that he was concerned with the rehabilitation of the first-mentioned concept of causality criticized by Hume.
Although Kant has occasionally encouraged such an interpretation, it represents a complete miscalculation of his position. Contrary to this interpretation, Kant accepts Hume's rejection of the former concept of causality. He (Kant) is nowhere concerned with this concept, but with proving that precisely the more modest concept of causality - contrary to what the empiricists believe - does not originate from experience, but has an experience-independent logical principle as a prerequisite.

The principle involved becomes clear when one considers the question of under what conditions it can be said that an experience regarding a causal relationship (in the sense of temporal contiguity) between two or more specific variables *has* been confirmed by another experience.

The prerequisite for this can be precisely determined:
It consists in the categorical assumption that there is no variability (contingency) in the effectiveness of causes. Only if causes are assumed to have a time-invariant effect does it logically follow from the observation that a particular experience relating to a cause-and-effect sequence

(1) has not been repeated that the true cause of the effect in question has not (yet) been found, and

(2) that a repetition is to be viewed positively insofar as one could also have had the negatively viewed experience of non-repetition.

Experience, it can be stated, teaches at best that certain observations with regard to causal consequences can be classified neutrally as repetitions or non-repetitions. However, these neutral experiences only become positive or negative experiences if one has categorically ruled out variability of the effectiveness of causes in advance, regardless of experience.

The same result is reached by answering the question of the prerequisites for speaking of an experience as a falsifying experience.

- If it is assumed that the nature of the effectiveness of causes can change over time, i.e. they can act in this way once and in a different way at other times, the experience of non-repetition of a certain sequence of events cannot possibly be interpreted as an event that indicates the incorrectness of the first experience and thus requires clear consequences with regard to this experience (i.e. its revision); rather, under the assumed assumptions, a given first experience and its non-repetition are two mutually compatible events that are neutrally registered as such, but from the recording of which logically no consequences whatsoever follow. Only if, instead, an invariance of the effectiveness of causes is categorically assumed, the determination of a non-repetition proves to be consequential: namely, if one event is actually the cause of another and causes have a time-invariant effectiveness, then the same functional relationship must be able to be established between cause and effect event each time; and accordingly, a non-repetition indicates

(1) that the alleged cause cannot be the true one, and

(2) that the contradiction of experiences must be explained by the demonstration of other systematically effective but disregarded causes, with which the alleged cause was in different ways contingently correlated.

III

The reasoning principle underlying the modest concept of causality of empiricism (as well as the conception of the ‘logic of the social sciences’ presented at the beginning) has thus been worked out as the so-called ‘principle of constancy’.
In its shortest version, it states that causes have a time-invariant form of effectiveness. It represents the logical basis for the known rule ‘equal effect = equal cause’ or ‘unequal effect = unequal cause’, according to which we interpret repetitions as confirmations and non-repetitions as falsifications (in each case in the meaning explained above).
Only with the assumption of constancy of effect does it in fact follow - in accordance with the equivalence relationship usually expressed - from the determination of two equal effects,

a) in general, that the cause complex must also have been identical in both cases, and

b) precisely that a hypothesis claiming a particular event as the cause of a given effect has been confirmed insofar as the same covariative relationship could have been established in both cases, although this could also have been different, which would have disqualified the cause as the cause;

and also: only if the effect is assumed to be constant in advance does it logically follow from the finding that a certain covariate relationship between given variables was (clearly) different in two cases,

a) in general, that there must also have been a different constellation with regard to the cause complex, and
b) precisely, that the variable used as the cause cannot be the true cause, but must have covariated differently with the (unexplained) true cause in both cases.

Although even a prerequisite for 'learning from experience' -- for hypothesis confirmations and falsifications -- the constancy principle or the rule 'same effect = same cause', which logically presupposes it, is itself experience-independent.
Paradoxically, it has been the empiricists who have shown this; for their statement that causal bonds between events cannot be observed is tantamount to saying that a proposition such as causes exhibit a time-invariant form of effectiveness cannot be justified by experience.
On the positive side, experience can never show the correctness of such a sentence, because there is no experience of event links, and even if there were, one would not consider the links whether they connect the linked events in a time-invariant manner, i.e. in always the same form.
And from a negative point of view, experience can never falsify this proposition, because in principle it cannot be ruled out that an 'unequal cause' cannot actually be found in accordance with the rules for an effect determined to be 'unequal'.
It follows that a principle that itself does not come from individual experience, and that cannot be a result of individual learning, is the prerequisite (Kantian: the condition of possibility) for making individually confirming and falsifying experiences and learning from errors.

- It is not apparent from experiences whether they are confirming or falsifying, whether one can and/or should learn something from certain events, and whether there are causes that are effective over time. Rather, it is our mind that turns certain experiences into confirmations and falsifications and transforms experiences that are neutral in themselves into stimuli that induce learning processes by following a logical disposition that is not acquired through experience and cannot be taught by experience. It treats observable sequences of events as if they were fundamentally controlled by causes that are invariant over time.

In simple terms: causality does not exist in the world, but is a strategy our mind uses to learn how to navigate the world; it is not a property of the objects our mind deals with, but a property that belongs to the mind itself, a property of its modus operandi.
Contrary to the decisional echoes of formulations such as 'the mind makes' or 'the mind treats', it should be emphasized, not least in order to prevent misunderstandings, that neither the fundamental use of the discussed principle of mind, nor its content, can be made the object of contingent individual decisions.
Rather, the principle of constancy is a universal mental structure, and anyone who has ever learned from experience has already (tacitly) assumed and applied it.
The principle is no more a matter of individual disposition than the question of whether or not one should learn from experience.
And a second possible misunderstanding in connection with the currently discussed principle of reasoning should also be explicitly countered here:

that a logical operation scheme must be classified as arbitrary because it must be considered experience-independent (in the sense mentioned above), and that one must resort to an idealistic theory of knowledge (= reality is a materialization of the mind!) in order to be able to explain the applicability of a 'pure' principle of the mind to reality.
The inappropriateness of such interpretations is illuminated as soon as one understands pure principles of the mind for what they are -- results of the evolutionary process. --
Just as a fish's fin is not 'acquired' through experience of life in water and the means of locomotion available there, but is a 'given' for every single specimen of the species, so too are the strategies for dealing with objects of experience, as expressed in the principle of constancy, predetermined for each individual, a priori; But just as fins, in the course of phylogenetic history (not individual history), must prove themselves as tools for life in water, i.e., they must prove themselves suitable for life in water in order to enable the continued existence of fish, so too must the pure principles of reason be understood as strategies acquired and proven through phylogenetic history. Although experience-independent, they are thus anything but 'arbitrary': they are the results of a phylogenetic learning process.

IV

An understanding of the peculiarity of pure principles of reasoning is of central importance in the context of the arguments presented here as a whole and yet to be presented.

In view of a naive-empirical basic position that dominates scientific practice, from which a priori logical principles appear as 'absurdity', or at least as something that must be met with the utmost skepticism, a second, understanding-promoting example of a pure intellectual principle should be briefly discussed before the actual course of argumentation is continued - as it were in parenthesis.

Like the concept of causality, the concept of substance (i.e., the concept of objects as entities that exist independently of subjective perceptions of these objects) has also been the subject of detailed epistemological analysis in classical empiricism.
With Locke, the result of this analysis can already be found precisely formulated:
There is not and cannot be an idea of substances derived from experience.
This Lockean insight results from the fact that the third event of the sequence of events: object perception - disappearance of the object from the perceptual sphere - renewed perception of the object in question can always be interpreted both as the rediscovery of an object that has never ceased to exist and as a re-creation of an object interrupted in its existence in or with perception, without ever being able to come into conflict with experience.
The consequence of the insight into the independence of experience of the concept of substance should have been - under empirical premises - the banishment of the concept of substance from the stock of legitimate scientific concepts. Inconsistent enough, however, none of the classical empiricists has really taken this step.

Locke defends this inconsistency by pointing out that the idea of substance is dear to us because it is useful. However, there is nothing worth mentioning about the usefulness of the idea. Nevertheless, the characterization of the concept of substance as 'useful' is correct. 'Useful' is even an outspoken understatement given the importance of this idea. Rather, despite the fact that it cannot come from individual experience, it is a (vital) necessary mental structure, no less necessary than the possession of an organ such as the lungs.

The fact that, in view of objects that have disappeared from the perceptual sphere, an active search is always considered possible, with a rediscovery of the objects as its result, is the result of a blindly assumed conception of substance. In other words, the concept of substance is the logical prerequisite (condition of possibility) for any initiation of a search activity; without it, the phenomenon of search would not exist. -- However, active searching aimed at finding lost objects is just as indispensable as having a heart, lungs, and kidneys -- and it doesn't take much imagination to picture this. Like these organs, the conception of substance is also an indispensable result of a tribal historical learning process: an experience-independent reality for every individual, and yet a (vital) necessary adaptation proven in the sequence of generations.

V

After this additional illustration of pure, a priori principles of mind, we come back to the actual topic.
It is assumed that the previous argument is accepted as convincing, and it is admitted that the question of the existence of causal laws is not of an empirical, but epistemological nature: one does not learn from experience that there are laws or not, but that the mind operating according to a certain, extensively discussed scheme constitutes objects that behave lawfully.
Why should the mind not also be able to constitute the subject area of the social sciences: human action and cognition as regulated by laws?
If it is he who can in principle create a lawful world based on a certain mode of operation, why not also in the special case of the (world) section 'Social World'? What is so peculiar about the social world that what is otherwise possible is not supposed to be possible here?
Obviously, it would be completely inappropriate to cite empirical characteristics here:
although it is inadmissible to refer to empirical similarities of subject areas in order to justify a 'uniform' logic of the empirical sciences, it is inadmissible to base oneself on differences in order to argue the opposite view.
Whether and why a particular subject area has no laws must rather, if it is the mind that brings lawfulness into the world, follow from the analysis of the mind itself.
In fact, it should be shown that on the basis of the logical analysis of the - experience-independent - mental operation principle discussed in detail above, there is a compelling need to distinguish between an object area with causality (natural sciences) and one without causality (social sciences), as well as the precise delimitation of the two areas from each other.
A mind that imposes lawfulness (i.e. constant relations between observable quantities) on an objective event by working according to the rule 'equal effect = equal cause' or 'unequal effect = unequal cause' can also conceive of itself, i.e. its own states (states of experience) existing at different points in time, as logically impossible, as characterized by constant relations.
If it were to assume that the transition from one of his states to another was regulated by laws (constants) that he could find, so that he could predict the subsequent states on the basis of a state, this would logically imply that there can be no experience for this mind that, like the finding of 'unequal effects', can falsify previous states and as such require a change of state.
However, if there are falsifying experiences of unequal effects for a mind, i.e. experiences of non-repetition, which the mind interprets without exception as an only apparent inconsistency of the subjective event, which in truth is always due to its own error with regard to the true causes, it must necessarily think of its own state sequences as variable, as inconstant.
In short - either the mind operates according to constant lawfulness, but then it cannot learn from errors and impose constancy on a objective events,

or it can do the latter, then its work is not subject to any laws that can be found for it, but is a-causal.

An object area that is characterized as 'lawful', and whose lawfulness is not an observable lawfulness originating from experience, thus logically requires an object area characterized by a-causality as its own complement.
Which area is to be understood as causal, and which as a-causal, is obvious: Only in the context of a non-binding mental game could the mind or its state sequences be 'kept constant' (which is synonymous with the view that the mind cannot learn from errors!), while at the same time variability was attributed to natural events by accepting non-repetition as an expression of causes with a variable effect. In fact, however, the existence of the rule 'equal (unequal) effect = equal (unequal) cause' in our head implies that, conversely, it is the state consequences of our mind that are a-causal consequences (for us), while the objects of external experience must form the causal complement.
This form of complementarity proves necessary even in two respects

- Not only does the mere existence of the above rule indicate that we can learn, and the state consequences of the mind can therefore not be causally interpreted by us; this form of complementarity is also mandatory for us in so far as one cannot do without learning from experience. And as necessary as it is for us to be able to learn from errors, and as compelling as the experience-independent idea of a constant nature is for us, the state consequences of our mind must also be interpreted by us as a-causal.
- By forcing one area of objects to remain constant, the mind inevitably makes itself logically inconsistent.

VI

For the philosopher, it may already be apparent at this point in the argumentation to what extent the complementarity of subject areas, which has just been demonstrated as necessary, logically results in a precise, unequivocally determinable distinction between a scientific object area with causality and one without causality.
The non-philosopher is at least clear about this conclusion on the basis of the previous statements, so we assume that this complementarity of subject areas must somehow be reflected in the practice of scientific subject division; however, to what extent a clearly defined distinction actually results from the previous analysis, or even where, in terms of scientific subject division, this boundary runs, may not be obvious to him.
The correspondingly detailed proof of this, which can be broken down into two steps, begins -- in step one -- by showing that from the statement 'insofar as I can learn (and I must learn), I cannot in principle predict my own states of knowledge with regard to causal experiences, and I can therefore also in principle only conceptualize the relations connecting these states as a-causal (variable)', the following statement can be derived in a logically compelling manner:

'As far as I can learn (and I must be able to learn), none of those who can or could in principle enter into an argument with me - and we could not all in cooperation - can predict due to the knowledge of current, future states of knowledge of individual other persons or the totality of persons, and we must therefore all conceptualize the corresponding object area of the sequence or succession of the states of knowledge of all persons capable of reasoning in principle as an a-causal object area.'

This second statement follows directly from the first, taking into account the indefensibility of a 'solipsism':
That there are people with whom I can argue, and that there are other objects besides me that one does not perceive as 'things', but as 'other minds', can absolutely not be denied. By denying it, you address someone and think you can convince them of something with understandable reasons, and thus inevitably imply their ability to learn (the ability to learn from what I have said).
This rejection of solipsism now means: First of all, I cannot predict my own states of knowledge and do not conceptualize the corresponding relations as constant or lawful; however, if there are other persons besides me who can learn, i.e. people with whom I can argue, then the assumption of an object area with double contingency is also implied: others respond contingently to the knowledge I have learned and presented in the context of an argument, and I, in turn, react contingently to the contingent reaction of the others. Thus, the sequence of one's own states of knowledge, like the succession (interaction) of one's own and others' states of knowledge (the argumentation between minds), is logically and inevitably characterized by inconsistency (non-regularity) of relations.

The subsequent second verification step then consists in the demonstration of the logical transition from the statement:

'As far as I can learn (and I must be able to learn), the sequence and succession (interaction) of the states of knowledge of all persons capable of reasoning must be understood as an a-causal object range' to the following statement:

Insofar as I can learn (and I must be able to learn), the sequence and succession (interaction) of the states of knowledge and actions of all persons capable of argumentation and interaction must be understood as an a-causal object area.'

Both sentences result from the fact that it can be shown that 'knowledge' (precisely Knowledge regarding causal relationships between certain measurable variables) and a 'change in knowledge' based on confirming or falsifying events 'Action' (specific, purpose-rational action) and a 'change in action' based on successful or unsuccessful repeatability must be epistemologically presupposed, as must the possibility of argumentation and practical interaction.
- Verbalized or verbalizable knowledge regarding causal relations is only referred to as 'knowledge' because it can enter into purposeful actions; one can also speak of an experience falsification, for example, only because and insofar as each falsification corresponds or must correspond to an unsuccessful (purposeful) action, i.e. an action that does not or only incompletely realizes its given goal; and finally, one can only speak of an argumentation insofar as it can be expressed in principle in a sequence (accompanied by words or explaining the use of words in the first place) of active demonstrations on the ego side and corresponding, possibly subsequent, 'understanding' manifesting imitations of actions and a final, contingent response demonstration on the part of a current or fictitious alter.

With this proof of the indissoluble epistemological coupling of knowledge and action, however, it can then be formulated in general terms: If one can learn at all, one must be able to learn in the action system, because learning in the system of action-relieved linguistically formulated knowledge or verbal communication is only a derived form of learning, i.e. it is only 'learning' at all, because it has or can have a correspondence in the action system. Actions (as manifest knowledge), as well as states of knowledge (as information about possible actions), must

therefore be included in the object area, which, if one can learn, must necessarily be understood as a-causal.

With the rejection of solipsism and the proof of the indissoluble epistemological coupling of knowledge and action, our analysis thus comes to the conclusion that, beyond the narrow range of consequences of one's own states of knowledge, the entire range of consequences or sequences and interactions of states of knowledge and actions (purposeful as well as interactive) of all persons capable of arguing or interacting must be understood as a-causal.

- Since this field is clearly identical to what is traditionally referred to in the scientific division of subjects as the humanities and social sciences, it can be concluded from a purely epistemological analysis that the assertion of the possibility of a social science causal analysis involves a logical contradiction, the existence of which, it can be assumed, has remained largely unnoticed only as a result of a highly widespread and deeply rooted empiricist ideology of knowledge in the scientific system. If we can learn -- and we cannot deny that we can -- then our knowledge and actions must be thought of as a-causal with logical inevitability. And only because this area is inevitably a-causal for us can we conduct causal research at all in the logical complement of it, i.e. in the area of everything that does not belong to the a-causal area delimited above (that of the natural sciences). The possibility of a natural science advancing due to falsification of predictions logically presupposes that there is no causal research in the social sciences.

Only - this is what the a priori structure of our mind wants - by constituting ourselves as 'learning' and thus unpredictable, can a complementary range of 'explainable' phenomena open to causal research be produced by us.

VII

What consequences with regard to the self-understanding (and, if applicable, the research practice) of the social science disciplines result from the previous proof of the a-causality of the social science subject area?

First of all, it is almost obvious that the self-image of those social researchers who believe they are conducting causal research is in need of revision.

Such a claim cannot be thought of logically without contradiction and must therefore be rejected as nonsensical: if one can learn (and one can only claim at the cost of a contradiction that one can learn that one cannot learn), then (and this is logically necessary!) the temporal succession of human states of knowledge and actions must be thought of as an a-causal, contingent sequence of events that is not characterized by constant relations.

The (mostly computer-aided) numerical calculation of variable-connecting relationship constants (namely regression constants) carried out hundreds of thousands of times daily in social science practice can therefore not be interpreted (as regularly done) as a calculation of law constants carried out on random sample data, which is hypothetically assumed (i.e. with always existing possibility of 'falsification' due to unsuccessfully replicated results) to be valid for a

more comprehensive, spatially and temporally unlimited (i.e., described exclusively by general terms) universe.
Logically without contradiction, this practice can be interpreted in this way (and only in this way):
On the basis of a closed, finite (i.e. historical) set of data, the function constants are calculated for a presupposed function type (usually: linearity), on the basis of which the given, historical values of the related variables can be calculated apart while minimizing the prediction error.

As such, the constants are also only historical facts, and statements about them, like all statements about historical facts, (but unlike statements about laws!) are verifiable statements.

Beyond the area of the data on the basis of which they were calculated, they have no meaning; since the social-scientific subject area can only be thought of as a-causally structured, in principle, constants cannot be law constants.

And finally, they cannot therefore be confirmed or falsified (again unlike law constants) by other, further experiences, but the re-establishment of a certain relationship constant within the framework of a new experience or its non-recurrence is nothing more and nothing less than a verifiable statement with regard to another historical fact, as well as the associated (likewise verifiable) statement that with regard to two or more historical facts, one can speak of the event of a match or non-conformity: again a historical fact.

At this point in the argument, the following could be considered:

Admittedly, the self-image of many social researchers is in need of revision. They cannot (logically!) do what they claim to do, and in the light of this insight, their activity undoubtedly also loses its importance, significance, size, etc., because due to them, one is reclassified from social theorist to fact collector.

But does not social science practice remain completely unaffected by this insight, so that in the end one only has a new name (i.e. a new characterization) for an old thing, i.e. the practice of the social science causal researcher, but can otherwise confidently continue with the beloved activity in the usual way?

In other words, apart from nominalistic, are there also real consequences that result from the compelling insight into the a-causality of the social science object area?

This question must be firmly answered in the affirmative. The reason for this becomes clear when one first realizes that from the self-image of conducting causal research, concrete consequences with regard to research practice can be derived and actually are derived.

In terms of causal researchers, one is always confronted with a given explanatory task, i.e. a dependent variable, in the following situation:

Since Hume, it has been known that variable-connecting relations cannot be perceived by the senses. Therefore, with a given dependent variable, every other variable comes into question as a cause in principle, as well as every conceivable function type.

Or in other words: because one does not look at variables, whether, and if so, in what functional form they are a cause variable for a certain other variable. Therefore, one cannot exclude a priori a variable as a cause variable and a function type in terms of the form of the law.

Rather, you have to try it out and then see whether or not your own selection proposals can be confirmed in replication studies (under conditions that change as much as possible).

-- Therefore, it is only logical if works, as in fact many of the empirical-social scientific studies presented today, are at least in parts like ‘fishing expeditions’: you fish around in the large pond of possible causes and then present (technically easiest in the form of a large number of multi-dimensional tables) an overview of how each variable is related to every other and to the totality of all the variables fished out as a whole, and otherwise you leave everything else to (yet) pending replication studies.

If there were indeed causality in the subject area of the social sciences, the following could at best be objected to this practice of (commenting) compilation of arbitrary variable covariations (relations) and associated hypothetical assertions of corresponding relationships as universally valid relationships:

that they - at least very often - lack a ‘deductive touch’, the attempt to combine disparately juxtaposed individual statements about variable relations into a system of statements related to one another by logical ordering relations, i.e. into a theory.

Under the given conditions, however, no argument of a fundamental epistemological nature can be thought against this practice: even if it does not represent the ‘whole’ of scientific work, and possibly the culmination of scientific activity consists in something else (i.e. in the systematization of disparate individual statements into an integrated theory), there is nothing wrong with this practice of a social ‘piecework technology’, and one must even recognize it as a necessary (even if not: sufficient) component of science -- even the theorist among the causal researchers must, in his constructions, recognize the results of this practice as empirically given theoretical building blocks, and he can often be stimulated in his systematic activity by individual results that initially appear completely disparate (as part of a fishing expedition).

However, the assessment of this practice changes drastically if the idea of a causally structured subject area has to be dropped as logically untenable in the case of the social sciences.

At the same time, this eliminates the possibility (characteristic for causal research) of controlling the aforementioned ‘arbitrariness’ in the selection of causes for given effects a posteriori by making it possible to make accurate predictions with regard to given effects on the basis of ‘true’ causes.

For if one cannot deny without contradiction that one is able to learn, and that, if one can learn, one cannot conceptualize knowledge and action without contradiction as produced by causes of constant effectiveness, but only as contingent, uncaused reactions, then it follows logically that there can be no predictions for us with regard to knowledge and action due to true causes, and thus also the possibility of controlling the ‘arbitrary’ selection of causes by success-controlled, aposterior predictions is eliminated.

Under the assumption thus changed, the practice, which previously seemed sensible, becomes only an arbitrary, completely uncontrolled and uncontrollable activity:

it must be classified as an arbitrary set of arbitrarily selected antecedent variables with a given subsequent variable in any functional context. Having become unjustifiable in epistemology,

the practice of arbitrary antecedent variable selection, which is uncontrolled due to a priori considerations and justified only by a posteriori controls, must be abandoned.

This logical compulsion to abandon the stated practice is associated with a frequently heard 'real' reason in a remarkable way: it can be argued against social scientific causal research that even its most ardent propagandists cannot seriously claim that - with all previous efforts - any empirical laws of action have already been found; the unsuccessfulness of econometrics, which is generally regarded as the most advanced branch of social scientific causal analysis, is an unmistakable sign of this.

In view of this reality, should one not come to the conclusion that there is something fundamentally wrong with this practice, which promises so much and cannot deliver on it?

-- It is known that this conclusion is not mandatory. In fact, causal researchers always have the opportunity to argue that this fact is 'coincidental', that one just has to wait for the Galileo or Newton of the social sciences; and as flimsy as this argument may seem, it cannot be dismissed as completely nonsensical.

It becomes nonsensical only at the moment when the aforementioned 'real' reason for rejection (i.e. the factual failure of the practice) is understood as an empirically determinable consequence of the underlying, more fundamental logical reason for rejection for this practice:

Only the logical necessity of having to perceive the social science object area as a-causal explains why a social science conducted as causal research cannot be successful, and why everything is waiting for Galileo and Newton's window dressing.

It is no coincidence that there are no empirical laws of action, but our mind is structured in such a way that it cannot help but constitute the object area 'social sciences' in such a way that there are no laws for it to find there. He can find it there as little as he sees himself unable to make experiences in which the predicates X and non-X are assigned to any object at the same time - so no more than the compulsion to avoid contradictions seems to him to be a real compulsion.

VIII

If it is thus made clear, on the one hand, that the existence of a certain self-image of social scientific work can lead to practical methodological consequences for this work, and that the untenability of a certain image also eliminates the cognitive justification of certain aspects of the social scientific procedure; if, on the other hand, it is clear to what extent the unsuccessfulness of a certain practice can even be explained by the logical untenability of the self-image on which it is based

- then the question of the alternative program arises.

In contrast to causal analysis, what form of social science practice can be justified in epistemological terms and thus carried out with the prospect of success?

And what, according to the directly related question, does the logically consistent self-image of the social sciences look like, to which this practice corresponds or must correspond?

And finally: can the changed self-image - apart from the seemingly negative explanation of the unsuccessfulness of a social science conducted as causal research - also provide 'positive' explanations for other phenomena in which it finds equally real expression?

A practice that seeks (constantly effective) causes on the basis of which given dependent variables can be predicted ex ante, and whose selection justification (qua causes) is based exclusively on the a posteriori experience of successful predictions, is logically nonsensical. Logically possible is therefore the complement thereto: a practice that dispenses with falsifiable predictions and instead 'only' claims to reconstruct ex post; and a practice that does not reconstruct causes that can only be justified a posteriori as such, but that reconstructs the object to be explained (i.e. an action or a state of knowledge) from such components and component relationships that are logically distinguished from the outset as a special class of elements, as the correctness of their selection becomes verifiable through the admission of the actor(s) (knower(s)) themselves that corresponding actions (states of knowledge) were actually -- consciously or unconsciously -- built up by them from the selected components and could accordingly actually be decomposed into them as their real (i.e. analytically rightly distinguished) elements.

In place of a practice that tries to explain measurable variables (i.e. everything that is measurable) by causes whose selection from the class of all antecedent events is a priori unlimited (i.e. in principle every event can be a cause) and can only be justified a posteriori by a predictive success, but cannot be verified, a practice occurs that tries to reconstruct exclusively events that can (also) be understood by the actor himself as a logical-analytical unit from components (subunits) whose selection from the class of all events is a priori limited (i.e. from the outset not every event can be a 'component') and can be verified a posteriori by the acknowledgement of the actor that the explanation unit actually consists of the components (combinations) indicated, in such a way that a change (substitution) with regard to one of the subunits would also constitute a logically different, changed explanation unit.

Thus, on the one hand - as the comparatively less demanding form of permissible forms of practice - all the activities that consist in decomposing actions into the components that are directly conscious, i.e. verbalizable to the actors themselves: into motives, action-inducing interpretations of situations, reasons, goals, technical and practical knowledge, normative orientations, etc. - and of the permissible forms of social science practice, on the other hand, the one that (also) undertakes to reconstruct the logical constitutions of action that are unconscious to the actor himself, as well as the social structural features that result - quasi doubly unconsciously - from the succession of partly unconsciously controlled, contingent response reactions to partly unconsciously regulated actions, are permitted.

(In the same way as the linguist who reconstructs grammatical rules that are not, or no longer, or no longer fully conscious of ourselves, and which cannot be adequately formulated by ourselves, but which we nevertheless actually follow in the formation of sentences or utterances, and which we can therefore recognise in principle as rules of our speech if they are formulated for us by another person - the linguist - so does the comparatively more demanding social scientist.

In a sometimes lengthy trial-error process, he reconstructs the perceptions and norms that must ultimately be recognized and acknowledged by us as those that have actually triggered and decisively structured our actions and interactions, regardless of their eventual verbalization - and alongside and in addition to all conscious perceptions and norms.

IX

The fundamental elements of the revised self-image of empirical social research already arise directly from the above remarks on the - in contrast to causal research - logically permissible forms of empirical-social-scientific research activity.

Rounding off and supplementing these explanations to an even more unambiguous picture, this can be drawn in this way.

The empirical social scientist must, insofar as he is interested in a logically consistent characterization of his work, understand his work in conscious analogy to that of the linguist: just as this one and the same given sequence of sentences or statements are not explained, but broken down ex post into the more or less conscious and concrete layers of their constitutive components - into their conscious semantic and pragmatic components, which give the sequence its comparatively most concrete determinateness; into their less conscious grammatical surface structures, which determine the same sequence on a more abstract level; and into their mostly even less conscious grammatical-logical depth structures, which give the sequence its comparatively most abstract logical basic form (whose modifications then represent the more concrete sequence components based on it) - thus: reconstructing-abstracting is also the approach of the empirical social researcher who is consistent in his self-image. He first breaks down a given action or a sequence of successive actions into the logical components that the actor(s) themselves can usually directly name and describe as such.

Then, beyond this, he tries to work out the more general normative-legal orientations underlying this conscious superstructure, as it were hidden in the more specific actions and only abstractly separable from them, as they are recorded, for example, in the codified corpus of common law, in which rules of action (comparable to school grammar in language analysis, for example) are actually in force, which the layman is usually only vaguely aware of as rules that he actually follows on a given occasion.

And finally, once again requiring an additional abstract performance, in an activity similar to that of the universal grammarian, he begins to reconstruct the rules, which in their highest degree of abstractness have (as far as possible) universal factual validity: rules that everyone, so to speak, always follows in fact, but whose observance - precisely because they have always been mastered - is almost invariably unconscious.

Of course, it is not necessarily part of the permissible self-image of the empirical social researcher that an individual seeks to fulfill all of these tasks for a given action (sequence).

Within the framework of scientific division of labor, the reconstruction of the action components of different layers of consciousness and abstraction can, of course, be a matter for a large number of researchers. However, each of them must be aware that, whatever he does, his activity consists in the reconstruction of those components that the actors themselves must in principle be able to recognize and acknowledge as actual logical-analytical building blocks of the action - and thus has absolutely nothing to do with research into causes, which are in empirical relation to the actions.

However, it is not indifferent to the self-image of the social researcher to reconstruct which abstraction layer he is engaged in. As already hinted, the higher the abstraction level at which the reconstruction attempts are to be placed, the higher the scientific performance and (hopefully) the correspondingly distributed scientific prestige.

While the recording of the conscious superstructure can take place in direct access to a certain extent - even if, as can be seen, for example, from textbooks on survey methods, it is handled expertly, requires a not to be underestimated amount of technical skills, and even if it can be quite time-consuming, for example, in the content-analytical study of historical documents - the unconscious action components require considerable abstract, form-perception-like structure recognition skills.

Often, the reconstruction of unconscious, abstract components of action can only be achieved through productive methodological detours:

Just as one can sometimes not recognize the famous forest for its trees without an extension of the perspective, so the abstract components of action, of which one is usually not aware as such, often only become ‘visible’ before a significantly broadened horizon of experience (compared to that of the layman, as well as that of the superstructure reconstructor), in view of which the abstract self-evident things initially hidden in the concrete appear relativized and become perceptible qua relativized.

In this case, studies from the field of social anthropology (ethnology) and, in the case of only one society, studies on deviant behavior are of decisive importance. Their knowledge, as in the case of a change from synchronous to diachronic orientation, that of studies of both general history and that of the individual (ontogeny), usually conveys the necessary distance from the ‘normal’ in order to make it conceptually determinable in contrast to the extraordinary, different, not-yet-normal (and also, conversely, this through that); and their knowledge also often opens up the view of the even more abstract, even more fundamental universals.

In view of the fact that it is not surprising that, in contrast to the large number of studies coming onto the market every day that make the conscious superstructure of actions their subject, the number of social science works that belong to the type of action grammar is so extremely small; and in view of the fact that it is studies of this type that rightly require special respect (and usually also receive it), as in the case of those who are concerned with language, the crown belongs to the theoretical linguist and especially to the universal grammarian, so it belongs to those who deal with actions and the social structures resulting from the interplay of actions, to whom the abstract unconscious rules that underlie our concrete actions are raised to the level of consciousness, and who ideally - if he reconstructs universal or quasi-universal action structures - thus also provides a correspondingly timeless validity.

X

In a nutshell, the revision of the self-image of empirical social researchers, which has become necessary due to the proven impossibility of causal research in the social sciences, can, in our opinion, be most appropriately expressed as follows: Not a social or action technologist, but a social or action grammarian. Only the explicit self-image as a grammarian of action leads, as has been made clear, to a situation in which the practitioner can consciously do the right thing

and refrain from doing the wrong thing (i.e., that which, for purely logical reasons, cannot be done with any prospect of success), and in which alone a sure-footed and consistently defensible methodological assessment of the work of others becomes possible.

And finally: only when the social researcher learns to understand his activity in analogy to that of the grammarian, and - above all - only when he learns that he cannot understand it otherwise, unless at the price of logical contradictions, only then can the existence of universally known phenomena be explained to him as a logically necessary existence, the factuality of which must otherwise appear to the social scientist as a depressing problem.
As already mentioned, such a phenomenon is the factual unsuccessfulness of causal-scientific social research: certainly a depressing problem for the causal researcher, it is for those who know that social research can only (and only) be carried out in the attitude of a grammarian with the prospect of success, a matter of course, yes, a necessity.

- However, there is a second, as it were 'positive' phenomenon that has not yet been mentioned, which is probably even more interesting and revealing in this context:

It is a matter of the fact that, if one considers human history, precisely when the concept of immutable, eternal laws of nature (of which there can in principle only be apparent exceptions) is consciously expressed and - accordingly - a systematically advancing natural research is established, at the same time the philosophical idea of 'freedom' also arises, i.e. the idea that human action, in strict contrast to natural events, follows inconstant, deliberately insightful changeable rules, and is only therefore open to a moral assessment: initially and temporarily in classical Greece, and then in full development and since then continuously with the age of Renaissance and Enlightenment.

And on the other hand - when changing the perspective towards ontogeny - it is the related fact that in the course of the cognitive development of the child, as an expression of a universal development scheme, the concepts of natural law and of liberated, morally capable norms of action are either simultaneously present or simultaneously absent.

Again, for the social-scientific causal researcher, this phenomenon of a universal empirical coupling of the ideas of necessity and freedom, as we want to call it briefly, represents a serious challenge: he must understand the connection as 'accidental' and as resolvable in principle (such that, without a disappearance of the idea of necessity, which becomes increasingly less important with the progress of a social science understood as causal science, in order to disappear completely in the end) although there is no empirical evidence to support such an interpretation.

In contrast, social scientists who see themselves as action grammarians cannot simply accept as a matter of course the fact that the idea of freedom coexists permanently with the idea of natural law without any signs of dissolution—after all, they themselves conceptualize the subject area of the social sciences as a-causal — and thus also avoid the pitfalls of a constructivist 'rationalism', according to which the causelessness of human action seems to imply that social structures can be planned at will — because, as a grammarian, they know that the extent of our unconsciousness of the rules governing our actions sets narrow limits on such ambitions. Above all, he can explain, far beyond all this, why the mentioned idea coexistence is not a mere contingent-empirical fact, and why a replacement (for example, according to the scheme presented in the displacement hypothesis) of this coexistence is not possible, unless through its non-coexistence.

He knows - recalling the course of argumentation presented in this work as a whole, which at the same time contained the justification for why only the self-image of a reconstructive action grammarian can be considered as cognitively acceptable to the empirical social researcher - that the above-mentioned idea coexistence is based on an exactly corresponding cognitive complementarity of the two ideas as a foundation: a mind that learns the experience-independent scheme ‘equal (unequal) effect = equal (unequal) cause’ accordingly from experience and thus constitutes natural law, cannot determine itself or its own state consequences as determined, but must, logically inevitably, understand itself as a-causal, as free.

Empirical idea coexistence is nothing but the most tangible empirical expression of this universal a priori mental structure, and any hope that coexistence can be dissolved must therefore be regarded as demonstrably mistaken: as long as there is natural law for us, the idea of ‘freedom in human things’ is ineradicable for us - that is what our minds want. It can only disappear, should the former idea also disappear from our minds, and should we return to earlier, animistic worldviews.

Chapter 3

On the Impossibility of Causal Scientific Social Research
An investigation into the justification of economics as an a priori science of action

I

There is almost a consensus in the camp of economists (and not only there) today that economics is an empirical science: there is hardly a textbook of economics that does not explicitly emphasize the experiential nature of its subject. Nobel laureates such as P.A. Samuelson - exemplified as one of the most influential minds of Keynesianism - and M. Friedman - as head of the monetarists, the other two schools that dominate academic economic life today - agree on this assessment, regardless of their other differences.
However, it is also shared - and even provided with particular emphasis - by a school of economic research associated with the names Schmölders and Katona, which has been further removed from the center for economic orthodoxy.
And finally, it is precisely the non-economist for whom it seems to be a foregone conclusion that economics is an empirical science.
This almost all-encompassing unity is reflected on the concrete level of scientific practice in some obvious commonalities - shared across all school boundaries.
On a quasi-universal scale, for example, econometrics is attributed a crucial importance for the development and promotion of economic advances:

Samuelson and Friedman, as is well known and significant, have themselves emerged through econometric investigations, and for the third school mentioned by name, at least for the younger generation of scientists following the founders, an extensive use of statistical-econometric techniques can also be observed.
Statistical-econometric studies, according to the conviction shared by all, represent the systematically prepared foundation of experience on which theories have to prove themselves: the empirical eye of the needle, so to speak, through which a good theory must go, and in which a bad one sticks, without whose corrective function, in any case, all theory must remain mere speculation.

More important than the test of a theory of historical material, which econometric work can at best provide, however, is the prognostic test of theories (although in previous econometric studies it may have been first and foremost brought into a precise, testable version) - there is also undivided agreement with regard to this conviction.

Yes, even more than in the general econometric orientation, the uniform consciousness of pursuing an empirical science is expressed in the fact that economists of all schools are willing to accept the prognostic success as the all-important touchstone for their theoretical convictions - if the conditional prognoses derivable from their theories (given the actual existence of the boundary conditions required for their practical application) do not agree or do not agree accurately enough with the factually determinable events, then, so generally, the theory must be

regarded as falsified and, conversely, only if the prognosis is successful can the corresponding theory be described as proven.

Finally, there is a third conspicuous universal agreement, which seems to us to be noteworthy with regard to later explanations. It is agreed that there is no precise distinction between the subject area of economics and that of other disciplines, according to which it would be possible to determine exactly for each question that arises whether it is an economic or non-economic question. It is agreed that the demarcation of economic phenomena, as the subject area of economics, from phenomena of other social scientific disciplines is rather, quite in accordance with the problem of demarcation, for example, of physics and chemistry, or that of psychology and sociology, in principle arbitrary in nature, and can essentially only be motivated pragmatically, i.e. with reference to the factually established form of scientific division of labor.

Like psychology and sociology, economics deals with the explanation of human actions or with the explanation of aggregate sizes resulting from a large number of actions, but, in contrast to the other two disciplines, it specializes in the explanation of such actions or aggregate sizes, which are generally classified as ‘economic’, i.e. usually those that have to do with the production, distribution and use of goods. Overlaps and contacts with the problems of other social sciences cannot naturally be ruled out with such a vague definition - but they should not be ruled out either.

There are no objections in principle to a greater integration of the various social science disciplines (even if one has quite different expectations with regard to the fruitfulness of the results of such integration), and they cannot exist because a uniform logic of the empirical sciences is assumed for all social sciences.

Fundamental, categorical distinctions of economics from other disciplines, one can summarize, and a correspondingly principled anti-integration attitude, such as that advocated by L. Robbins, seems increasingly inappropriate and alienating today.

This also expresses the above-mentioned, cross-school empirical consciousness of economics.

II

Notwithstanding their wide dissemination, all of the beliefs outlined above are untenable. This is to be demonstrated in detail below.
Economics is an a priori, not an empirical science: everything that economists rightly regard as central components of their science are analytically true propositions, and insofar as these are predictions, they are predictions that possess apodictic certainty, i.e. those that can be proven to be true or false in principle - irrespective of any empirical verification or falsification - solely on the basis of logical analysis.

The statements of economics are analytically true sentences, however, only insofar as the subject area of economics is defined precisely.

It deals with the necessary (praxeo-) logical consequences of given, presupposed acts of choice (actions) in given, presupposed, more or less complex contexts; the explanation (prognosis) of the presupposed actions or contexts themselves, even if they represent economic actions

according to popular opinion, is not economics. Such explanations are, as will be shown, impossible for logical reasons of principle: whether economic action (purchase of a car) or political (voting for a party) -- human actions have no causes on the basis of whose knowledge they could be predicted; the concept of causal relationships in the field of human action, i.e. the concept based on experiences of falsifiable laws of action, implies a logical contradiction. In principle, actions can only be understood as reconstructible ex post from their components without contradiction.
In this respect, the just indicated demarcation of the subject area of economics from a priori science can prove necessary: it defines, namely, if an empirical science of action that produces prognostically usable knowledge is not possible, precisely the area within which the sciences may claim from human action alone, within which prognoses then, of course, have a logical structure that is peculiar in comparison to those of the empirical (natural) sciences.

In attempting to prove these theses in detail and thereby define them more precisely in terms of content, we are continuing the tradition of 'pure' economics, in particular the branch known as the 'Austrian School'.

Within its framework, it has been brought to the clearest awareness of the a priori nature of economics. The fact that the propositions of economics are tactically irrefutable, logically true statements based on contingent empirical experience is already evident in some of the most important writings of Menger and Böhm-Bawerk, as well as in their debate with the historicism of the Schmoller school.
However, Menger and Böhm-Bawerk are still a clear step away from explicitly overcoming empirical self-misunderstandings, which could have prevented a large number of misinterpretations to which both of their works were (and still are) exposed. This decisive step is only taken by L. v. Mises, with whose 'Human Action' the Austrian school has certainly reached its highest level of development to date.
Mises is not only the most prominent representative of pure economics, which explicitly sees itself as an a priori science, but he is also credited with the most scientifically logical analyses of the question 'How is an a priori science of action possible?' .
Above all, it is therefore his work to which our following statements are committed.
Now, however, as the statements made at the beginning about the current situation of economics may have made clear, there is no denying that the proponents of the pure theory that Mises, too, was apparently unsuccessful in trying to convince their peers of the correctness of their self-interpretation of economics.
Provided, however, that one is convinced of the correctness of their view, and does not want to hold exclusively ignorance, dishonesty or incompetence on the part of the 'empiricists' responsible for this fact, one will have to think about the reasons that have prevented a greater success.

Undoubtedly, the person of Mise bears the blame for this state of affairs: his quarrelsomeness, the untimeliness of his (economic-political) views, his polemical talents, his apodicticity, in part; just as surely it will be possible to claim that the great weather situation, characterized by the domination of a naive empirical philosophy of science, is not altogether favorable to the possibility of a transformation of the self-image of economics into the image of an apriorist science (comparable to the revolutionary change that has taken place with regard to the understanding of the human cognitive apparatus in the transition from Hume to Kant).

In our opinion, however, something else is of decisive importance for the apparently inadequate persuasiveness that has so far been able to be developed from the position of pure economics -- a significant gap in the argumentation itself: Mises (and Robbins, by the way) does not provide evidence that an empirical science of action that produces prognostically usable knowledge is

not possible in principle. He merely points out (just like Robbins) that such a science in fact does not exist, and that one obviously does not have a single law of action that has been successfully passed through several empirical tests; and, presumably suspecting that such an indication is not sufficient as a principle argument in earnest, he adds that there are no constant relationships in the context of human action, and the fact mentioned is not surprising in this respect.

However, this indication is then considered sufficient to draw the following conclusion: Since even inexplicable (an 'ultimate given'), actions and contexts of action must be given as data, and economics can then be determined as the discipline that elaborates the logical implications resulting from the implementation of a presupposed action within the framework of a presupposed data crown (or changing data crown according to a presupposed scheme), which have already been given with the prescription of the data constellation.

Apparently, the decisive weakness of this argument lies in the fact that it cannot show the necessity of the consequence of having to conceive of economics as an a priori science. At best, one can say that such a consequence is made plausible or pragmatically justified.

However, as long as the irrefutable logical proof is not provided that an empirical science that explains actions is not possible, this decision is subject to a moment of arbitrariness.

This is all the more so because the conceptualization of economics as an a priori science entails a narrowing down of the economic analysis that is accessible, which, with its explicit exclusion of any explanation of action (no matter how economic it may be in the popular sense), must seem to be interpreted as a quite significant relinquishment of knowledge.

In view of this, it does not seem unreasonable to take the following position:

Admittedly, it is possible -- by waiving corresponding insights -- to conceptualize economics as an a priori science, but actually it is important - as a scientifically more demanding goal - to pursue economics as an empirical science of action (with predictions that must prove themselves on the basis of experience and that can fail on the basis of experience), and it is a desideratum of future economic research to reformulate the analytically true propositions of pure theory as propositions that empirical tests are not only accessible, the truth content of which is considered to depend on the outcome of such tests.

With this twist, the position of pure theory is put in the corner, as it were:

One does not necessarily deny that it is possible; however, it is only one of two possible positions, and as one can only too easily assume arbitrary renunciations of knowledge and - seemingly hair-raising to the naive mind - accuse them of immunization against experience, it is at the same time the one with which one, as a real scientist who naturally wants to stand on the ground of facts, naturally does not want to be associated.
- It is therefore not surprising that the majority of today's economists are preoccupied with matters that, if one follows the position of pure theory, have nothing to do with economics at all, or with the translation of sentences or sentence systems from the (correct) language game of a pure science (as mathematics and logic undoubtedly represent) into the (incorrect) language game of an empirical one—as well as with the verification of the reformulated sentences in experience.

It is the weakness of the position, of which Mises is the most prominent representative, not to have demonstrated such a reaction as possible, indeed probably anticipatable, to the challenge of pure a priori theory as fundamentally inadmissible, i.e. as a reaction that is demonstrably logically contradictory (despite its apparent reasonableness).
In doing so, we are convinced, as already mentioned above, that such proof is possible when the demonstration objectives of this treatise are announced.
The additional burden of proof, which is placed on the position of pure theory by the fact that one believes that one can expect from it - if there is any chance at all of ending its shadow existence - not only proof of its possibility, but that of its necessity, can certainly be borne.
In our attempt to justify economics as an a priori science, the corresponding evidence will represent a significant first step in the entire process of argumentation -- economics, i.e. the thesis, must be understood as an a priori science, because an empirical science of action that generates prognostically usable knowledge cannot be conceptualized without contradiction. The apparent solution does not actually exist, and pure theory does not dispense with anything that is not already impossible.
Irrespective of proof of the impossibility of an empirical economy, the following applies:
A blind empirical consciousness that wanted to pursue economics exclusively as empirical science in the face of the possibilities (as well as the limits, of course) of an a priori economy should be regarded as no less unreasonable than one that wanted to completely replace mathematics and logic as they are known by empirical mathematics and logic: in one case and the other, this led to situations that are not only not conducive to the promotion of scientific progress, but in which scientific progress is systematically halted by blindly reinterpreting statements that could be proven wrong at any time on the basis of logical analysis alone into hypotheses that need to be legitimately maintained and thoroughly empirically verified.
However, if even the inadmissibility of an empirical consciousness in economics is proven at all (and not only that an exclusively empirical consciousness has fatal consequences), the degree of unreasonableness of the position of an economics conducted in the attitude of an empirical scientist is increased even further:
In this case, the accusation that one is demonstrably doing nonsense, i.e. doing something that one cannot claim to be doing in principle without becoming involved in contradictions, must in principle be raised against any attempt to find or test empirical laws of action, whereby in some special cases there may even be the additional accusation that one is also behaving like someone who considers the Pythagorean theorem to be in need of empirical review. -- This will also be demonstrated below.

III

The basic idea of the proof of the impossibility of an empirical-causal science of action, i.e. a social science interested in prognostically usable knowledge of laws, be it economics or sociology, comes from K. R. Popper.
Remarkably, to our knowledge, hardly any special attention has been paid to his brief argument so far - and this despite the fact that Popper certainly cannot (any longer) complain about too little attention being paid to him otherwise.
The scientific significance of his argument can hardly be overestimated.
However, and this, we believe, partly explains the lack of attention to the argument by its supporters - in fact, it runs counter to what critical rationalism stands for in terms of the methodology of the social sciences according to its own self-interpretation: the program of a ‘piecemeal social engineering’ based on falsifiable empirical experience.

The evidence itself can be formulated in a few sentences, modifying Popper's argument slightly, improving it and also bringing it into a more general form:

(1) I can learn (and anyone with whom I can enter into an exchange of ideas - discussion, argumentation, etc. - is inevitably also recognized by me as capable of learning).

Comment: This statement is true regardless of experience, i.e. it applies a priori.

This is reflected, among other things, in the fact that the negation of the statement 'I can learn' is logically contradictory, (making the statement itself irrefutable)

a) either openly contradictory as a statement 'I can learn that I cannot learn', or
b) implicitly contradictory as a statement 'I cannot learn', which, since it presupposes that one understands the concept of 'learning', means that one has learned that one cannot learn - and is therefore also contradictory.

On the other hand, the a priori validity of the bracketed part of the statement - quite correspondingly - is shown by the fact that here, too, the negation of the sentence 'you can learn' addressed to a person, i.e. 'you cannot learn', is contradictory (and thus confirms the irrefutability of the initial statement), insofar as the mere addressing already implies that the person addressed can learn or understand.

Note: The fact that one can learn must (and will) be admitted immediately by every scientist: he himself is looking for new facts or confirming events, or develops new concepts suitable for describing facts, or is looking for new explanations for given phenomena -- so in any case he assumes that he can learn.

(2) If one conceptualizes oneself (and other people) as capable of learning, this implies the recognition of the statement that future states of knowledge (one's own as well as those of others) are perceived as unpredictable.

Comment: 'Unpredictability of future states of knowledge' and 'Learning ability' are definitional equivalents.

If one were always to know - as it were from the beginning of one's existence as a being endowed with consciousness - what one will ever know at any later point in time, this would mean that one cannot learn; but if one concedes to oneself (and others) the ability to learn, this is the admission that one cannot predict what one will know at a later point in time.

-- Just as the statement that one cannot learn is indefensible, the statement that one already knows what one will ever know is also untenable a priori: by pronouncing it and intending to defend it in the context of a discussion, one has already conceded the possibility of contingent, previously unknown response reactions.

And conversely, the statement that future states of knowledge cannot be predicted can no more be refuted by experience than the statement that one can learn: experiences can only ever confirm both statements.

Note: The fact that the scientist also assumes the unpredictability of future states of knowledge hardly needs to be emphasized. If he already knew everything he will ever know, he could, in a sense, retire.

-- Incidentally, the recognition of the possible existence of (hypothesis) falsifying experiences already implies that one does not already know what one will know at a later date.

(3) The unpredictability of future states of knowledge implies the unpredictability of future action.

Comment: Again, this is a definitional equivalence.

Knowledge is possible (virtual) action. One only speaks of knowing (about) something or having understood or comprehended something -- be it a concept, be it the existence of a state of affairs, be it a (hypothetical) connection between events or whatever else -- if this can also express itself practically.

If this were not the case, and if learning, i.e. a change in a state of knowledge, could not also be expressed in an intersubjectively perceptibly changed practice, there would be no way to convince oneself and others that one has actually learned (understood) something.

(For example, how should a teacher ever find out if a student actually knows something?) However, only when this is possible we talk about knowledge.

This makes sentence (3) immediately understandable: If one cannot predict future states of knowledge, one cannot predict future actions, since the unpredictable future knowledge must in principle be able to find expression in future actions.

Note: The scientific practitioner also admits - at least implicitly - the unpredictability of future actions by, among other things - as is regularly done in particular by economists - acknowledging the existence of the notorious phenomenon of self-fulfilling or -destroying prophecies: a statement formulated as a conditional prognosis about an area of action can only be declared a self-fulfilling or -destroying prediction (and not simply - prognosis confirmation or -falsification) because it is assumed that those to whose behavior the statement refers can react in a principally contingent manner (i.e. learning) to this statement (principally contingent in such a way that it also did not represent a way out if the scientist formulated a prognosis about how the first prognosis will be reacted to, since, if one has conceptualized persons as capable of learning in the first step, one also assumes that they can of course also react contingently to the prognosis of the prognosis, etc., ad infinitum).

However, this is nothing else than this: that forecasts that are assumed to be self-fulfilling or destroying prophecies are in reality not ‘normal’ - confirmable or falsifiable forecasts, but only statements about past regularities, while with regard to the future, one admits unpredictability in principle (implicitly).
The consequences for the methodology of the social sciences resulting from the recognition of the above 3 sentences can be summarized in the following two points
- An empirical social or action science generating prognostically usable knowledge is impossible.

If you ascribe to yourself as well as to other people the ability to learn - something that you have to do and always do a priori - then this logically implies that you have to regard future knowledge and action as unpredictable; and any action scientist who thinks that, for example, by formulating a prediction equation, he has set up a falsifiable law of action based on experience becomes entangled in a logical contradiction with such a statement and thus speaks nonsense.
- Social science in terms of empirical science can only be understood without contradiction as a reconstructive science.
- The possibility of predictions of an a priori science of action, i.e. of statements whose validity can in principle be assessed at any time on the basis of logical analysis alone, remains unaffected by the above evidence.

The action scientist only refutes the possibility of predicting future actions (and, it must be added, the future behavior of aggregate variables, insofar as these logically contain individual actions or are composed of them), insofar as these can all be influenced by unpredictable future knowledge (qua possible actions).
- Conversely, however, this just says:

Predictability with regard to a subject area that cannot in principle be influenced by possible learning that is unpredictable in its results remains undisputed by the above evidence.

In this way, however, only the range of logical action consequences, i.e. the consequences of presupposed actions in presupposed data constellations, is uninfluential for the field of 'action', this being precisely determinable: actions themselves, as well as social action sequences, i.e. action reactions empirically following given actions, can be influenced by unpredictable future knowledge and can therefore not be the subject of possible predictions; experience-independent, a priori valid, uninfluential by learning (whatever you may call it), are (and can only be) by definition statements about logical action sequences, i.e. statements whose knowledge or non-knowledge may be highly significant for the actor and may influence his actions in one way or another, but whose validity is independent of his knowledge or ignorance and of his factual action of any kind in the same sense as the validity of the Pythagorean proposition is independent of my knowledge of it and of it, however I may act with given knowledge.

- In short - it follows from the above evidence: If an empirical action science can only be justified as reconstructive, then logically, if there are any predictions in the entire field of action science, this part of science must have an a priori character.
For our topic: the justification of economics as a necessarily a priori science is obviously primarily the second consequence of importance.
Our argumentation has reached the point where it should be demonstrably clear that if there are explanations or prognoses in action science, such statements must have an a priori character, since an empirical action science can only proceed in a reconstructive manner.

The question that then arises is:

'How is economics qua a priori science possible? What do economic theorems that claim a priori validity look like?'

However, before addressing the constructive task of answering this question, it seems appropriate to us - as it were in parenthesis - to take a brief look at the question of why Popper did not also convince himself of the logically irrefutable insight into the above consequences.

Although he, like us, comes to the conclusion that actions, insofar as they can be influenced by unpredictable future knowledge, are also unpredictable; and, also still quite interpretable in the sense of our results, he also notes that this does not, however, generally question the possibility of social science predictions: on the contrary, he writes,

"the possibility of examining social theories - such as economic theories - with the help of the prediction that certain developments will occur under certain conditions remains quite open."

But Popper erroneously believes that these can be ‘normal’ empirical predictions comparable to those in the natural sciences.
Not only does the entire book, which is preceded by his argument, offer multiple proof of this, but nowhere, as might otherwise have been expected, is there an explicit statement that such social forecasts, despite his learning argument, are and must have the character of a priori statements.
The reasons for Popper’s lack of insight can only be speculated on here; after all, in this way, however, there is an opportunity to interweave some remarks that could be suitable for making more friends with the idea of the logical irrefutability of the above-mentioned consequence.
- From the point of view of cognitive psychology, the fact that the idea of an a priori economy was unknown to Popper is probably decisive for the error mentioned - a look through the name registers of his books shows in any case that neither the name L. v. Mises appears there, nor that L. Robbin’s work on the nature of economics seems to have been known to him. For example, the majority opinion of the specialist economists (which, due to erratic impressions of the achievements of the discipline, also seems highly plausible to the layman, especially prima vista) that economics is an empirical science, had to appear to him as an undisputed matter of course. However, due to this combination of ignorance and judgments based on ignorance (i.e., prejudice), he finds himself in a position that is unfavorable for gaining new insights from an epistemological perspective: he is unable to find the logically obvious solution to a problem he has raised simply because he is unaware of the idea that represents the solution as a readily available idea, and a prejudice prevents them from considering it on their own.

In addition to these psychological barriers, or, even more likely:
in connection with them, however, a self-misunderstanding of one’s own argument may also be responsible for Popper’s inconsistency.

He may interpret the statement that actions cannot be predicted, insofar as they can be influenced by a possible change in knowledge, as if only a definable class of actions were affected, while another class of actions could be determined (the one that economics as an empirical science is concerned with explaining), for which such an influenceability does not exist.

However, such an interpretation of the statement -- semantically and grammatically quite conceivable -- is unacceptable for logical reasons. Only the interpretation is logically flawless, which understands the above statement as ‘no action is predictable, insofar as all actions can be influenced by (unpredictable) possible changes in knowledge’.

The statement that future states of knowledge are unpredictable logically includes the fact that it is also impossible to predict which actions are or may be affected by possible knowledge change and which are not, because information in this regard is itself part of unpredictable future knowledge.

Therefore, the unpredictability of future actions applies a priori to all actions - whether they change over time due to corresponding changes in knowledge, or whether they remain stable,

as the knowledge manifesting in them: this can always only be reconstructed retrospectively. Only statements about actions that, like those of logic, are resistant to experience are unaffected by possible changes in knowledge.

Why ever Popper may have stopped a decisive step before this insight -- this consequence necessarily follows from his own statements.

IV

As the following explanations will make clear in detail, economics, although itself an a priori science, nevertheless differs in a remarkable way from the formal-apriorist disciplines such as logic and mathematics.

Only the extent of the peculiarity compared to the latter makes it at all understandable to what extent the scientific-logical character of economics could remain obscure for so long in terms of a priori discipline and has remained almost unchanged even after correct explication: prima vista nothing seems more strange than the common classification of logic and economics as a priori sciences. Nevertheless, this will have to be shown above all, there is nothing to be said about this classification; on the contrary, it was a serious deception if a different conclusion was reached with regard to this question; although economics has clear peculiarities, which presupposed blindness to be denied, their existence cannot change the justification of the above classification.

Just as formal logic does not begin unconditionally, but must be assumed by those who are supposed to understand it that they are able to understand elementary, logic-constitutive terms -- such as the attribution or agreement of predicates related to an object (i.e. the concept of ‘true’ and ‘false’, of ‘is’ and ‘is-not’), and also that of logical particles such as ‘and’ and ‘or’, etc., so economics also takes its starting point from basic concepts.

These concepts, like those of logic, are elementary and universal, i.e. any normal person can understand them without difficulty.

Economics qua a priori science of action begins with the concepts that must be characterized as universal categories of action: anyone who can act must conceptually reconstruct his actions, whatever their specific form, using these very categories - only by using them does he constitute something at all as an ‘action’; he therefore understands them because he knows what it means to act.

Anyone who can act knows what an action goal is; he knows that every action, in order to be an action, has a goal that has subjective value from the standpoint of the actor - more precisely, a higher value than the state from which the action seeks to achieve the corresponding goal (otherwise one would not act), and a higher value than all other alternatives that appear subjectively conceivable to the actor at the given time (otherwise one would act differently). In short, every actor can understand what it means to say that each of his actions seeks to satisfy the need that seems most urgent to him subjectively at a given time or (which means the same thing) the goal to which he subjectively attaches the highest value.

Furthermore, any actor can agree with the statement that any action, whatever its goal may be in order to be ‘action’, is characterized by the fact that in order to achieve the goal, it must use certain means.

Such means can be objects with a physical existence, but also things that cannot be described as such, such as ‘time’ and ‘services’: everything that an actor uses to realize a goal, and which, because of its corresponding servitude, has for him a value derived from the appreciation of the goal, i.e. the character of a good, is a means to a given goal.

- Means are necessarily scarce (this is the only reason why they have a derived value for the actor); so-called ‘free’ goods, i.e. things that are in abundance, are not means that an actor uses to achieve goals; they can, such as the air that is needed to breathe, be necessary for the maintenance of our existence - but as long as they are not scarce, they are not traded in relation to them, rather they represent a part of the environment in which they are traded.

Acting against it means: spending something that is scarce in the service of certain goals. (In terms of action science, economics is therefore not concerned in principle with free goods, but exclusively with means.)

Of course, something that is identical from a physicochemical point of view at different times or in different situations can be a (scarce) resource at one time and a free good at another. Something is not means-in-itself, but something becomes a means by treating it as scarce with regard to the realization of its goals, as something that has to be budgeted for.

("Time" is for us as finite beings the means that has an absolutely universal medium character; each of our actions manages (at least) with time, i.e. is under time pressure or treats time as scarce.
Even in the land of carnival, as long as human existence is finite, we would have to budget for time:
"Although all his (man’s) appetites could be satisfied immediately without any expenditure of labor, he would have to arrange his time schedule, as there are states of satisfaction which are incompatible and cannot be consumed at the same time. For this man, too, time would be scarce and subject to the aspect of sooner and later”)

Since every action, whatever it is aimed at, implies the expenditure of scarce resources, it therefore also has a cost aspect (in order to be an action), which every actor as a universal constituent of action can also easily reconstruct. Every action causes costs for the actor - as is expressed in everyday language, for example, also true with regard to the scarce means of action time through the statement time is money.

The costs of a given action are equal to the appreciation that the actor gives to the subjectively most important need (goal), which he cannot satisfy (strive for) because the means necessary to satisfy it are tied up or spent in the pursuit of the (more highly valued) need (goal) to be satisfied with the action in question.

If an action then, although its objective is always worth the corresponding costs to the actor ex ante, ex post leads to a state that has less value in the subjective assessment of the actor than the costs associated with the action according to subjective assessment, this is referred to as a loss;

if, on the other hand, a situation is realized that exceeds the costs incurred in terms of value, even from a retrospective point of view, it is a benefit or profit achieved by the action.

- Each action, in accordance with the conceptual provisions reconstructed here as action categories, always and without exception strives for a (maximum) profit;

but our non omniscience with regard to the 'technical' prerequisites for the achievement of goals in the broadest sense (one can learn, i.e. also: one can be wrong!), as well as the fact that the time difference between the beginning and end of action itself allows a change in the subjective evaluation standards to be possible, threatens, without exception, any action with the possibility of (also) a loss.

Economics is based on the understanding of these terms.

In order to understand their theorems, in principle one does not need more than their knowledge, which can be assumed for every actor, especially because they are the categories that everyone, regardless of what concrete form their actions may take, and regardless of under what special conditions they act, yes, in general: regardless of what the world in which they act looks like - must use even for the most general conceptual characterization of their action, as long as they only band at all or as long as it is only an action at all.

How does economics, starting from the conceptual contexts that have just unfolded, now arrive at a priori knowledge, above all: at a priori knowledge, which at the same time represents knowledge about reality, and also: which is not trivial, at least less trivial than the statements presented so far and which can be understood as valid (analytically true) a priori on a universal scale?

The answer to be elaborated immediately afterwards is:

By constructing and/or reconstructing more or less complex worlds and worlds changing according to more or less complex patterns, in which the general categories of action experience a concrete determination, and then unfolding for data constellations and constellation changes assumed in this way, which means the execution of certain actions within their framework (logically implied).
The statements of economics, trivial or non-trivial, are therefore first of all, to emphasize this once again, fundamentally not concerned with the explanation of actions and worlds of action.
When which persons, using which means and estimating which costs, pursue which goals and thereby make which losses or profits, is in principle inexplicable; if, undeniably, one constitutes oneself as capable of learning, then there are no laws of action that can be falsified by experience, on the basis of which conditional forecasts could be derived, but only information that can be reconstructed as data from the past.
Being able to learn means perceiving the nature and sequence of future mental disequilibrium states as unpredictable, and accordingly also the activities that appear suitable for resolving such disequilibria.

- On the contrary, it is characteristic of all statements and theorems in economics to start from an unexplained presupposed world. Which world aspects are important for the economist as the data to be assumed for his activity can be seen from the general categories of action:
He begins his work on the basis of presupposed data on action goals as well as on the constancy and change in the subjective evaluation of certain goals; on the concrete means used in the pursuit of goals, and the changes in the scarcity of these means occurring over time; on the

subjective costs associated with certain actions, and the change in subjective cost evaluations; and finally on the losses and/or profits achieved by certain actions resulting from a subjective comparison of values.
The world from which the economist starts - each time supplied with unexplained presupposed data of the type(s) mentioned - can be more or less complex:
It may be a so-called Robinson economy, in which one only deals with the relevant data (values, resources, costs, etc.) for a single person; it may be a world in which a large number of actors find it valuable to cooperate and in which goods are exchanged; it may be a society with or without a medium of exchange (money) that enables the indirect exchange of goods; it may be one with or without monopolies and monopoly prices; In the worlds that form the starting point for economic analysis, a wide variety of actions may be threatened with the most diverse forms of social sanctions, and the performance of such actions may therefore (if the relevant threats are perceived by the actors) be burdened with costs of various kinds. The worlds of action can be characterized by capital-free or capitalist production, i.e., by actions that only achieve their actual goal—consumer goods—via intermediate products (capital). Means of action can be more or less specific, i.e., they can be made useful for fewer or more distinguishable goals. Means of production, like end products, may be divisible at will or not; the realization of certain goals may require the combined expenditure of few or many means, connected in simple or complex ways; etc., etc.

The pure economics theorist is in principle not subject to any restrictions in such constructions; his interest does not apply to the construction itself and its content of reality; it merely forms the starting point logically necessary for his activity - his task as an a priori action theorist is rather the determination of the logical consequences that result from the execution of a given action within the framework of such presupposed constructions for these constructions themselves or for their modification, regardless of how realistic the constructions themselves may be. It is only by choosing realistic constructions as the starting point of the work that economic analyses, of course, acquire real significance.

In other words, it is only the fact that the construction is at the same time the reconstruction of data constellations that are more or less frequently encountered that makes the statements of pure theory - namely those that are formulated as a prognosis - applicable.

However, even as a scientist interested in exclusively realistic constructions or applicable predictions, the economist remains an a priori action theorist:

on the one hand, this is expressed in the fact that he can arrive at statements about consequences of action that can be clearly assessed with regard to their validity within the framework of unrealistic, even completely unrealizable data constellations, even if such constellations may understandably not be his actual curiosity;

on the other hand, it finds expression in the fact that, accordingly, statements with use cases are also independent of the outcome of their application in the field of experience. Such statements are by no means tested in the application.

This does not mean that important information cannot follow from the application for the action theorist: just as a mathematician may only be made aware of errors in his static calculations through certain experiences (e.g. the collapse of a bridge), so the action theorist may only be able to come across logical errors in his own statements through his apodictic statements.

However, it is crucial that these errors - in one case and in the other - could have been found at any time in the principle even without such undoubtedly stimulating experiences, and that the new proposed solutions following the error discovery also claim in principle to be the unambiguously and definitively correct solution (regardless of all further, future experiences).

Heuristically, therefore, experience can undoubtedly also acquire significance for an a priori science; strictly speaking, however, experience can neither confirm nor falsify its statements. The assessment of their validity is fundamentally the task of pure reason.

Even in the result of realistic constructions, the economist does not invariably follow the rules of empirical research in the derivation of his statements, but - and this constitutes the status of economics as an a priori science - the method of pure logical analysis, i.e., he subjects himself to the compulsion to experiment with thoughts, which results as an inner necessity of pure thinking.

Even and especially the economist who strives for realism is subject to this compulsion:

In order to be able to derive the logical consequences of action within the framework of realistic data constellations, i.e. applicable forecasts, it often proves necessary to analyze contrasting action under such conditions that differ in essential aspects by their obvious unreality from the situation to which the actual interest applies.
Similarly, for the understanding of logical action sequences under extremely complex data constellations, it often proves necessary to initially start from abstractly simple constructions in order to gradually approximate the image to the real complexity by introducing additional prerequisites.

V

The following analyses also begin with abstractly simple constructions, in order to then, last but not least, also consider an undoubtedly realistic, concretely complex example:

According to the previous, possibly quite academic, explanations of the logic of economics qua aprioristic science, before these explanations can finally be rounded off to a complete picture of the peculiar character of economics, the following should first be examined by way of example:

To what extent does the introduction of a given action result in consequences for the world that can be determined purely logically—regardless of whatever changes to the world the actor may have intended—based on a presupposed world of values (goals), means, costs, and gains (losses) with a given degree of complexity and reality? Or, changing the perspective of the question, to what extent can a change in the conditions of the assumed target-means worlds, regardless of whether this change is produced or has arisen naturally, have logically determinable consequences for action under such changed conditions, consequences that can be determined independently of how the actors themselves act under the respective data constellations?

The analytical part of the answer to this can be formulated relatively unproblematically and quite clearly as follows:

Since every action, as can be seen from the categories of action reconstructed above, expends means and, in some cases, also produces (other) means (which can then be used to pursue further goals), it naturally changes the value-means-data constellation from which the action is assumed to proceed—whether the actor himself assesses it that way or not; and just as naturally, a change in the data of a presupposed goal-resource world must have consequences for action, because goals and resources are not objects that exist in themselves, but nothing more than categories of action.

However, it is not the analytical part of the answer, which can be determined so concisely, that is of primary interest in connection with the questions posed.

Instead, what is to be attempted below is to provide an answer to the questions by concretely demonstrating, using some selected examples, how those determinations that address the questions as a problem are made in practice.

In other words: The answer should be that economic analyses are presented in an exemplary manner in terms of logical analyses, i.e. as the results of pure thought processes.
Three examples - two of them, as already indicated, rather abstract-simple, the third comparatively complex - are to be discussed and analyzed in some detail for this purpose. In all three cases, these are theorems that are generally accorded an important place in the system of economic knowledge - although, as has already been said, they are not regularly understood as theorems of an a priori science, but are empirically misunderstood.

a) the marginal utility theorem or the law of decreasing marginal utility;

b) the so-called law of returns;

and finally

c) following a preliminary analysis of the premium-interest theorem (time preference theory of interest), the monetarist (Austrian) theory of the business cycle.

The marginal utility theorem is an example of a statement that states the logical consequences for action resulting from a change in the data of the world of actions:

The world that forms the starting point here is characterized by an offer of a certain quantity m of a certain homogeneous good G; and the (only) change that is introduced consists in a change in the offer of means of action by one unit (where one unit is what is perceived as such).

The marginal utility theorem derives what such a change logically means for any actor confronted with it.

The analysis extends equally to the case in which a supply of goods decreases from in to m-1, as it increases from m to m+1.

It says: If it has hitherto been possible to use m units of G as a means of satisfying needs, and now only m-1, this means -- assuming the analytically true proposition that everyone always and without exception does what seems most important to him subjectively under the given circumstances see Sect. 51) -- that one must dispense with the use of a unit of G which one considers to be the comparatively least important or the least urgent; and, conversely, if the supply of G increases from m-1 to m or from m to m +1, this means that the increase in means

can be used to satisfy that need which remained unsatisfied until then (i.e. with an offer of m-1 or m) is the most important of the needs that can be satisfied by additional units of G, but at the same time the most unimportant of which is now actually satisfied by a G unit, since one would already dispense with its satisfaction with a decrease in G by only one unit.

If we now name the use to which a unit of G added to a given stock of G would be added (but could not be added if G remained constant) or the use that would have to be dispensed with in a stock reduction by one unit of G (but not actually dispensed with if G remained unreduced, constant), marginal employment, and the subjective appreciation derived from it, marginal utility, then the law of decreasing marginal utility results directly from the already stated conceptual relationships.
It is stated that for each homogeneous good G, each additional unit added to a given quantity m of G can only ever produce a marginal benefit that is lower than the marginal benefit that a unit of G can produce if the corresponding supply with respect to G were one unit lower; for each additional unit can only ever be used to satisfy such a need, the satisfaction of which is subjectively less urgent than the satisfaction of even the least important of those needs that a G-unit is able to satisfy in the case of an unabundant supply, in such a way that the satisfaction of the former, but not that of the latter, would not take place or would be reversed.

This law is analytically true, as Mises also states in a corresponding context. The statement is, as he says, "Journal and aprioristic and does not depend on any experience"; it has nothing to do with psychology or the like, and a psychologist who believed that he had to and could subject it to an empirical review could at best make himself ridiculous: While the total utility that a given quantity of any good can generate can certainly increase with each additional unit of that good -- and exactly as long as an additional unit still has a positive marginal utility for the actor, this is the result of purely logical analysis, whenever the supply of a good increases by one unit, necessarily the marginal utility that this (last) unit can provide for the actor.

The marginal utility theorem is definitely not an empirical law of action. Especially, it cannot be reinterpreted into such a law, because the concept of such laws is logically, as shown, contradictory.

Nevertheless, such an empiricist interpretation, which is not merely a simple misinterpretation revealing ignorance of the classics of subjective value theory, but rather a logically inadmissible (mis)interpretation, has become almost the norm among economists in the wake of the enforcement of a naïve empiricist philosophy of science.

Samuelson, as the most representative example, gives the following version of the law:

"As you consume more of the same good, your total (psychological) utility increases. However, let us use the term marginal utility to refer to the extra utility added by one extra last unit of a good’. Then, with successive new units of the good, your total utility will grow at a slower and slower rate because of a fundamental tendency for your psychological ability to appreciate more of the good to become less keen. This fact, that increments in total utility fall of, economists describe as follows:
"‘as the amount consumed of a good increases the marginal utility of the good (or the extra utility added by its last unit) tends to decrease.’"

- Our assertion that this statement presents the law of diminishing marginal utility as an empirical law probably does not need to be substantiated further: that one becomes ‘less keen’ with

an expanded offer, and, above all, that, as the last sentence states, one only 'tends' to do so - this clearly proves our thesis.

Both interpretations that can now be given to the law formulated in this way are, of course, unacceptable in the double way characterized.

If we first consider the interpretation according to which the decreasing marginal utility should express itself as such in the context of psychological tests that have utility measurements as their subject matter: it represents a misinterpretation in that it represents a measurable (interval) variable according to the utility or value of the subjective theory of value, within the framework of which the marginal utility theorem was developed.

Neither interpersonal benefit comparisons can be made, nor intrapersonal (across time differences). Decreasing marginal utility can therefore not be expressed as a measurable or quantifiable (psychological) phenomenon and the marginal utility theorem can therefore not be understood in principle as a law of decreasing degrees of satisfaction or the like. According to subjectivist theory, value (utility) is not a psychological phenomenon at all (at least no longer psychological, as the categories of logic are psychological), but an interpretation category (with an ordinal character) for actions: just as one only with the use of the dichotomous logical categories 'is' or 'is not' constituted something like 'facts' (i.e. can interpret something as a fact), so one only on the basis of the ordinal category 'value' constitutes something (which is otherwise only movement or even standstill) as an action to be understood as a unit with goals that are preferred over other goals.

Apart from this existence as a logically necessary instrument for constituting actions in terms of actions, values have no existence for subjectivist value theory.

But that's not all: even if one wanted to accept this (mis)interpretation as an interpretation endowed with its own right, this would not even be logically permissible.

If the statement regarding decreasing levels of satisfaction refers to learning objects, it cannot be regarded as a 'normal' conditional prognosis, as demonstrated in detail, which is capable of confirmation as well as falsification, but should be understood as a prognosis of the type of a self-fulfilling or -destroying prophecy, i.e. as a prognosis to which the test persons themselves can react in a fundamentally unpredictable way. For purely logical reasons, such a test can never be a test of an empirical (psychological) law, but only data from the past can be determined.

The rejection of the second conceivable interpretation of the empirically formulated marginal utility theorem looks quite similar, according to which a decreasing marginal utility - as it were behavioristically - is expressed in the fact that increasingly smaller amounts of any comparable goods are exchanged.

Again, this was a misinterpretation of the subjectivist theory of value, insofar as it is based on objective measurability of value (here: in units as an objective measure of comparative goods), while for subjectivism (for which 'value' alone has the meaning just explained of a logical interpretation category, according to which it finds expression exclusively in actions in terms of electoral actions and, apart from this, i.e. preferences manifesting in actions, has no existence), it is a completely normal fact compatible with the marginal utility theorem if, with the growth of the units of a certain good A, any(!) changes in the exchange ratios of A units to units of any other commodity, such as a good B, occur:

If, for example, in the case of an offer of n units of A, one unit of A should be exchanged for four units of B (and thus four B are preferred over one A), but in the case of an offer of n + 1 units A only for six B -- an event that would contradict the statement of the marginal utility theorem in the second empirical version -- this too (like any other change) would be a normal process without consequences for subjectivist value theory itself.

It meant no more than that the value of A units manifested in (exchange) actions has increased compared to B units - irrespective of the increase in supply with regard to A - while, despite this, in the transition from n to n+1 offered A units, there has necessarily been a decrease in the marginal utility of an A unit, insofar as the additional unit can only be used to eliminate such unsatisfaction, which is less pressing than all those needs that appear to be actually satisfactory in a scarce supply of n A by an A unit.
And again, such an interpretation would not only be wrong as an interpretation, but also logically illicit, since the concept of constant laws, related to an object area of learning persons, is logically contradictory:
If one can learn, and learning outcomes are unpredictable (i.e. can only be reconstructed), then it is also unpredictable whether and if so in what way changes in the knowledge about which means (goods) can be used for which purposes (goals) or about which changes have resulted or will result with regard to the offer of which means affect the relative valuation of different goods.
An empirical law of decreasing marginal utility of the type specified above cannot exist for unavoidable logical reasons.
The marginal utility theorem is and can only be: a pure truth of the mind.
A hypothetical statement of law about how actors actually react to an increased or decreased supply of goods—whether in the context of test-based goods evaluations or in the context of everyday evaluative handling of such goods—is not possible without becoming entangled in logical contradictions.

Only about this is a statement permissible, which are the logical consequences that result for actors from the fact of an increase (decrease) in goods.
They are unfolded in the marginal utility theorem.
As a praxeological theorem, his statement may seem trivial - but the aim of our remarks is not yet to develop a complex praxeological theorem, but rather to exemplify the structure of such theorems in the first place.
That in the context of a priori statement systems - and this applies to economics no less than to logic - the analysis results cannot be more complex than the degree of complexity of the assumed data constellation or data constellation change requires, should be understood as a matter of course; in any case, they do not represent more than the development of all conceptual implications already contained in the prerequisites (and also: non-implications).
It is therefore only natural that the law of diminishing marginal utility can be no less simple and abstract than its point of reference: a simple multiplication (reduction) of goods.
- Whatever the case may be, however, it should not be forgotten that, in the event of such a simple increase in goods, no more can legitimately be ventured in terms of action-scientific prognosis than what is expressed by the marginal utility theorem. This, above all, should not be lost sight of as a non-trivial result of the previous explanations—however trivial the marginal utility theorem itself may be, in principle, nothing more can be said about the consequences that a change in the supply of homogeneous goods has for action.

Even the second example of a theorem of pure economic theory to be considered can certainly still be considered simple -- simply because the data constellation itself, which forms the starting point of the analysis, is simple.
Nevertheless, we believe that even the discussion of this second example will convey a notion that even the praxeological consequences of simple assumed action-world constellations do not have to be as self-evident as it may have seemed in the case of the marginal utility theorem.
The law of returns (often also, as will be shown: rather misleading, called the law of diminishing returns) makes a statement about the logical consequences for action resulting from the fact that in order to achieve a certain goal, not only one means must be used, but two (or more) in combination.

Just as the marginal utility theorem answers the question:
"what are the logical consequences for an (each) actor resulting from an increase (decrease) in goods?", the income law answers the question: "what are the logical consequences for an (each) actor if he has to use more than one good in order to achieve a given goal?"

Whether there is more than one homogeneous good at all, and whether there are goals for actors for the realization of which the combined expenditure of several goods is necessary (and if so, which specific goods for which specific goals), is not the subject of the law of returns.

Although the assumption of such goals is undoubtedly a realistic assumption (in the pursuit of almost all our goals it is given), for the law of returns it is the unexplained starting point of the analysis. Just as the marginal utility theorem presupposes changes in the supply situation with regard to homogeneous goods, but in no way explains, nor does it explain, which de facto consequences of action a changed supply situation triggers, but only explains what this logically means for each actor, irrespective of whether there are actually changes in the provision situation in reality or not, and irrespective of what the de facto reactions to it may be, the law of returns does not explain when or why which means are de facto combined in what way in order to achieve which goals, but presupposes and explains what target pursuit means under this prerequisite, irrespective of what the concrete goal may be, and irrespective of which means are de facto combined in what way.

The law of returns makes the following statement:

If several means (goods, production factors) always have to be used to achieve a certain result (action goal), there is an optimal combination ratio for the means combination.

This optimal combination ratio is determined, for example, for two production factors F1 and F2 and a given target Z experimentally in such a way that, for a given input quantity f1 of F1, that input quantity f2 of F2 is determined which optimizes the output/input ratio Z/f2: accordingly, the f2 value kept constant would be used in the determination of the optimal f1 value, which also results in the optimal f1/f2 ratio with regard to the realization of Z.

Only such an optimal combination ratio ensures that there is no waste of the productive capacities of the production factors with regard to the achievement of goals;

conversely, any deviation from this ratio means a waste of part of this capacity in such a way that increasing the input quantity of a factor above its optimal value while keeping the other factors constant at their optimal values

- either leads to the output no longer increasing at all, or at least no longer in the same proportion as the additional input. (An increase of f2 above the optimal value, with an f1 fixed on its optimal value, would lead to a sub-optimal Z/f2 ratio.)
Only this - that there is such a determinable and characterizable means-combination ratio for all the goals that can only be realized with a combined expenditure of several means - claims the law of returns.

Any action that pursues such goals must, according to the statement, use or have used the corresponding means either optimally or suboptimally combined.

As indicated, the validity of this can be derived as a logical implication solely from the fact that it actually requires the combined expenditure of more than one (scarce) means in order to be able to achieve the corresponding goal.

This can be proven by showing that an optimal combination ratio of the combined production factors can only fail to exist if one of these factors is not in fact an economic good, or, in a more precise positive determination, only if a means does not even possess the necessary prerequisite for determining a good as an economic good, such that even if it were a ‘free good’ at a given point in time, it could not become an economic good due to shortages.

Accordingly, the proof begins with the explication of what is to be regarded as this necessary prerequisite.

It consists in the fact that something has to be consumed by a factor in the course of the production process (i.e. the actual realization of the goal), so that a certain output can be assigned to a certain input, and thus there is a definite input/output relationship.

Conversely, nothing would be used up by a factor in production, and there would accordingly be no definitive input/output relationship for it, since it would not only be inexhaustible itself - if it only exists at all - but would also be able to produce unlimited quantities of the target variable, insofar as the achievement of the goal depends on its use - so this factor lacked the necessary prerequisite for being or becoming an economic good. An actor would never have to economize with such a factor; its use incurs no costs whatsoever, since it is not expended, and its use for one purpose does not preclude its use for another; and once given, it has no value for the actor, since it is inexhaustible and indestructible, and the actor can therefore never be in a position to have to choose between disposing of it and disposing of anything else.

- (The ‘knowledge’ of how to achieve a certain goal is such a fundamentally non-economic production factor: once possessed, it is not used up in the production process, and can be used as often as desired with a view to achieving the goal:

"The formula, the recipe that teaches us how to prepare coffee, provided it is known, renders unlimited services. It does not lose anything from its capacity to produce, however often it is used; its productive power is inexhaustible; it is therefore not an economic good.")

If such a basically non-economic production factor is used together with an economic one - and they both have to be used in order to be able to realize a certain target variable - then an optimal combination ratio between them cannot result for the following reason:

Since the non-economic production factor, once given, has unlimited productivity with regard to the target, the definitive output quantity can depend exclusively on the definitive input quantity of the economic production factor used.

Or in other words: any output quantity can be generated solely on the basis of the variation with regard to the input quantity of the economic factor (assuming that the non-economic factor exists at all, and the supply is correspondingly large with regard to the economic factor) -- and there can be no output quantity that could not be achieved by varying only this one factor, because the other, non-economic production factor is not variable, but develops an always equal, constant effectiveness as a factor of unconsumable production capacity, no matter how many input units of the economic factor are consumed in the production process; in other words, it is impossible that a variation of the non-economic factor (an increase or decrease in the input quantity), with the input of the economic factor kept constant, changes anything in the output quantity, because a factor that is unconsumable in the production process, once present, cannot be increased or reduced, but is and remains inexhaustible.

However, the very existence of output quantities, which cannot be achieved solely by varying the input quantity of one factor, but which require the simultaneous variation of the input of the other, is the hallmark of an optimal combination ratio.

This optimum cannot exist for the ratio of economic/non-economic production factors, because inexhaustible factors are naturally unvariable with regard to their input quantity and thus the variability of the output quantity can only be attributed to the variability of the input quantity of economic factors; conversely, however, and this was to be shown, such an optimum must necessarily exist if the combined factors, as is always the case with economic factors, are both consumed in definite amounts in the production process and the input quantities can thus be varied in two ways with respect to one another: if the production of a certain target variable requires the combined use of several factors and these factors are not inexhaustible, but are consumed in the course of production in amounts, then it is logically excluded that each output quantity can be realized solely by increasing the input quantity of only one factor -- there must be an optimal combination ratio from which one cannot deviate without wasting productive resources.

The experience-independent, i.e. a priori validity of the law of returns is thus demonstrated.

The statement that there is (and must be) an optimal combination ratio for means combined in the production process for the purpose of achieving goals follows logically from the assumption that the combined means are those that meet the necessary prerequisite for being eligible as an economic good.

-- And as a logical truth, the following can also be stated:

If there is an optimum combination, then in the area of transition from one factor combination ratio to another, which can be characterized (at a constant and a varied factor) as an approximation to the optimum ratio, there must be the phenomenon of what the law of increasing return makes the object of its statement -- the phenomenon that, compared to the growth rate of the variable input factor, overproportionate growth rates with regard to the output quantity can be achieved; and conversely, in the area of increasing distance from the optimum ratio, what the statement of the law refers to of decreasing return must apply -- disproportionate increase in the output quantity compared to the growth rate of the input caused by additional units of the variable factor, and finally a point beyond which additional units of the variable factor no longer

achieve an increase in the output quantity without a simultaneous increase in the input of the factor kept constant.

- If isolated statements are each incorrectly or at least misleadingly shortened, the laws of increasing or decreasing returns result as partial truths of a general law of returns on which they are based; due to a correctly formulated law of returns and its a priori validity, they are both proven to be true within logically exactly determinable and distinguishable areas - as it were as the two sides of one and the same medal.

In view of this, the law of returns and the laws of increasing or decreasing return that it implies as partial truths are decidedly not to be understood as laws of technology. In contrast to such laws, their validity is independent of experience; they represent a priori truths and are therefore praxeological laws, which as such have as little (or as much) to do with the concrete technological, scientific or occupational aspects of production as logic has with psychology.

However, just as numerically the marginal utility theorem also specifies what a priori statements about the consequences of commodity multiplications cannot be made, and what therefore falls within the scope of a reconstructive empirical social or action science, the law of returns as a praxeological theorem also implicitly provides exact information about which questions in connection with it can only be decided or answered on the basis of aposterior experience and therefore fall within the scope (this time not of empirical action science, but that) of the generally empirical production technology. Obviously, this includes the questions left unanswered by the law of returns itself:

What is the optimum ratio for specific goods to be combined in the production process?

Specifically, what does the curve look like that shows the change in the output quantity for certain combined factors as a function of changes in the input of the varied factor for the range of approximation to the optimum combination?

What does it look like for the range of increasing distance from this optimum, and where is the point from which a change in the varied factor alone, without a simultaneous change in the constant(s), no longer causes any changes in the output quantity?

- Answers to this can only be provided by the non-apriorist experience of production technology.

However, the law of returns itself is valid regardless of experience—and if, misled by an empiricist scientific ideology that currently dominates the self-image of economic science so completely, one nevertheless considers it to be a law of production technology whose validity depends on empirical experience, then such a conviction is as false as it can possibly be.

For example, if Samuelson (once again cited as a representative example of empirically misguided economists) writes that ‘the law of diminishing returns is a fundamental technological law’; if he then gives it the following formulation, which is quite inaccurate compared to the correct version:

"an increase in some inputs relative to other fixed inputs will, in a given state of technology, cause total output to increase; but after a point the additional output resulting from the same additions of additional inputs is likely to become less and less. This falling off of extra returns

is a consequence of the fact that the new ‘doses’ of the varying resources have less and less of the fixed resources to work with";

and if he shows with undoubted clarity from the context of his remarks - even more than from the cited formulation itself - that this law is, in his opinion, an empirical law with all the characteristics of empirical laws, such an attitude cannot be classified otherwise than in the following way:

It is analogous to the attitude of (and corresponds to it in terms of its non-rationality), who does not recognize that theorems of mathematics and logic, formulated accordingly, are capable of a non-empirical justification, and who instead tries to justify, for example, a Pythagoras translated into an empirical language exclusively empirically.

From a psychological point of view, such an error may be more understandable with regard to the law of returns than a corresponding error with regard to Pythagoras—from an epistemological point of view, however, both cases involve the same type of error.

VII

The discussion of the third, more complex example of pure economic theory will end with a very similar result:

The presentation of the basic structure of the pure (logical) theory of the business cycle will show to what extent monetarist theorists such as M. Friedman are subject to an epistemological self-misunderstanding, which on the one hand leads to significant inaccuracies in the formulation of the theory, and on the other hand to non-theoretic pseudo-precisions if they understand their business cycle theory as an empirical, comprehensive statistical-econometric theory in need of review.

An empirical theory of the business cycle cannot exist for reasons of a principled, logical nature described in detail above. Not only is a corresponding monetarist theory untenable a priori; every empirical theory is -- be it one of the so-called exogenous cause theories such as W. St. Jevon’s sunspot theory, theories of investor psychology à la Keynes, or Schumpeter’s innovation boost theories, be it one of the so-called endogenous cause theories such as Marxist or Marxist-inspired underconsumption and/or disproportionality theories, or be it any synthesis from these and other theories such as eclecticisms à la Samuelson.

The conception only through the experience of falsifiable hypotheses regarding constant relationships between certain cause variables on the one hand, and the phenomenon of the business cycle on the other, is contradictory in itself - regardless of whatever is envisaged as a cause complex in this context.

Since the economic cycle is undeniably generated by actions or actors - it is actually a pattern that changes according to a certain flowchart, which can be seen as the dominant general pattern of action in the large number of individual actions - and the actors themselves can learn in a fundamentally unpredictable way (of course also with regard to supposed laws of action), the idea of an empirical-causal-scientific business cycle theory, from which (ex ante) conditional forecasts can be derived, represents a logical absurdity:

Forecasts can logically be understood without contradiction only as forecasts of the type of a self-fulfilling or -destroying prophecy, and the corresponding theories are basically no more than historical experiences about data constellations associated with the phenomenon of the business cycle in a contingent way.

Physical or political-social events, mass psychological phenomena, intersectoral development disproportionality, population movements, or whatever else - they may all be reconstructable ex post as data constellations that were associated with the phenomenon of a business cycle and/or are given by the actors themselves as the reason for a general, cyclically changing pattern of action: however, they do not explain the business cycle or make it unpredictable.

An explanation (prognosis) of the same by explaining a cyclically varying general pattern of action can only be a logical explanation.

In other words, the theory of the business cycle must make it comprehensible as a logical result of a certain, presupposed world of action, as a phenomenon whose occurrence is logically implied in the conceptual determinations of a presupposed constellation of data, which, moreover, since the phenomenon to be explained is undeniably 'real', must in turn be characterized as a 'realistic' construction.

Only in terms of praxeological theory does it permit the derivation of statements that cannot be interpreted from the outset - because of the fundamentally unpredictable learning ability of the actors, whose general pattern of action has the statement as its subject - only (and only) as such via inconstant, contingent historical facts (thus not as conditional predictions in the proper sense of the word), which can rather be regarded as real predictions (with apodictic validity, of course):
only praxeological relations can be discovered, but learning cannot fundamentally overtake them and thus relativize them to historically variable conditions -- learning to overtake them in connection with the business cycle phenomenon can not be the validity of the praxeological business cycle theorem, but only the occurrence of that data constellation that, once it exists in reality, then with logical necessity produces a boom-bust cycle.

By unfolding the monetarist theory of the business cycle not as a praxeological but as an empirical theorem, it, like any other theory of the trade cycle, becomes logically contradictory in its conception.

Nevertheless, as has already happened indirectly in the immediately preceding explanations, it deserves a prominent position in contrast to all non-monetarist theories. Only it can be proven, provided that it is translated from the false language of empirical science into the correct language of 'pure' science, with a few modifications, to be accurate, i.e., logically correct theory.

All non-monetaristic theories, on the other hand, are wrong in two ways: they are not only not empirical theories that allow the derivation of conditional forecasts, but they are also, as will only indirectly result from the following presentation of the logically correct business cycle explanation, understood as praxeological theorems, logically incorrect or inadequate theories of the business cycle.

The theory of the business cycle must do the following:

Firstly, as its explanandum, it must explain the phenomenon of a general expansion of economic activities, which prima vista looks like a normal expansion, but must actually be characterized as a boom that brings about a more or less pronounced restructuring of the economy, only to be replaced by a general economic downturn in which the entrepreneurial activities set in motion in the boom phase prove to be loss-making (in monetary terms) on a general scale (higher than normal contractions).

And secondly, as a praxeological theory, it must provide an explanation according to which the business cycle is the logically necessary result of an action world data constellation preceding it in time, in other words, it must offer an explanans that conceptually implies a time-shifted boom phase that can be determined analytically unambiguously as a boom in contrast to normal expansion, and that, if necessary, also makes an actual economic event, regardless of all appearances, and regardless of the fact that the economic phenomenon can actually be superimposed by other, temporally coincident phenomena, unambiguously and undoubtedly identifiable as a boom; and it must offer an explanans that conceptually implies that the boom phase necessarily follows a recession phase that, according to what has been said about the boom, becomes unambiguously identifiable as a recession with a-normal entrepreneurial (monetary) losses in its frequency, and that also makes empirical recessions unambiguously identifiable as such, regardless of any superimposed expansions.

The answer to the question of the appearance of such a theory can be reconstructed by first developing, in a first step, the so-called time-preference theory of interest, which owes its existence, after important preliminary work, especially by W. St. Jevons, as is known, primarily to E. v. Böhm-Bawerk: a precise understanding of the importance of the interest phenomenon is an indispensable element in any attempt to successfully explain the business cycle phenomenon.

The theory can be understood as a logical implication of essentially three presupposed, analytically true theorems or of their combination:

a) the statement that all persons at all times and without exception pursue the goal of action that appears to them to be comparatively the most valuable or most important under the given circumstances;

b) the statement that every action takes ‘time’ to achieve the goal, so that no matter what the goal is, as long as you act to achieve it, you have to accept a certain waiting time, be it short or long, before the goal is achieved;

and

c) the statement that ‘time’ is fundamentally scarce for us as finite beings and therefore, since we cannot wait forever, but are always under the compulsion to have to meet needs ordered by urgency, the length of the waiting time causes costs that are fully included in the benefit-cost calculation of every action as a cost factor.

A number of other statements can be derived from these statements:

If a longer waiting time means higher costs, and you always do what can be expected to bring the greatest subjective gain under certain circumstances, then this means that if there are several technical implementation options for a goal considered identical, which differ in terms of the costs associated with their implementation alone in terms of the length of the required waiting

time, you will always choose the technology that requires the least waiting time. It generally follows from the above statements that an actor only accepts an extension of the waiting time if he values the product (goal) of the corresponding action higher than the satisfaction of all those needs that he has to forego as a result of the choice of an action goal with an extended waiting time; or, in other words, the realization of an action goal with a certain, given waiting time only comes into consideration for an actor if the satisfaction of all those needs that appear during the time of waiting appears to be ensured beforehand, and whose satisfaction appears to be subjectively more urgent than the desired goal itself or the satisfaction that can be derived from its realization.

If one further realizes that each action, by pursuing a need-satisfying goal, consumes means or goods, and if one calls those means that one could consume for satisfaction purposes, if one did not aim for a goal with an extended waiting time, but which one actually saves in order to be able to aim for a goal that requires such an extended waiting time, as well as those means that one could consume if one only pursued action goals with comparatively shortened waiting times, but which one does not consume, but actually obtain, in order to aim rather at those action goals that require a given waiting time, capital or capital goods; thus, if one defines capital as the goods whose non-consumption makes possible the bridging of given waiting times or the bridging of the waiting time extensions associated with certain actions (goals), the following statements result from the above:
Only then does an additional accumulation of capital occur if the actions made possible by this with an extended waiting time lead to the expectation of results that represent a higher satisfaction than that of which one has to get rid due to the non-consumption of the additionally accumulated capital; but also only then does such an additional accumulation occur if previously the supply of all those goods whose consumption during the extended waiting time becomes necessary to satisfy all those needs whose satisfaction can wait less long than the satisfaction of the need that requires the realization of an action goal with an extended waiting time appears to be ensured.
And there is also an explanation for the extent to which additionally accumulated capital results in higher productivity, but nevertheless, despite waving productivity increases, in fact (also) production methods with a sub-optimal input/output ratio can (could) be used.
- The extended waiting time made possible by additional accumulated capital allows to increase productivity

a) firstly, by allowing the use of (technically known) production methods which, although they produce a larger output of goods already produced per input unit, require a correspondingly long waiting time compared to the conventional method in order to come into effect,

b) on the other hand, by allowing the production of goods that cannot be produced at all in the shorter time, or cannot be produced in a certain quality, and

c) finally, by allowing the production of additional and/or other goods in addition to the production of (quantity of) goods already produced;

but on the other hand:

the use of the advantages offered by increased capital accumulation is always and at all times limited by the fact that, before it can take place, the supply of goods must appear to be secured with regard to the needs whose satisfaction can wait less long than the waiting period would

require - if this is not the case according to subjective assessment, the objective advantages that greater capital accumulation entails will not cause it to be brought about by non-consumption.

With each action, it can be summarized, an actor necessarily makes a decision about the extent to which he is willing to renounce the satisfaction of more or less present needs in favor of the satisfaction of more or less future needs or to what extent such renunciation is limited by a higher urgency of present needs compared to future needs burdened with higher waiting costs;

with each action, goods are valued in terms of present goods, i.e. those whose consumption could satisfy needs arising in the present, and compared with the value of future goods, i.e. those that - due to the non-consumption of the present - can only be used for the satisfaction of needs within the framework of more productive but more time-consuming production techniques at a point in time more distant from the present.

The comparison can turn out either way: in favor of future goods, which would lead to additional capital accumulation, or to their disadvantage, which would lead to the consumption of present goods.

Assuming that the various current goods have certain exchange relations with each other, as well as the various goods given at a certain point in the future, the outcome of this comparison generally depends on the exchange rate of any current goods in relation to any future goods that can be used a definite amount of time away from the present, which an operator considers appropriate in view of the different urgency of his needs, which can be postponed for different periods of time: if, at a given quantity and quality of current goods, the production of future goods to be expected due to an extended production period in terms of quantity and quality exceeds the exchange ratio from which an exchange of current goods for future goods appears profitable, there is non-consumption of the current goods; if it is above this, it leads to their consumption.
This exchange ratio of current to future goods removed from the present by definite amounts of time is called the interest rate (for the corresponding distance in time).

And since, according to the assumption, extended waiting times mean increased costs, i.e. current goods of a certain kind are preferred to future goods of the same kind, and they only become interchangeable if the exchange rate is a discount rate, the praxeological theorem discursively derived here from the universal action categories, which clarifies the logical function that fulfils the interest rate as an element that necessarily co-determines the course of each action, is called the time-preference theory of interest.

The interest rate expresses the extent to which an actor considers the satisfaction of short-term needs to be more important than the satisfaction of needs that require longer waiting times and, conversely, the price at which he is willing to forego possible present satisfactions in favor of future provisions.

Manifests itself in the extent and limitation of this willingness in the amount of current saving or unsaving; and any change in the interest rate is expressed as a change in the amount of saving.

If the interest rate increases, this means that the value of current goods increases compared to future goods according to the assessment of the actor - more future goods are needed to exchange a given amount of current goods for it - and in the extreme case, an increase in the interest rate to infinity, this would mean that every saving stopped at all;

conversely, if the interest rate falls, the subjective appreciation of future goods increases in comparison to current goods -- it takes less of future goods to exchange a given amount of current goods for them -- and in the admittedly impossible extreme case of a complete disappearance of the interest rate, this implies a complete disappearance of all consumption -- there would be only saving.

The extent of saving, as the expression of the interest rate becoming manifest in the present, depends on the current assessment of the value of non-existent future goods -- it is the mass of present goods that one is willing to exchange for a mass of future goods presented and valued as such.

However, whether the decisions on the non-consumption (investment) of current goods always made in the present, due to the interest rate always becoming decisive in the present, prove to be correct, i.e. whether the current goods invested in the production of future goods actually yield the expected interest in the form of future goods, which compensates for the renunciation of their non-consumption, only becomes clear in the future.

Only in the context of the fictitious construction of an ‘evenly rotating economy’, the construction of an always the same world without change, in which there are only ever the same situations for actors, in which one always strives for the same goals with always the same means and always the same success in an always the same temporal pattern, in eternal repetition, is this problem by definition eliminated, and the invested contemporary goods in the form of future goods, as it were, always automatically provide exactly the amount of interest required to induce the actors to maintain the capital stock of the assumed evenly rotating economy through corresponding reinvestment.

If it were otherwise, i.e. if the result of any productive activity did not exactly correspond to the expectations about its result at the beginning of the same, i.e. if the later result did not in any case represent the perfect compensation for previous non-consumption of current goods, then, contrary to assumption, it would not be possible to carry out the same actions again and again, and not to reinvest the capital used in the production process necessary to maintain the convertible economy, but rather change or restructuring in the provision of productive activities with capital would be the logical consequence.

In the context of the realistic construction of a changing world with learnable or erroneous actors, thus an invariably uncertain future, current investments in favour of future earnings always represent a company fraught with speculative risks:

not only is it always possible to make mistakes with regard to the technical conditions for the realization or manufacture of certain future goods in the broadest sense, but above all, it is fundamentally impossible to predict the extent to which the (goods) valuations that actually exist at a later date will correspond to the valuations anticipated at an earlier date. Only in the context of successful, genuinely entrepreneurial operations do non-consumed current goods actually yield the equivalent value of future goods, which was regarded in the decision to save as adequate compensation for lost present consumption, and, if necessary, if the value of the actually existing future goods exceeds that which had been anticipated as that whose realization should compensate for lost present consumption, actual (more or less large) profit; a loss that is also possible at any time in the event of a fundamentally uncertain future, on the other hand, if the value of future goods falls below the non-consumed present goods plus interest.

The level of the interest rate thus determines, on the one hand, the extent to which the use of current goods comes into question for the risky production of future goods and, on the other hand, it represents a logically necessary evaluation element for the cost-benefit assessment of any desired result of action. Only if the value of an action result is higher than all costs associated with the production of the same, plus the waiting costs expressed in the interest rate, can one speak of profit.

Against the background of the interest theory outlined above in imaginable scarcity, which itself is obviously not an empirical theory that is concerned with explaining the level of the interest rate, but rather a praxeological theory that unfolds the conceptual consequences that result from the fact that actions, in order to be actions, need time to reach their goal, the monetarist theory of the business cycle can now also be reconstructed, equally scarce, which, as a likewise pure theory, as explained, has to consist in the explanation of a data constellation (recognizable as realistic) that conceptually implies a subsequent boom-bust cycle.

The ‘world’, which is assumed to be unexplained in theory, looks like this:

There is a market unhindered by institutional restrictions, in which persons freely exchange goods; the exchange is mediated by money as a medium of exchange; the market expression of the exchange of current goods for future goods takes the form of (money) loans; and the corresponding intrapersonal exchange also uses the method of monetary calculation.

In this world, the phenomenon of the interest rate takes the form of an interest rate for money loans of a certain duration; it is not uniform, since the credit market as a real market is not perfect, and lenders and borrowers as real persons cannot obtain a complete market overview free of charge; however, due to the aspiration of lenders for the highest possible interest rate and borrowers for the lowest possible interest rate, there is a tendency towards a uniformization of the interest rate for loans of a given type, so that it is justified to speak briefly of the interest rate.

The interest rate for money loans of a certain type and duration should be called the market interest rate, while the interest rate that would regulate the exchange of current goods for future (non-monetary, i.e. ‘natural’) goods in a moneyless society is called the natural interest rate. Both interest rates normally correspond to each other: the function of the market interest rate is solely to give monetary expression to the natural interest rate and to enable monetary calculation (also) in the area of comparative valuation of present and future goods.

However, due to a certain characteristic of the market interest rate, there may be temporary deviations from the natural interest rate on which it is based: due to technically comparatively easy to manage shortages or expansions of the money supply (money including fiduciary media) on the credit market, the market interest rate can be manipulatively pushed above or below the level that corresponds to the natural interest rate, and at which it would have been without corresponding intervention.

While a decrease (increase) in the money supply (assuming constant cash holdings) with a constant supply of goods, apart from changes in the internal structure of relative prices of goods, must trigger a decrease (increase) in the general price level with a time lag and requires an adjustment of the market interest rate to this changed level, i.e. a reduction (increase) in the market interest rate corresponding to the lower (higher) nominal prices of current goods (with unchanged relative prices of current goods compared to future goods), a decrease (increase) in the money supply first leads to an increase (reduction) in the market interest rate when it reaches

the economic system via the credit market, i.e. to the exact opposite of what is required in order to adjust it, corresponding to the changed money supply, to an unchanged natural interest rate, and to the exact opposite of what must happen with the market interest rate in the long term (once the goods prices have fully adjusted to the changes in the money supply becoming effective via the credit market!).

The peculiarity of the market interest rate is thus that it can be brought into a temporary contrast to the natural interest rate through monetarist intervention in the credit market, and then incorrectly reflects the degree of willingness expressed in the latter to forego current consumption in favor of future goods.

In the context of this world of action, the following concrete constellation of data is now given: Due to an intervention, either by the government or by the central bank, additional amounts of money or money substitutes flow into the economic system over definite periods of time on the way via the credit market.

After that, the credit expansion comes to a stop. For the additional loan funds, borrowers can only be found with reduced nominal lending rates or with reduced requirements on the part of the lenders with regard to the entrepreneurial prospects of success on the borrower side.

In any case, credit expansion manipulates the market interest rate downward relative to a given natural interest rate, and even if it begins to rise after a certain period of expansion, it lags behind the level that would correspond to the natural interest rate until the expansion has stopped and commodity prices have adjusted to the increased money supply.

The expansion in the credit market has thus led to a situation in which more money is invested in favor of risky future returns (instead of spending it on current consumption) than would have been invested without the expansion and than corresponds to the willingness of the public (i.e. all actors), as expressed in the natural interest rate, not to consume current goods, but to invest for the purpose of the risky production of future goods.

This data constellation, which will now be shown, logically implies the occurrence of a boom-bust cycle. Before the event of credit expansion, i.e. during the state of coincidence of market and natural interest rate, the following situation exists (by definition):

The market interest rate correctly indicates the costs that a company (action) has to reimburse (at least) in the form of future returns in order to be able to consider it as sufficient compensation for lost (possible) present consumption during the waiting period and thus as successful - correctly insofar as for anyone who starts a company in the face of these costs, there is also systematically enough capital available to bridge the waiting time that this company needs in such a way that the satisfaction of all needs that arise during the waiting period and are considered unavoidable appears systematically ensured.

The market interest rate, insofar as it coincides with the natural one, regulates:

it is true that investments are only made in the production of future goods to the extent that the necessary capital has actually been provided by the public through the non-consumption of current goods;

in other words, it regulates that the total waiting time required for the achievement of all productive activities does not exceed what the public considers tolerable in view of the urgency of

their needs arising during this time and the limited capital resources they themselves have made available for such bridging purposes.

Credit expansion has fundamentally changed the situation:

Although, compared to the situation before the expansion, no more savings were made by the entirety of all actors -- the sum of the real goods provided as capital is unchanged -- the expansion of the money supply on the credit market and the relative reduction in the cost of money loans created by it have brought about a situation in which there is greater investment in the production of future goods than would have been the case without this expansion.

There is an expansion of economic activities, i.e., new or different operations and/or old operations carried out on a larger scale are initiated on a comparatively larger scale, which will only pay off in the future through future goods.

But although this expansion is real - and it is phenomenologically no different from a normal expansion - it is categorically different from the latter.

While normal expansion requires a lower natural interest rate compared to the initial situation, i.e. additional capital accumulated through additional savings, which can additionally be used to bridge the extended waiting period required by the expansion, an increase in capital has not taken place in the case of an expansion of investment activities set in motion by credit expansion. Such an expansion must therefore prove to be an overexpansion; it is a boom that must necessarily turn into a recession:

In order to produce all future goods of the productive activities started at an expanded level compared to the situation before the credit expansion, an extended waiting period is required. All goods that would have been produced in any case independently of the expansion must be produced, and also those that are additionally to be produced beyond this as a result of the expansion; since this requires both the means of production that are required to produce the first group of goods and those that are required to produce the second, and since it takes a longer period of time to save both groups of means of production by not consuming current goods than just one alone, the production of all the goods to be produced in the expanding economy requires a comparatively extended waiting period.

In fact, however, in the case of a boom generated by additional amounts of money flowing into the credit market - in contrast to normal expansion - there has been no corresponding increase in capital goods: they are just as numerous as before the expansion; thus, the boom begins with the production of a quantity of future goods, the production of which would require a longer waiting time overall than is permitted due to the real scarcity of capital goods.

To the extent that overexpansion has taken place in the boom, when credit expansion comes to an end and commodity prices and market interest rates have adjusted to the expanded volume of money, a recession must set in, which is characterized by the fact that a number of enterprises from the area of the overexpanded economy cannot be completed or continued due to a lack of capital: there is not enough capital to be able to complete all the activities started, and in the competition for the capital systematically insufficient for the booming economy, to the extent of the overexpansion that has taken place, i.e. according to the systematic overestimation of the length of the waiting period considered tolerable by the public, such enterprises must lose out as malinvestment, which, in view of their anticipable returns, see the comparatively least scope for themselves to be able to keep up with the price-increasing competition in order to be able

to keep up with the funds required for their continuation. The general overexpansion must end in unviable general malinvestment.

-- The data constellation ‘credit expansion’ implies a booming expansion of economic activities; and a boom -- i.e. an upturn without increased capital -- implies, as soon as the monetary expansion comes to an end, a recession that in general therefore goes beyond the normal case in which entrepreneurs are occasionally deceived about the future market opportunities of certain products, because it has a systematic reason for the fact that in the boom activities were initiated to an extent that systematically exceeds the capital required for their complete implementation, so that the point must be reached at which the least likely of these activities must be systematically discontinued.

The logical prerequisite for the existence of a boom is that, due to monetary measures alone, the production of future goods is taken up to a greater extent than would have been the case without this measure; and as long as the logical prerequisite for an extension of the boom is given, as long as the continuation of monetary manipulations succeeds in continuing the production of future goods at a level that is above that which would occur if such interventions ended.

In order to assess whether there is a boom, it does not matter whether these interventions result in events that affect the capital stock of the society due to certain circumstances: whether, for example, an increase (decrease) in capital is made available due to a reduction (increase) in the natural interest rate, whether the society in question is a growing economy that emits a surplus of capital goods during each production period or not, whether it comes to the physical destruction of capital due to disasters or the like, etc., etc.

The only decisive factor for the existence of a boom is that the expansion of credit leads to a stimulation of investment activities, which, whatever else may or may not be produced due to a given capital stock and given changes in the same, more produced or less produced, is in its extent above what would have been recorded without this expansion.

If this is the case, then a recession is inevitable.

If an increase in the money supply offered on the credit market would also lead directly, without any time delay, in the same breath as its own occurrence, to an increase in the general price level and to a corresponding adjustment of the market interest rate, it would be impossible to actually bring about the logical prerequisite for the existence of a boom by monetary measures;

a boom is only feasible insofar as this simultaneity does not exist, but the increased money supply falls into the hands of buyers willing to invest before goods prices and market interest rates have adapted to the increased money volume; and a boom is extendable, accordingly, only insofar as the money supply constantly flowing further into the credit market only exerts its effect on price level and market interest rate with a time lag: only because and if such a delayed effect is caused by additional money quantities thrown onto the credit market, is it possible to find additional borrowers for these additionally offered money quantities, and to manipulate a credit expansion - it is only possible because a cheapening of credit takes place without this being nullified again in its stimulating effect by a simultaneous general increase in the price of goods.

If credit expansion is brought about by the manipulation of the money supply, then, logically inevitably, a boom arises; and this must, if the expansion that keeps it going, which incidentally

cannot be endless without leading to a collapse of the entire monetary system, comes to a halt, pass into recession with the same inevitability:

Although it is in principle impossible to predict which of the activities carried out on an over-expansive scale during the boom will turn out to be malinvestments, because, apart from the general forecast that the general price level of goods will have to adapt to the increased money supply in the long term, it is impossible to predict both the pace at which this will take place and the extent to which and the temporal pattern of which goods will be affected by it, as well as to predict what relative valuations future goods will experience in a future market; but with apodictic certainty it can be predicted that, regarding a given capital stock, the overly expanded economy during a boom must get into a situation in which companies must now systematically fall by the wayside, which, in the competition for the capital systematically insufficient for the implementation of all projects, as is now becoming apparent, is proving to be the ones to which the least relative urgency is attributed by the market (systematically disregarded by credit expansion).

Investment ruins are the inevitable result of every boom; and the waste of capital visible in them in the pursuit of overambitious, unstoppable plans has, as a necessary result of every credit expansion, led to a relative impoverishment: the absolute wealth of a society, if it is a growing economy in general, may be greater than before after the end of the credit expansion, but it is less than it would have been without the adventure of credit expansion; because, as the phenomenology of the ‘recession’ makes all too clear, credit expansion has led to a systematic waste of resources in the pursuit of goals with too much waiting time:

"There are plants which cannot be utilized because the plants needed for the production of the complementary factors of production are lacking; plants the products of which cannot be sold because the consumers are more intent upon purchasing other goods which, however, are not produced in sufficient quantities; plants the construction of which cannot be continued and finished because it has become obvious that they will not pay."

"Prices drop suddenly because these distressed firms try to obtain cash by throwing inventories on the market dirt cheap. Factories are closed, the continuation of construction projects in progress is halted, workers are discharged."

The already emphasized, prominent position that the (empirical) monetarist theory of the business cycle, which has recently enjoyed rapidly increasing popularity, rightly deserves despite its epistemological untenability as an empirical theory, stems from the fact that, from a phenomenological point of view, it largely corresponds to the pure theory described above.

M. Friedman’s outline of a ‘dynamic theory’, reproduced below, clearly illustrates this correspondence.

With him, too, the sequence of events begins with a monetarist intervention -- but not with a simple expansion of money, but (since he assumes that the economic system has adapted to an existing rate of money growth in such a way that the market interest rate, anticipating these continuous increases in the supply of money, prevents itself in step with these changes) with an acceleration in the rate of expansion.

“Let the growth rate of M2 accelerate. For something like six months, the main effect will be that actual balances will exceed desired balances, which may temporarily depress short-term interest rates but will have little other effect.

“After about six to nine months, the rate of growth of nominal GNP will accelerate, as the holders of the excess cash seek to dispose of it. The increased spending, by precisely the process Hume described, will ‘excite industry’, as producers facing unexpectedly high nominal demands treat the increase as special to them and thus seek to expand output.

"For a time they can do so, because their suppliers too, including laborers, take the increase in demand as special and temporary and do not alter their anticipations.

"This, if you will, is the temporary Keynesian phase, where output responds more quickly than prices.

“In its course, prices do respond, rising more rapidly than before, and interest rates stop falling and start to rise. But it takes about eighteen months after output starts to quicken -- or two years after money accelerates -- for the main effect to have shifted from output to prices. "During this period, anticipations are changing, reflected most sensitively perhaps in interest rates, but even after prices have started to absorb the bulk of the acceleration in money, anticipations have not fully caught up. In the next year or so they will, which will force a decline in the rate of growth of output back to or below the ‘natural level’, producing the stagflation stage."

In addition to obvious similarities: expansion, reduction of the market interest rate, boom, inflation, increase of the market interest rate, bust, -- the inadequacies of this theory are also characteristically clear in the passage quoted.

The detailed information about the time lags incorporated into it is undoubtedly interesting, but it represents no more and no less than information about historical data; from a theoretical point of view, it is meaningless: non-theoretic pseudo-precisions that should not appear in the formulation of the theory itself.

More important, however, is another inadequacy - the lack of a distinction of theoretical importance: Monetary expansions can take two forms with logically clearly distinguishable consequences, but in the outline reproduced this difference is

- in our opinion quite characteristic of modern monetarism - rather obscured than illuminated and encourages an undifferentiated use of ‘monetary expansion ‘in general’ as an explanatory variable.

A boom-bust cycle is only set in motion by credit expansion; only when the increase in money supply results in additional activity for the production of future goods does it also result in a (boom-like) upswing.

The situation is quite different, however, if one assumes that the additional money supply does not directly affect the credit market and market interest rate, but is used during its circulation through the economic system exclusively for exchange with current goods produced by the given economy. In this case of ‘simple’ inflation, there is gradually a general rise in the price level, from which some goods may be affected more and more, others later and weaker; some people benefit from this process, others suffer losses as a result; the economic system can be fundamentally restructured and individual branches of production end up with more, others with less capital.

What a monetary expansion that does not affect the credit market, but does not imply, is an extension in terms of the start of the production of future goods beyond the level corresponding to the actual capital endowment (i.e. a boom); as long as additional amounts of money are used only in exchange for current goods, there can logically only be restructurings: changes in the structure of relative goods prices and corresponding reallocations of factors of production from one branch of production to another -- but production is not expanded in such a way that the production of all future goods taken as a whole now requires a longer waiting period than before the monetary expansion, because, according to assumptions, the additional money has not been used to exchange for future goods, but exclusively for current goods. Overall, the waiting time can therefore not have been extended; and so there is no boom.

This inadequacy of a lack of conceptual differentiation, however, only symptomatically indicates the underlying general inadequacy of an economy operated in empirical consciousness, which has already been repeatedly addressed in our remarks:

however much the empirical economists - here: Friedman - come close to the truth with their presentation of the phenomenology of certain event sequences, they lack the fact that they do not show why these event sequences are the necessary result of certain data constellations, and why which other events can only be classified as fundamentally unpredictable, contingent accompanying circumstances of the same; as empiricists, they remain at the level of phenomenology and strive for empirical confirmations instead of exact logical justification through exact conceptual differentiations.

However, since the concept of an empirical-causal science economy is logically contradictory and action science (ex ante) explanations can only be logical explanations, they behave in this effort to confirm or falsify experience - also Friedman behaves - like someone who first translates Pythagoras, falsifying him, into the language game of an empirical science and then tries to confirm it by empirical measurements.

VIII

After demonstrating the impossibility of an empirical-causal science economy, the presentation of the epistemological foundations of economics as a necessarily aprioristic science, and the exemplary illustration of economic theorems as propositional systems of a peculiar, non-empirical science, we now return to the logic of economics before finally discussing the practical scientific consequences resulting from the logically inevitable recognition of economics as pure science.

- A problem that is central to their full understanding and that is, without exception, misunderstood in the prevailing empirical confusion has remained almost completely unexplained:

the problem of the logic of the application of pure action theorems. After the presentation, in particular, of the last, third example of a theorem of pure economics, this is now the suitable place to discuss the aforementioned problem.

The logic of the application of statements of empirical-causal scientific disciplines (i.e. of the natural sciences) is to be characterized as follows: an if-then (the more... the more) statement becomes applicable whenever the conditions formulated in the if component are actually

present; it is completely irrelevant what the other actual circumstances of an application situation are, not mentioned in the if component: If the application conditions explicit in the if component are present, there is a use case, regardless of the fact, however similar or dissimilar the application cases may otherwise be.
- Only on the basis of this application logic can hypotheses be tested (and, as hypotheses about empirical relations, they can only be tested in and through repeated application!): if more were required for a (repeated) application than the exclusive (repeated) existence of the explicit if-conditions, if similarity of the entire 'application situation' were required (beyond the similarity of the application conditions explicitly mentioned in the hypothesis!), then neither hypothesis confirmation nor falsification would be possible, because situations could never be determined with certainty to be 'the same', and it would not be clear when an application actually exists for the first time.

In short: a hypothesis of an empirical-causal science discipline cannot in principle occur (unless it has ceased to be verifiable and falsifiable) in connection with a so-called ceteris paribus clause. Rather, it must be considered as 'blindly' applicable, as well as the conditions of its application that are explained by it. Otherwise, the empirical-causal science disciplines progressing solely through learning from confirming and, above all, falsifying experiences will collapse.

While scientific hypotheses do not actually have ceteris paribus clauses, in accordance with the scientific requirements presented, their use in connection with economic theorems is not atypical.

This could well have given cause - since the disastrous consequences that the use of this clause entails for the development of empirical causal sciences are almost obvious - to doubt the correctness of the usual scientific classification of economics.

However, the actual reaction went in the opposite direction -- firmly rooted in the empirical ideology of science, according to which it must seem unthinkable to seriously consider the idea of economics as a non-empirical discipline, instead the mentioned practice was simply interpreted as a sin against the spirit of science:
by incorporating corresponding clauses into economic theorems, one wants to immunize oneself against experience or experience tests, and in order to put economics on the right path of an advancing science, the complete abandonment of this strategy is an indispensable requirement.

This conclusion must now be classified as premature. If economics is not and must not be an empirical but a necessarily a priori science of action, then it is at least possible that the application logic of pure sciences, in contrast to that of the empirical causal sciences, results in a changed assessment with regard to the use of ceteris paribus clauses, and that the mentioned practice, which is worthy of criticism from an empirical point of view, merely indicates that economists, whatever they intend to do, do not submit to the logic of the empirical sciences at all.

In fact, from the application logic of (pure) economics results the complete harmlessness of ceteris paribus clauses; to characterize their use as an immunization strategy only testifies to the complete incomprehension of economics as a pure, a priori science of action.
- In contrast to empirical hypotheses, whether a pure action theorem is valid or not can be determined independently of its application, indeed it must even be determined independently of it, because qua praxeological theorem must be justifiable without recourse to empirical data.

This does not make the application of such theorems impossible, but it does change the situation in that blind application in the sense mentioned above (which became necessary because otherwise the validity of empirical hypotheses cannot be determined at all) is then ruled out: apart from establishing the actual existence of the conditions specified in the if-component of the (pure) theorem, it must also be established for an application that, based on the remaining conceptual definitions of the situation in which the theorem is to be applied, there is in fact no situation that is logically distinct from the data constellation specified in the theorem.

Such a situation would be characterized by the fact that, due to additional conceptual provisions of this situation of possible application, there is actually a data constellation, from which it can be shown, again solely on the basis of pure logical analysis, that it does not logically imply the phenomenon to be explained, and that it is thus logically different from the situation in which the theorem alone can be applied.

Since, in this case, what constitutes a logically distinct situation can (and must) be determined a priori — and indirectly, therefore, which changes with regard to the application situation do not constitute a logically different situation, i.e., within which limited framework a theorem can be applied blindly — it would be conceivable in principle to represent, with the development of each praxeological theorem, all those conceivable conceptual determinations whose addition to the conditions specified in the theorem itself would constitute a logically distinct case.

However, practical reasons make such completeness difficult, if not impossible; and didactic reasons do not even make it desirable in every case. Therefore, economic theorems often occur in conjunction with a ceteris paribus clause: they explain a ‘pure’ case, unfold its implications, under certain circumstances state some ‘realistic’ provisions, the addition of which to the conditions of the pure case constituted a logically different case, and then assert the validity of the theorem ceteris paribus - it applies and can be applied if an application situation, whatever its other provisions may be, is logically no different from the validity conditions stated in the theorem itself.

- This practice is harmless because, in principle, even in the case of incomplete explication of application conditions, it can always be decided, before the factual application of a theorem, solely on the basis of logical analysis, whether a corresponding situation exists or not; and it is also harmless insofar as, even in the case of an initially erroneous application, the discovery of this error, i.e. the rejection of an application as incorrect application, cannot be interpreted as immunization, since the justification for such a rejection must be justified at any time before the instance of pure logical analysis.

One simply cannot immunize praxeological theorems from experience, because they claim to have an experience-independent validity as logical theorems: whether they apply, and also whether there is a case of possible application, is decided in principle not by experience, but by logical analysis.
A blind application of economic theorems and the term ‘immunization strategy’ to a rejection of such blind applications made with reference to the ceteris paribus clause are absolutely inappropriate: Scientific nonsense is carried out by those who think they are testing the economic theory presented above, for example, by blindly applying it in a situation in which there is monetary expansion, but at the same time tax increases impose additional costs on companies that compensate for the initially made cheapening - and not by those who criticize and refer to such a practice, that even without explicit mention of this data constellation, it should have been clear that, due to tax increases, the occurrence of credit expansion, which could normally be set

in motion by money expansions, could apparently have just been prevented, whereby a situation would actually exist that is logically clearly different from the application situation of the theorem in question; and nonsense drives the one, to mention another example, who believed, for example, that setting minimum wages above the level that would form on a labor market unhindered by interventions leads to institutional unemployment' in a situation in which an increase in the minimum wage is accompanied by an economic expansion that entails an increase in the marginal productivity of work above the fixed minimum wage level - and certainly not the critic who, in the face of such a practice, finds that one apparently literally does not know what one is doing.

IX

The fact that the recognition of the a priori character of economics and the corresponding understanding of its logic, including the logic just considered of the application of pure theorems of action science, results in consequences for the current practice of economics that are hardly to be overestimated in their extent, has probably already become indirectly clear from the statements made so far.

If you try to make them explicit here, the most obvious consequence is probably the - as it were negative - consequence that arises for the practice of empirical economic research.

- According to its self-image, it provides the empirical foundation for theory and ensures that speculation is transformed into validated theory.

For this supposed achievement, it receives financial support year after year from those who believe they need such knowledge for a variety of reasons and who have sufficient funds of their own and from others, to an extent that makes the other social sciences green with envy.

With the insight into the untenability of this image of empirical economic research, such a willingness to support should also cease.

If it is said that at best - namely, if it applies a correct praxeological theory logically correctly in empirical illustrations of established truths independent of experience - and that otherwise it cannot even achieve this, but only theoretically (praxeologically) reconstructs and presents more or less meaningless data in a more or less dramatically bridled manner, which in truth are nothing but (economic) historical data: that its achievements, however, in any case have nothing to do with theory development and testing, and nothing to do with the generation of prognostically usable knowledge, but that all this, completely independent of any form of empirical economic research, takes place within the framework of a priori practical scientific investigations - then it can be expected that the willingness to support this type of research financially decreases, presumably to the normal extent, which also characterizes the willingness to support with regard to all other (non-economic) historical investigations.

Instead, if things were done properly, the type of economic research that deserves greater attention, which from an empirical perspective is nothing more than mere, pale theory, but which in reality is the only one that produces theory that can actually be used for forecasting: praxeological research.

Only armchair economics, which is undoubtedly much cheaper than what is known as empirical economic research, can bring about progress in the development of economic science; empirical economic research is irrelevant in this regard, no matter how much those who earn their living from it resist this view.

Practical consequences no less dramatic than those for the role of empirical economic researchers, however, naturally also reverse for the role of those who intend to pursue economic theory. He no longer submits to the verdict of experience tests with his work, but is instead fundamentally subject to control through logical analysis.

On the part of the supporters of the conception of empirical-causal science economics, it has been emphasized with regard to this type of control that it cannot guarantee more than the tautological correctness of conceptual transformations and derivations, whereby this note was regularly sold as if it designated a control procedure that was inferior in some sense.
While the first hint regarding an a priori science of action is undoubtedly correct, the assumption associated with it is just as undoubtedly absurd: mathematics and logic also produce nothing but tautologies in this sense, and yet no one would probably want to claim that the validity controls to which the products of these disciplines are subject are in any meaningful sense less strict than they are empirical controls.
Rather, the opposite is apparently the case; and even an a priori economy, as the exemplary presentation of pure economic theorems above has already been intended to make clear, actually does not subject itself to lesser, but considerably stricter requirements with regard to its statements.
For the practical work of the theorist, the consequences of this aggravation are quite drastic:
With regard to his work, the empirical economic theorist offers two related immunization strategies that can be derived from the logic of empirical causal sciences, which are also widely used in scientific practice.
From the institutional separability of the process of hypothesis formulation and that of the empirical test, it follows, on the one hand, that without this being reproachable - one can first of all claim almost everything without having to immediately prove his assertion; rather, its provision can be postponed to the later time of a test (which may never be carried out). It is almost superfluous to note that this option is also used.

- On the other hand, it follows from the same (separation) reason that discussions between empirical scientists - again, without it being possible to reproach the participants strictly speaking as unscientific - can take the form of speeches and research accompanied by ludicrous references:

every hypothesis, however well confirmed, can easily be provided with the critical indication that this and that, this and that variable, have not yet been sufficiently taken into account or controlled, and that the hypothesis must therefore, depending on the goodwill of the critic, continue to be viewed with more or less great skepticism; and conversely, even if a single hypothesis should not withstand an experience test, the ‘central idea’ of its hypothesis (or what one spends on it), the ‘center’ of its research program, can in principle always be saved by suitable ad hoc assumptions until the next test, and thus, however high the waves of criticism beat, can be continued with unbroken self-confidence on familiar paths.
In the context of economics, this possibility has also taken on a real form as a division into schools, some of which take insufficient notice of each other, but the less they try to refute each other in direct confrontation, the easier it is to engage in blanket labelling and defamation.

With the inevitable recognition of the a priori character of economics, with the discovery of empirical consciousness as demonstrably false consciousness, participation in such a practice, which is obviously questionable (and nevertheless derivable from the logic of empirical causal sciences as to its possibility), becomes reproachable.

It becomes unscientific in the strict sense, because other, stricter rules now apply - the methodological basic norm of the logic of a priori action science is the rule of the ‘hic Rhodes, hic salta’: for every claim, in principle, it must be possible to take the evidence immediately, or it must be abandoned; likewise, the proof of the inaccuracy of a certain theorem must be able to be provided on the spot, and it is inadmissible to dismiss any theorem by means of incidental hints if it cannot be strictly refuted logically; and finally, no evasive maneuvers are any longer possible as answers to criticisms, except for the proof of the logical inaccuracy of such a criticism to be provided on the spot.

It is doubtful whether all those who practice or intend to practice economic theory can and/or want to submit to such stricter requirements.

In any case, however, it can be said with certainty that hypotheses which, misunderstood as empirical-causal scientific hypotheses, are still regularly presented today as honorable, empirically verified or verifiable assertions, must then be laid to rest once and for all when the actual rules of aprioristic sciences are applied to them:
Cost-push inflation theories, the (trade union) purchasing power argument, Keynesian refutations of Say’s law, minimum wage theories, the theory of the Ricardo effect, non-monetarist business cycle theories -- these are just a few keywords for theorems that logically do not achieve what the corresponding proponents regularly expect from them.
The decisive practical consequence for the role of the economic theorist, which results from the recognition of the a priori character of economics, is the drastically increased compulsion to logical argumentation, discursive reasoning and deductive derivation.
- In complete reversal of what G. Schmölders demands for economics, we no longer need "diligence and manliness", but in fact more "ingenuity and logical subtlety".
But we do not need this increased ingenuity to bring economics closer to scientific ideals, but to finally treat economic theorems explicitly as what they must be logically, if they are ‘theory’ at all: statements of an a priori science of action.

Bibliography

Notes to Chapter 1

[1] See, for example, Ezekiel/Fox, Methods of Correlation and Regression Analysis, New York 1966; Rao/Miller, Applied Econometrics, Belmont 1971; Pindyck/Rubinfeld, Econometric Models and Economic Forecasts, New York 1976.
[2] See, for example, L. Robbins, The Nature and Significance of Economic Science, London 1935; L. v. Mises, Human Action. A Treatise on Economics, Chicago 1966.
-- The criticism of logical (opposite: mathematical) economists deserves special emphasis because it makes it clear that it is by no means - as econometricians regularly claim - about the alternative 'mathematical vs. literary economics'.
[3] Cf. H. Blalock, Causal inferences in non-experimental research, Chapel Hill 1964; similar, Theory Construction, Englewood Cliffs 1969; ds. (ed.), Causal Models in the Social Sciences, Chicago 1971; Namboodiri/Carter/Blalock, Applied Multivariate Analysis and Experimental Design, New York 1975; O.D. Duncan, Path-analysis: sociological examples, in: Blalock (ed.) 1971; ders., Introduction to Structural Equation Models, New York 1975; Goldberger/Duncan, (eds.) Structural Equation Models in the Social Sciences, New York 1973; also cf. D. Heise, Causal Analysis, New York 1975.
[4] It follows from this that the principle of constancy can by no means be understood as a merely contingent methodological principle: this would only be possible if one simultaneously claimed that human beings can manage exclusively by registering experiences in the above sense. See also F. Kambartel, Erfahrung und Struktur, Frankfurt/M. 1968, chap. 3, esp. P. 91 ff.; also: H.H. Hoppe, Handeln und Erkennen, Bern 1976, p. 85 ff., as well as chap. 4.
[5] On the explication of human actions qua intentional actions cf. L. v. Mises, Human Action, Chicago 1966, Chap. 1.
[6] In economic theory, the condition with regard to human action, which is implied by the assumption of the principle of constancy, is referred to as the 'evenly rotating economy' and is described, for example (further illustrating our explanations above), as follows:
"The evenly rotating economy is a fictitious system in which the market prices for all goods and services coincide with the final prices. There are in its frame no price changes whatever; there is perfect price stability. The same market transactions are repeated again and again. The goods of higher orders pass in the same quantities through the same stages of processing until ultimately the produced consumers' goods come into the hands of the consumers and are consumed. No changes in the market data occur... The essence of this imaginary construction is the elimination of the lapse of time and of the perpetual change in the market phenomena. The notion of any change with regard to supply and demand is incompatible with this construction. Only such changes as do not affect the configuration of the price-determining factors can be considered in its frame. It is not necessary to people the imaginary world of the evenly rotating economy with immortal, non-aging and non-proliferating men. We are free to assume that infants are born grow old, and finally die, provided that total population figures and the number of people in every age group remain equal. Then the demand for commodities whose consumption is limited to certain age groups does not alter, although the individuals from whom it originates are not the same." v. Mises, op. cit., p. 247.
[7] See also the following observations in this regard, as in the previous footnote:
"The imaginary construction of an evenly rotating system is a limiting notion. In its frame there is in fact no longer any action. Automatic reaction is substituted for the conscious striving of thinking man after the removal of uneasiness… Action is to make choices and to cope with an uncertain future. But in the evenly rotating economy there is no choice and the future is [p. 86] not uncertain as it does not differ from the present known state. Such a rigid system is not

peopled with living men making choices and liable to error; it is a world of soulless unthinking automatons; it is not a human society, it is an ant hill".
v. Mises, op. cit., p. 249 ... P.248
[8] Cf. on this argument as well as on the following remarks by K. R. Popper, Das Elend des Historizismus, Tübingen 1971, esp. P. XI-XII;
see also F.A. v. Hayek, The Theory of Complex Phenomena;, Rules, Perception and Intelligibility, both in: ders., Studies in Philosophy, Politics and Economics, New York 1969.
[9] Of course, one can no longer argue about the fact that one can argue with one another: a solipsism is indefensible, because if one were to believe that one could defend it argumentatively, one would have already thrown it overboard.
Cf. on this anti-solipsism argument e.g. K,R. Popper, Language and the Body-Mind Problem, in: ders., Conjectures and Refutations, London 1969.
'This, I think, solves the so called problem of 'other minds'. If we talk to other people, and especially if we argue with them, then we assume (sometimes mistakenly) that they also argue: that they speak intentionally about things, seriously wishing to solve a problem, and not merely behaving as if they were doing so. It has often been seen that language is a social affair and that solipsism, and doubts about the existence of other minds, become self-contradictory if formulated in a language. We can put this now more clearly. In arguing with other people (something we have learned from other people), for example about other minds, we cannot but attribute to them intentions, and this means, mental states. We do not argue with a thermometer."
Ibid., p. 297;
see also: ders., Epistemology without a Knowing Subject; ders., On the Theory of the Objective Mind; both in: ders., Objective Knowledge, Oxford 1972 (cf. in particular P. 119 ff and p. 2 3 5 ff.).
Cf. on a similar language-theoretical overcoming of solipsism also K.O. Apel, Transformation of Philosophy Vol. 11, Frankfurt/M. 1973 (especially the 7 treatises of the second part);
also J. Habermas, Legitimationsprobleme im Spätkapitalismus, Frankfurt/M. 1973 (footnote 160, pp. 152-3).
[10] Popper (Das Elend des Historizismus, Tübingen 1971, p. XII) states in this context:
It is
"not possible for any scientific forecaster - whether human or calculator - to predict his own future results with scientific methods. An attempt to do this can only achieve its goal post festum: the prognosis comes too late. If he achieves his goal, the prognosis has turned into a retrognosis. "
[11] It can therefore be stated (in Kantian language) that necessity requires' (i.e. natural constancy) freedom '(i.e. a learning mind uncaused with regard to its activities) as a condition of its possibility.
[12] It is perhaps interesting to note that the same illusion would have to arise again for God, if one were to assume that He also learned.
In the same way, it would also have to arise if one were to attribute omnipotence to it: namely, omnipotence obviously also included the ability to learn, for example, to enact certain laws in the execution of learning processes and to put others in their place.
Of course, God, if he is omnipotent, cannot at the same time be omniscient (at any rate, our human mind cannot think this without contradiction!): because if he is omniscient, i.e. if he always already knows what he will ever know, he cannot, of course, as Almighty, decide at any time to suspend the validity of this knowledge by creating just completely new facts.
[13] Sentences (1) and (2) are also implicitly assumed to be correct in the assertion that has just been classified as logical nonsense: one claims that one can learn from experience, and one also admits that one cannot know with what knowledge (i.e. with what concrete prediction equation) one may one day retire.

[14] It goes without saying that the logical nonsense of a causal interpretation of such relationships naturally also eliminates the possibility of attributing a comparative more' or less' explanatory power to the individual so-called cause variables. (This usually happens when variables are standardized and one then deals with betas (path coefficients) that are comparable in terms of their size from a statistical point of view, instead of incomparable b's (regression coefficients).) Where there is no cause, there can logically be no more or less significant causes.
[15] [p. 87] Do you perceive a situation that is objectively identical and appears to be an identical net skin impression differently, and is the starting point of the action therefore a logically different one? And/or do you use other means to try to get from a given initial situation to a preferred target state? And/or are you trying to achieve other action goals?
[16] Two questions that go beyond the scope of this paper, however, probably arise at this point (they are discussed in detail in the two following papers:
(1) If the search for 'causes' in the field of human cognition and action is logically nonsensical and there are only facts that are contingently associated (covariated) with certain acts of cognition and/or action, does it not necessarily follow that the (social) scientist thus lacks any possibility of highlighting certain facts other than in any objectifiable sense 'more relevant' antecedent events of certain actions or states of knowledge?
If no event can be singled out as a 'cause' from the infinite number of antecedent events, are not, in principle, all these innumerable events (qua 'coincidentally' associated with the variable requiring explanation) equal?
The answer to this question presented here without detailed justification - is: no! If something cannot be highlighted as a cause of something, it does not logically follow that there are no facts that could not be highlighted in the same context as something else (which is not a cause!). For example, 'reasons' or 'motives' for a particular action or insight can be highlighted from the infinite number of antecedent events:
Reasons or motives are events that the 'originator' of these phenomena themselves indicate as decisive for their occurrence, or would indicate when asked, and which, unlike 'causes', are not intended to explain an entire class of phenomena ex ante, but 'only' (ex post) a spatially and temporally determined, individual event of this class.
(Two phenomena of the same class that can be specified in terms of space and time can thus have two different causes, whereas according to the principle of constancy, their cause must be of the same type, i.e., the same type, since the phenomenon to be explained is also the same, i.e., belongs to the same class.)
The fact that the study of reasons can reveal 'objectifiable results' and yet has nothing to do with 'causes' is probably shown most vividly by investigations into the history of science (see, for example, G. Holton, Thematic Origins of Scientific Thought, Cambridge 1975): for example, a lot can be said about the origins and motives of relativity theory (this is Holton's main topic), but anyone who wanted to claim that one (including Einstein himself) could have predicted relativity theory on the basis of knowledge of causes!
(2) If actions and cognitive achievements cannot be predicted on the basis of causes, does this mean that predictions in the field of human action and cognition are impossible at all?
Again, the answer - again only outlined - is 'no': it simply means that there are no predictions based on empirical laws that can be falsified by contingent experience. However, there may well be non-falsifiable predictions based on pure mental work through contingent experience. - Such (logical) forecasts (and not, according to a self-misunderstanding of many economists: empirical forecasts) include the predictions derivable from the theorems of pure economic theory, such as the forecast derived from the modern version of quantity theory, that an increase in the money supply (money including fiduciary media such as circulation loans) due to a government decision, for example, with a constant quantity of goods and a constant demand for money hoarded as a cash reserve (i.e. not acting as an exchange medium), must (logically) necessarily lead to a loss of purchasing power of money (inflation). (See in detail, L. v. Mises,

Human Action. A Treatise on Economics, Chicago 1966, esp. P. 408 ff.; as well as, The Theory of Money and Credit, New York 1971.) - If there are predictions in the area of human action and cognition, they must always be predictions that are logically correct, as in the example given, provided that the assumptions they are based on are factual. Economics is the science that examines the logical consequences of certain presupposed actions or decisions with regard to 'goods' in assumed situational contexts and, if necessary, compares them with [p. 88] the subjectively intended goals.

(On the development of economics as an a priori science and on the criticism of the frequent self-misunderstanding of economics as an empirical science, see in particular L. v. Mises, Human Action, Chicago 1966; and L. Robbins, The Nature and Significance of Economic Science, London 1935.)