The Practice of Sociology and the Abandonment of Content as a Regulative Idea*

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Die Forschungspraxis der Soziologie und das Fallenlassen von ‚Informationsgehalt‘ als regulative Idee*


Abstract: The most important advances of critical-rationalistic methodology over classical justificationist methodology are the discovery of content as a regulative idea and the discernment of immunization strategies that obstruct criticism by abandoning content as a regulative idea. An attempt is made to list a number of these strategies and to indicate their usage of explicit avoidance in specific cases of empirical social research from the sociological literature.

1. Background and Problem

One can scarcely maintain that the practice of sociology as an empirical science could not be open to improvement. The inquiry into possibilities for improvement leads to questions concerning the method practised, and to the question as to whether there is a more fruitful alternative. In recent years ALBERT has worked out very clearly the difference between the classical pre-Popperian methodology and the modern Popperian critical-rationalistic methodology. In his analysis the preferability of the latter emerged (ALBERT 1968: 8–54). As the classical methodology in empirical sociology, or the tradition of empirical social research, does not appear to be without adherents, an attempt will be made here to contribute to the diffusion of the critical-rationalistic methodology.

In classical methodology, truth and certainty are regarded as the two goals of science, i.e. as the regulative ideas which control the operation of a system of scientific statements. It is then taken for granted that the quest for certainty is compatible with the search for truth. On closer inspection however, it appears that this compatibility is only realisable if one limits oneself to drafting statements lacking in content and which therefore furnish no information about reality. The only obtainable certain truths do not say anything about reality, are not open to criticism, and would not be worth the effort of criticising in any case. Under this limitation, certainty is bought at too high a price. It becomes desirable to develop a new programme for science, one in which the desire to say something about reality is introduced in the form of a regulative idea. In POPPER’s critical-rationalistic programme of science, content is explicitly introduced, truth retained, and certainty dropped as a regulative idea.

A scientific researcher who behaves in accordance with the classical principle of sufficient reason, the principle of positive justification, searches for ultimate grounds, works with „authoritative sources of knowledge“, and only makes „founded“ statements which he can „justify“ or „legitimate“. The only way he can consistently apply this principle is by employing practices that render criticism difficult if not impossible. Such practices imply that content is abandoned as a regulative idea. A researcher who acts in accordance with the principle of the critical-rationalistic method, the principle of subjection to critical tests, will draft and opt for statements open to criticism. He will also employ such practices that increase, rather than obstruct, the opportunities for criticism.

One of the most important tasks in furthering the practice of sociology, is to list and describe practices which obstruct criticism and to indicate their usage in sociological literature. The effectiveness of carrying out this task will be enhanced by focusing on cases which bear closely

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upon the problems central to contemporary empirical sociology. It is in this sense regrettable that POPPER focused on „conventionalist strategies“ in physics, psycho-analysis and the politically orientated strands of Marxism, TOPITSCH on „empty formulae that say neither anything specific nor new about reality“ in traditional metaphysics and social philosophy, and ALBERT on „immunisation-strategies“ in neo-classical economics and theology. For it may be presumed that the analyses of such cases will be misconstrued by empirical sociologists, or else be met with a shrug of the shoulder. It will also be effective to analyse cases in sociological literature where practices harmful to criticism are not employed. An attempt will be made to list such practices, and to point out instances of their usage or explicit avoidance. It should be noted that this list of „immunisation-strategies“ makes no claim to be anything like a complete typology of practices obstructing criticism.

2. The Escape into Tautologies

The situation in which a piece of research leads to the refutation of previously accepted statements without the availability of alternative explanations, is one which is unacceptable to those who adhere to the empiricist version of the classical justificationist methodology (ALBERT 1968: 21–28). In the literature of the tradition of empirical social research, immunisation practices are especially to be expected in the closing argumentation of research publications that involve results unfavourable to the initial central hypotheses. One of the ways in which attempts are made to avoid this situation of „being left empty-handed“ is the escape into tautologies which we will now try to point out in a well-known book by BENDIX and LIPSET, entitled Social Mobility in Industrial Society (1959).

The problem posed in this book was one of the most urgent ones within the Marxist explanatory programme. According to Marxist theory, it was impossible that in capitalistic countries (countries in which the economy is controlled by the market-mechanism) there should not emerge socialist parties (political parties which endeavour to replace the market-mechanism by an organisation-mechanism). Yet in the United States of America, the most capitalistic country of all, no socialist party of any significance existed, as was the case in many less capitalistic countries. Whithin the framework of the Marxist explanatory programme, attempts were made to accommodate this inconsistency by means of an auxiliary piece of theory which is usually connected with the name of SOMBART, and his book published in 1906, entitled Warum gibt es in den Vereinigten Staaten keinen Sozialismus? This piece of theory holds that socially mobile people, unlike those who are socially stationary, have no interest in socialist parties, and that the social mobility in the United States was far greater than in the European countries. The assumption about the differences in social mobility, however, was not empirically tested, but was simply illustrated by conforming cases (such as, the American millionaire who started out as a new-paper boy – SOMBART 1906: 135). BENDIX and LIPSET were the first to test the fundamental assumption and reported their findings in their 1959 publication.

The findings contradict SOMBART’s hypothesis:

„. . . the overall pattern of social mobility appears to be much the same in the industrial societies of various western countries“ (BENDIX and LIPSET 1959: 13).

The authors may leave the reader to theoretically account for this refutation; then again, they may attempt to do so themselves. BENDIX and LIPSET do not propose a new hypothesis concerning the conditions of the emergence of socialist political parties. However, the last sentence of the book notes that it needs be recognized that „a high rate of social and labor mobility is a concomitant of industrialization regardless of political conditions“. The authors appear to regard this statement as the most important theoretical modification to issue from their research. This new statement we find formulated as follows only a few pages earlier on in the book:

„Our findings support the thesis that social mobility is an integral and continuing aspect of the processes of urbanization, industrialization and bureaucratization“ (BENDIX and LIPSET 1959: 280).

Let us examine this statement closely. At the outset it is clear that the statement no longer relates to SOMBART’s problem. But furthermore, the content of the statement is peculiar. For variables such as „social mobility“ and „degree of industrialization“ (urbanization, bureaucrati-
zation) are usually defined in relation to each other, rather than independently. Thus an increase in a country’s degree of industrialization logically implies a certain degree of social mobility (more people moving from primary to secondary industry, and more moving into the tertiary sector). If we take a social mobility matrix as our point of departure (see figure 1), we see that the marginal frequencies expressed in percentages indicate a country’s degree of industrialization at two different points in time.

<table>
<thead>
<tr>
<th>point in time</th>
<th>t₁</th>
<th>t₂</th>
<th>a+b+c+d</th>
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<tr>
<td>social position</td>
<td>p₁</td>
<td>p₂</td>
<td>a+b+c+d</td>
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<td>t₁</td>
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<td>p₁</td>
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<td>a+b+c+d</td>
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If $\frac{a+c}{a+b+c+d} > \frac{a+b}{a+b+c+d}$, then $c > b$ and thus $c \neq 0$.

A change in these percentages (an increase in the degree of industrialization) is logically possible only when the cell-entries for socially mobile people (righthand-upper, and lefthand-lower corner) are not equal to zero. It thus appears that the statement is analytic: it is a statement in which one predicate is incorporated in another and therefore certainly true, but lacking in content. Every logically possible finding would support the thesis: none is possible that would contradict it.

**Digression:** Where tautologizing is attempted, concepts with a very dubious function may start to appear in the argument. We shall look into this here.

In the United States, various projects are being carried out in the sociology of science. Thus, a group of researchers round LAZARSFELD looked at the history of empirical social research. In a recently published collection of essays, COLE, one of the group’s members, states:

"Thus far we have seen how there was a great deal of empirical research done in the nineteenth century. Some of this work, particularly that of the mid-century criminologists, was relatively sophisticated. Yet this tradition never lead to the development of an empirical sociology – it was a dead end. The question we must answer is why did continuity fail? It is my hypothesis that continuity is difficult to maintain when science is conducted in a non-institutionalized setting. It is the social organization of science that provides the mechanism for intellectual continuity" (COLE 1972: 108).

After listing nine indicators of institutionalization and pointing out that nineteenth century England rates negatively on those indicators, COLE proposes:

"Finally, perhaps one of the most important indicators of institutionalization is continuity itself. . . . I am aware of the logical flaw in including continuity as an indicator of institutionalization if one wants to show that continuity can not exist without institutionalization. This is, of course, assuming what we should like to prove and is thus tautological. However, the degree of correlation among the indicators of institutionalization is an interesting question. Any one of the indicators might be looked upon as a dependent variable influenced by the other indicators" (COLE 1972: 119).

COLE thus acknowledges the point made here: if the dependent variable is incorporated in the independent variable, the statement is analytic and thus tautological. COLE attempts to avoid the methodological mistake by inquiring into the correlation between the individual indicators, whereby he

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1 The authors themselves provide neither a definition nor an empirical indicator of „degree of industrialization“. One therefore has to view the hypothesis against the background of the tradition of the discipline.
retains "continuity" as an indicator of institutionalization. However, one may well ask what exactly has become the function of the concept of institutionalization: could not the inquiry into the correlation between the indicators be set up simply as an inquiry into the correlation among a number of variables on which the label "institutionalization" has not been placed?

LAZARSFELD, in his preface to the collection of essays, leaves no doubt concerning the extremely important function of the concept of institutionalization. He states:

"Reviewing the whole enterprise raises a curious question. How is it possible that all these sketches can be written without an explicit definition of 'empirical social research'? At this point we can take recourse to another idea which has recently gained prominence. . . . Kuhn speaks of the emergence of a new paradigm, but subsequent writers have stressed that he uses the term in a variety of ways. I find especially useful the parts of his writings that Masterman calls the 'sociological notion of a paradigm' -- something which can function when the theory is not there'. . . . Substitute in all this for the 'lack of theory' the lack of a well-circumscribed field and the lack of a well-conceptualized set of problems, and we shall find ourselves at home. How is this new paradigm to be characterised in our case?" (OBERSCHAL: xii).

LAZARSFELD states that the employment of the programme, or the "paradigm" resulted in a number of empirical generalizations, and he continues:

"But as such information accumulates, the need for a more stringent intellectual approach becomes even more pressing. And indeed it was found in the notion of 'institutionalisation' which the papers in this volume. . . . bring out so strongly. It is a timely new phase in the collective enterprise. . . . As can be seen from this volume, the idea of 'institutionalisation' has become the conceptual core of the whole program . . . ." (OBERSCHAL: xiii).

LAZARSFELD's remark that the concept of institutionalisation has a paradigmatic function in the sense of KUHN'S Structure of Scientific Revolutions (1962) is important in trying to grasp the concept's function. ALBERT argued that KUHN's historical-sociological researches into the actual functioning of the sciences in recent centuries has demonstrated the frequent use of "der Methode der Exhaustion", the method of exhausting the informative content of statements. ALBERT further argued that the practice of what KUHN termed "normal science" in fact boils down to the application of this method (ALBERT 1968: 31–33, 51).

A few remarks concerning this "Exhaustionsmethode", which was described and highly valued by the German epistemologist DINGLER.

The method advocated by DINGLER needs to be viewed explicitly within the framework of classical methodology which looks upon truth and certainty as the regulative ideas; the method of exhausting is then a way of arriving at certain truth. DINGLER exemplifies his method using MENDEL'S biological laws. If we have two parents who are similar in all characteristics save one, and we designate them with mm and m'm', then an ensuing generation will be as follows; mm: 25%, mm': 25%, m'm: 25% and m'm': 25%. DINGLER continues (our translation):

"If we then call a pair of parental characteristics, which behaves in this way 'independent', we have the proposition: independent pairs of parental characteristics behave according to the afore-mentioned Mendelian law. This proposition is of course a tautology but, as we have seen, only these have apodictic certainty" (DINGLER 1923: 322).

And furthermore:

"Now we no longer turn to nature to 'experimentally prove' Mendel's law, but we examine the single case and depending on whether it behaves according to the law, we call the specific pair of characteristics independent or not" (DINGLER 1923: 322).

The concept of "independent characteristics" clearly functions paradigmatically here: research programmes can be designed to check whether or not particular characteristics of certain animals or plants are independent ones. In the course of such research however, MENDEL'S laws are not once tested.

Take the following hypothesis: "For all characteristics and all living beings it can be said that, if two parents are similar in all respects save one, and the one parent is designated mm while the other m'm', then an ensuing second generation would have a frequency distribution with respect to the possible characteristic combinations of 25%–25%–25%–25%". Any potential refutation of that hypothesis can be compensated for by introducing the concept of "independent characteristics", while the frequency distribution is also used to determine whether a particular characteristic is independent or not. DINGLER opts for tautologies. From the tacitly
accepted hypothesis, which has been formulated as generally as possible, DINGLER draws information concerning one particular species and one particular characteristic. Should the supplied information appear to be untrue, he acts as if the information was not derived from the statement. The introduction of the concept of „independent characteristics“ resulted in the attenuation of the refutation of a hypothesis with a particularly high empirical content, and in the maintenance of the illusion of being in possession of certain truths. Had neither refutation been valued negatively nor the concept of „independent characteristics“ been introduced, exactly the same piece of research could yet have been carried out. After all, one can design a research programme with the aim of finding out whether a statement supplies true information for all species and all characteristics.

We can now look at the function played by the concept of institutionalization in COLE’s argument. This function appears to be limited in as much as the desired analysis of a correlation-matrix could well be carried out without the introduction of the concept of institutionalization. Instead, the concept’s function is to enable zero-correlations (i.e. refutations of hypotheses) to be disposed of easily, just as DINGLER was able to dismiss false predictions by claiming that not independent but dependent variables were involved: for when one carries out an item-analysis of indicators for a particular concept, it is accepted procedure to remove an item which correlates badly with the others and to consider the item concerned as simply an inadequate indicator of the particular concept. COLE assumes that both a common journal and a scientific society are ways of institutionalizing the scientists’ interactions (COLE 1972: 117-8). But should it appear that a common journal, unlike a scientific society, does not lead to a greater co-ordination of scientific activity, then he can easily claim that the hypothesis „if institutionalization, then continuity“, has not been refuted, but that a journal is an inadequate indicator of institutionalization. Should he adopt this course of action, he would be applying the method of exhausting. In general: one needs to keep in mind that the escape into tautologies can be accompanied by the introduction of concepts that fulfil a dubious function.

3. Ad hoc Introduction of Auxiliary Hypotheses

A different immunisation-strategy is the practice of introducing ad hoc auxiliary hypotheses. This strategy accommodates refutational findings of a theory’s principal hypotheses in the following way. The auxiliary hypotheses are introduced in order to explain away the empirical findings. These auxiliary hypotheses themselves, however, are not subjected to that scrutiny which normally befalls newly introduced hypotheses. Such scrutiny of hypotheses concerns their empirical content, the degree to which they have survived empirical testing and the desirability of empirical research into their truthfulness. The introduction of auxiliary hypotheses creates several possibilities for immunisation.

First, the empirical content of the auxiliary hypotheses may be zero, or indeterminate, as a result of which the whole theory becomes immunised. This is a case of objectively rendering criticism impossible.

Second, the empirical content (of the auxiliary hypotheses) though positive may be such as to allow only of an alternative explanation for the refutational results. One could, however, have opted for a different theoretical modification which offers more scope for criticism by posing an alternative explanation for the confirming results as well. This is a case of objectively rendering criticism difficult.

Third, the author may construct his argument in such a way that while there could be no objections to the content of the auxiliary hypotheses as such, the modified theory is presented as having the same empirical content as the original theory. This down-grading of the theoretical modification reduces attention to the question of the desirability of further research into the truth of the modified theory: if two theories have the same empirical content and one has already been tested, then there is less urgency about subjecting the other to any tests. This is a case of pragmatically rendering criticism difficult, as the statements of the modified theory are quite acceptable in themselves. Other researchers will simply be less inclined to carry out tests because of their apparent superfluousness. The way to avoid immunization
and, instead, allow for a theoretically progressive solution of the problem, is to maximize the content of the modified theory and to recognize the desirability of empirically testing that theory.

We shall now try to indicate the use of the first two of the above-mentioned varieties of this strategy in a section of DAHRENDORF’S *Class and Class Conflict in Industrial Society* (1959). This part of DAHRENDORF’S theory was empirically tested by LOPREATO, whose findings will be referred to here.

DAHRENDORF aims to develop a theoretical alternative to existent theories that offer an explanation as to why some members of a society have interests which are opposed to those of other members. First he holds that two social classes need be distinguished within a society which he conceives of as an „imperatively coordinated association“; a Command Class, whose members participate in authority, and an Obey Class, whose members are subjected to the exercise of authority. Second he holds that the participation in, or subjection to, authority explains the phenomenon of the opposition of interests. On the one hand DAHRENDORF opposes the WARNERian stratification theories, according to which several classes need to be distinguished in order to explain the opposition of interests. On the other hand, he stands opposed to Marxist theories which see the source of explanation to lie in the possession of, or exclusion from, the means of production. DAHRENDORF also describes exactly who belongs to which class. He notes emphatically that for the purpose of explaining the opposition of interests, the powerful class needs to be larger than it is in the light of elite-theories à la PARETO or MOSCA.

The question is whether and how DAHRENDORF’S alternative to the extant theories can be tested. LOPREATO (1968) attempted to do so and found most of the predictions to be at odds with the research-findings, using test-scores obtained via a national survey.

LOPREATO acknowledges, however, that DAHRENDORF would object to these tests. Since the latter distinguishes between manifest and latent interests, he can refuse to regard the survey-scores as valid test-results. LOPREATO rightly criticizes this position by pointing out that DAHRENDORF himself used survey-results in order to (as the latter put it) „illustrate some points“ (DAHRENDORF 1959: 281, LOPREATO 1968: 73). Let us elaborate LOPREATO’s criticism of DAHRENDORF, because the implication of the latter’s remark is crucial: successful tests with measuring techniques relying on false assumptions do count, while unfavourable results obtained with the same techniques are dismissed. Thus, if the prediction holds, the interests are manifest; should the prediction be disproved, the interests are latent.

It appears that DAHRENDORF uses an auxiliary hypothesis here; one which specifies which interests are manifest in certain circumstances, and which latent in others. Unless hypotheses clearly specifying the relevant circumstances are explicitly formulated, the informative content of the auxiliary hypotheses, and therefore of the whole theory, is indeterminate. Every unfavourable test-result can be explained away at will. It can simply be argued that the circumstances were such that the interests were latent, as was „shown“ by the results.

Let us see whether DAHRENDORF advances informative auxiliary hypotheses. He states:

„Latent interests are articulated into manifest interests . . . Articulation of manifest interests . . . can be prevented by the intervention of empirically variable conditions of organization. Among the conditions of organization, technical conditions (personnel, charter), political conditions (freedom of coalition) and social conditions (communication, patterned recruitment) can be distinguished. To these, certain nonstructural psychological conditions (internalization of role interests) may be added“ (DAHRENDORF 1959: 238–9).

This specification of relevant conditions appears unproblematic with the possible exception of the last-mentioned variable, „internalisation of role interests“, which might render the statement uninformative. What matters here is whether DAHRENDORF regards every independent variable as a necessary pre-condition: if so, then one weak link renders the whole chain useless.

On the other hand, if he regards them as simple conditions, then it is quite possible to drop one of them without impairing the content of that particular part of the statement. However, DAHRENDORF’S auxiliary hypotheses do not
simply amount to a specification of relevant conditions, but moreover to a revision of the concept of „interests“. For the auxiliary hypotheses to be falsifiable, it is necessary that the three types of interest (present manifest, present latent and absent) can be empirically distinguished from one another. DAHRENDORF describes interests as follows:

„Orientations of behaviour which are inherent in social positions without necessarily being conscious to their incumbents (role expectations) and which oppose two aggregates of positions in any imperatively coordinated association, shall be called latent interests. . . . Manifest interests shall mean orientations of behaviour which are articulate and conscious to individuals and which oppose collectivities of individuals in any imperatively coordinated association“ (DAHRENDORF 1959: 237–8).

Such a description of latent interests is so vague that it is not only the ill-disposed researcher who can interpret nearly every absent interest as a latent one. Furthermore, it is striking that no description is given of absent interests; such a description is desirable in view of the empirical testing of the auxiliary hypotheses. Thus, it appears that the auxiliary hypotheses concerning manifest and latent interests have immunised DAHRENDORF's theoretical alternative against criticism, or have at least diminished their amenability to criticism.

Another objection can be levelled against DAHRENDORF. For even if the informative content of the auxiliary hypotheses were neither void nor indeterminate, then it can still be said that the content of the hypotheses has been chosen in one direction only. DAHRENDORF appears to have employed the second as well as the first version of the ad hoc strategy. If one introduces the possibility of opposed latent interests, then one could obviously also consider the possibility of corresponding latent interests in the case of opposed manifest interests. Why is it that only an opposition of interests can be obscured? Can it be denied that conflicts may be exaggerated, or that men may fight with little at stake? If one wishes to increase the possibility of criticism, one should increase the content of the auxiliary hypotheses: alongside the old explanations of the test-results confirming the presence of opposed manifest interests, an alternative explanation should be placed, expressing the notion of corresponding latent interests.

Thus, one seeks not only alternative explanations for unexpected test-results, but also for predicted ones. If the principle of subjection to critical testing is taken seriously, one does not simply extend an argument until all apparent refutations have been accommodated. One also tries to see if apparent confirmations may not in fact be refutations.

Auxiliary hypotheses with a content appropriate to that aim have not been included in DAHRENDORF’s theory, so that on that count, as well, the theory exhibits immunization tendencies.

4. The Reluctance to Refute Renowned Thinkers

The immunization-practice that we shall deal with now could perhaps be termed the reluctance to admit refutations where the theories of renowned men are concerned. This practice may perhaps be regarded as a specific case of the third variety of the above mentioned ad hoc strategies involving auxiliary hypotheses. But in a more general formulation, this practice can be distinguished in its own right: for one may alter central hypotheses without explicitly indicating such modification.

A scientific community will usually be found to recognize some persons as having produced singularly commendable work. When a scientist attacks the theories of one of these renowned thinkers, his action is sure to be frowned upon. This will happen particularly among those who adhere to the classical methodology and who see in refutations the stalemate of science: after all, it then appears that an attack is made upon the „authoritative sources of knowledge“. In this way, there may emerge within the scientific community a certain miss-placed deference with respect to the empirical testing of these generally respected theories. Instead of concluding that the theory of a particular prominent person is incorrect, there will be a tendency to see the theory as having been „improved upon“, or „elaborated“. This immunization-practice involves the surrender of content as a regulative idea, for by means of it, the difference in content between theories is passed over.

An example of this practice can be pointed
out in *Union Democracy* by LIPSET, TROW and COLEMAN (1956). The authors themselves contend at numerous places throughout the book that the American trade-union they studied is a "deviant case" with respect to MICHELS' iron law of oligarchy. The particular trade-union was found to have maintained its democratic character, contrary to MICHELS' theory which holds that no voluntary organization can do so in the long run. MICHELS thus appears to have been refuted. The authors, however, say:

"This study has not 'disproved' Michels' theory; rather, in a sense it has given additional empirical support to his analysis. . . ." (LIPSET et al 1956: 464)

Now it may very well be that, as the authors themselves hold, if one hypothesis is removed from MICHELS' theory, "that" theory can explain the evolution of the particular trade-union studied. But to act as if both theories convey the same information implies the abandonment of content as a regulative idea and can only hinder the growth of knowledge.

5. The Retreat into Essentialism and the ad hoc Modification of Definitions

To the list of immunization-practices belongs also the *retreat into essentialism*, which can be regarded as a special case of the *ad hoc modification of definitions*. The retreat into essentialism entails operating with the notion of "essence" in order to save a hypothesis in the face of refutational results. A nomological hypothesis does not insist upon, but prohibits the existence of certain states of affairs. Now, when a hypothesis has been refuted, one can argue that what was held to be impossible (and now apparently is not), is really so, because that which has been shown to be possible is not essential to it.

Such reasoning is especially common to Marxist theories, which posited a large number of hypotheses about matters that were held to be impossible within the structure of a market-regulated economic system. When the course of development of capitalism over the last century showed that a large number of these matters could in fact take place (and particularly alterations in what had been designated the most deplorable facets of the systems), different aspects that had previously been neglected were now brought to the fore; these were now regarded as the system's essential characteristics while other aspects were suddenly termed inessential.

ENGELS ranks among those who used empirical social research findings to demonstrate the thesis that the operation of the "invisible hand" of the market-mechanism did not always increase the "wealth of nations" in such a way that the greatest happiness would befall the greatest number of people. Within Marxism, an opposing thesis gained a prominent place: it was predicted that the state of wretchedness and size of the class lacking access to the means of production would increase. Yet this thesis too failed to withstand the test of time. History has shown that a system in which the economy is regulated by a market-mechanism, is not incompatible with a system in which the welfare of those who do not possess the means of production increases.

Some important aspects are revealed by logically analysing the 1892 Preface to the English Edition to ENGELS' *Die Lage der arbeitenden Klasse in England, nach eigener Anschauung und authentischen Quellen*, which was originally published in 1845. While in 1845 the book confidently refutes the optimistic capitalist thesis with the use of various figures, the situation has become different by 1892. History had discredited the pessimistic version of the thesis that ENGELS had adhered to in his book, so that the refutational evidence needs to be taken into account. ENGELS however does not advance a progressive modification of his original theory. Instead he attempts to maintain it by obscuring the refutations with the notion of "essence". We quote from the 1892 Preface, the emphasis each time being ours:

". . . the manufacturing industry did become apparently moralized . . ." (22);
"Thus the truck system was suppressed, the Ten-Hours' Bill was enacted and a number of other *secondary* reforms introduced . . ." (23);
"Thus the development of production on the basis of the capitalist system has of itself . . . sufficed to do away with all those *minor* grievances" (23);
". . . this or that *secondary* grievance . . ." (24);
"Again, the repeated visitations of cholera, typhus, smallpox and other epidemics have shown the British bourgeoisie the urgent necessity of sanitation in his towns and cities . . . The bourgeoisie have made fur-
ther progress in the art of hiding the distress of the working class. . . " (24)

ENGELS had in 1845 insisted upon certain issues. They were issues that he had tried to unmask with the use of empirical findings and which we may assume he regarded as being of prime importance (who after all emphasizes secondary matters). By 1892, however, those issues had become fortuitous, and thus apparently even a decrease in the death- and illness-rates no longer indicated a real amelioration but, on the contrary a further deterioration in conditions. ENGELS’ theory had been refuted and he tried to preserve it by means of essentialistic arguments which immunise the theory by designating at will formerly crucial facets as secondary ones. One can also say that ENGELS had changed from one theory about certain systemic characteristics to another theory involving different characteristics. He then tried to make the change plausible with essentialistic arguments, thereby obscuring the ad hoc modifications of the theory’s concepts (in ENGELS’ case: increasing misery).

We shall attempt to indicate a more obvious instance of this immunization-practice of altering definitions in an ad hoc manner, in discussing an important theory of C. W. MILLS.

In 1946 MILLS proposed a set of hypotheses in order to be able to predict that „civic welfare“ would be less in cities in which a large number of small firms provide most of the employment opportunities, than in „big business cities“, in which a small number of large firms provide most of those opportunities. „Civic welfare“ of „community welfare“ denoted such matters as child-mortality rate, average income, housing conditions, public expenditure on recreation etc. Three pairs of cities were careful selected as research-units. The results were as predicted.

IRVING FOWLER carried out a replication of MILLS’ study, using thirty cities in the state of New York and broadening the operationalization of the concept „civic welfare“ to include more variables than MILLS had used. FOWLER published his results in an award-winning article (1958), in which he showed that his empirical findings were unlike those of MILLS. According to the principle of subjection to critical tests, it should then be acknowledged that MILLS’ theory (as it stands) has been falsified and that a modified theory, which is better able to explain civic welfare, should be proposed. GOLDSEN, who belonged to MILLS’ circle, wrote an article for the 1964 book dedicated to the memory of MILLS. Instead of exploring the modified theory proposed by FOWLER, she poses an essentialistic question: „Was he (Mills) wrong? After all, what is welfare and how you measure it? Fowler . . . had examined and tested 48 indicators of eleven different facets of community welfare. But . . . what about the psychological climate, the spirit, the sense of your own dignity and worth? What about confidence in the future for your children and their children? What about the feeling that your existence is serious and meaningful? A study like Fowler’s by no means settles the matter. The carefully woven net he had cast to capture the notion of „welfare“ might still have been too wide-meshed. Some of these more elusive aspects could still have slipped through“ (GOLDSEN 1964: 91–2).

GOLDSEN here rejects the usual interpretations (also accepted by MILLS himself) of the term „civic welfare“ in an ad hoc manner, and redefine the term by introducing new variables that were not previously considered to be of any importance to the operationalization of MILLS’ concept (otherwise he himself would surely have included them, and moreover, would have left out some that he did include). As the concept „civic welfare“ is changed in content, so is the hypothesis which incorporates that concept. Also, it has been overlooked that the tested statement was derived from a theory which tried to explain why civic welfare would be more extensive in „small business cities“ than in „big business“ ones. Even if what has been suggested about the „more elusive aspects“ should withstand critical testing then the theory is still at fault. The question as to what is wrong with the theory is merely obscured and not considered by altering the usage of terms after a refutation has been presented. By brushing over the differences in content between theories, content as a regulative idea is abandoned and the criticism of statements rendered more difficult.

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2 We did not have access to the 1946 government-publication by MILLS and ULMER. A summary is presented in BROOM and SELZNICK (1963: 623–6) as well as in KEFAUVER (1966: 186–96).

3 For the relationship between the content of concepts and the content of statements incorporating those particular concepts, see OPP (1970: 176–8).
6 Immunization and the Assumption underlying the Measuring Techniques

Our point of departure in this section is the oft-forgotten proposition that a single, isolated, hypothesis is logically falsifiable, but can never be falsified in practice. The reason is simply that empirical research must necessarily involve more than one hypothesis. Every test is carried out with certain measuring techniques, and no matter how simple such techniques may appear, they always presume other theories than the one explicitly being tested. (Thus, the survey-question „which political party did you vote for at the last election“ relies on the truth of hypotheses such as „if people are asked which party they voted for at the last election, and if they voted for the Socialist Party, then they will reply that they voted for the Socialist Party“). Now, when an unpredicted result turns up in empirical research, the refutation can be ascribed to two distinct sources:

(1) the falsity of the hypothesis which is the „raison d’être“ of the research, the research hypothesis;

(2) the falsity of the hypotheses underlying the measuring techniques used, i.e., in research terminology, the invalidity of the measuring instruments.

Unexpected test-results only imply that „something is wrong somewhere“, without indicating „what“ or „where“.

When a refutation is ascribed to the research hypothesis without much further ado, one demonstrates an excessive faith in data and, as a result, immunizes the often implicit observational theories underlying the measuring techniques. When, on the other hand, one simply ascribes the refutation to the observational theories, one demonstrates a pious faith in the theories to be tested, and immunizes those theories. In both cases one acts on the assumption of the certainty of one particular hypothesis, and when a refutation is obtained, one only proposes hypotheses with a particular content as theoretical alternatives. In the first case one never proposes new hypotheses referring to the measuring techniques, while in the second

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4 Logically falsifiable, that is, in the sense that a potential falsifier can be devised.

5 The first reports of this piece of research were published in 1957 in various journals.
that of the others\textsuperscript{6}. Because WINICK's measuring instrument relied on a false theory, one is correct in not concluding that his research hypothesis is false. This is not a case of chosing one theoretical alternative over another at will, but of refraining in a well-reasoned manner from drawing one particular conclusion from the unexpected test-results. The latter are here dismissed on the proposition that the measuring instruments are invalid only because it is shown what is in fact wrong with those instruments, and furthermore, because attempts are made to check the dismissal of the results.

It should be noted that even repeated rejection of observational theories does not have to imply an attempt at immunising the research hypothesis. STEVEN LUKES recently alleged that DURKHEIM immunised his theories as follows:

\"we may allude to Durkheim's rather high-handed way with evidence. . . At the \textit{Ecole normale superieure} when told that the facts contradicted his theories, he used to reply: \textquoteleft{}The facts are wrong\textquoteright{} (LUKES 1973: 33).

\textbf{Figure 2} DURKHEIM's structural explanation of suicide-rates:

As far as for example DURKHEIM's theory of suicide is concerned, this practice certainly did not immunise that theory. The contemporaneous statistical records were extremely disorganised and comparison at an international level

\textsuperscript{6} COLEMAN et al. themselves compared the dates found on prescriptions with those as recalled from memory by the particular doctors concerned. Doctors who occupied a less central place in the local network tended more often to indicate a date prior to that as recorded in prescriptions.

was scarcely possible. The international congresses that were supposed to bring about some measure of coordination had failed, while the work of the \textit{\text{"{Bureau Internationale de Statistique\textquoteright{}}} had only just commenced. In those circumstances it could do no harm to insist upon improvements in data before possibly abandoning hypotheses that were at odds with the available statistics. Retaining one's hypotheses is then the only way to further the improvement of statistical records, which in turn would lead to improved hypotheses.

\textbf{Digression:} some remarks on a way of immunising hypotheses which do not express monotonic relationships.

It seems possible to translate DURKHEIM's suicide theory into two mathematical functions.

The independent variable in the first relationship (figure 2) is the degree of integration of an individual into some group, while in the second relationship (figure 3), it is the number of group norms that are operative for an individual. The dependent variable is in both cases the suicide-rate. Neither function specifies a monotonic relationship as do most hypotheses in sociology. Instead, both functions are parabolic.

Looking at the first function: on the left side of the parabola is the egoistic type of suicide (the individual's inte integration is insufficient), while on the right side we have the altruistic
Figure 3  DURKHEIM's cultural explanation of suicide-rates:

Type (the individual's integration is excessive). Like-wise for the second function: on the left there is the anomic suicide (the individual's desires are not compatible with his means, because there are too few norms restraining his desires), and on the right the fatalistic suicide (the individual's desires are not compatible with his means, because there are too many norms circumscribing the employment of means). The empirical testing of such parabolic functions requires far more precise measuring techniques than is generally the case for monotonic functions. For one needs to be able to situate any score obtained: i.e. one needs a measuring technique which will determine whether an individual's score is above or below either the maximum or the minimum of the relevant function. Lacking such a measuring technique, the following possibilities can not be avoided:

(1) a negative result for the egoistic theory can be transformed into a corroborative result for the altruistic theory;

(2) a negative result for the altruistic theory can be transformed into a corroborative result for the egoistic theory;

(3) a negative result for the anomic theory can be transformed into a corroborative result for the fatalistic theory;

(4) a negative result for the fatalistic theory can be transformed into a corroborative result for the anomic theory.

The following holds for monotonic functions: for every value of the independent variable it can be said that, if one individual scores higher on the independent variable than another individual, then the first individual will never score lower on the dependent variable than the second individual (or vice versa in the case of a negative monotonic relationship). Thus, in order to test the relevant statement, no other information is needed apart from the particular statement "this individual scores higher (or lower) on the independent variable than that individual". In the case of such "U-shaped" curves as we are concerned with here, however, the following holds:

(1) for every value of the independent variable below a certain score, it can be said that, if one individual scores higher on the independent variable than another individual, the first individual will score lower on the dependent variable;

(2) for every value of the independent variable above that certain score it can be said that, if one individual scores higher on the independent variable than another individual, the first individual will score higher on the dependent variable.

Such parabolic functions require a certain precision of measuring technique. Otherwise, and not withstanding their falsifiability and content, such functions are immune to critical examination. Such theories will always survive empirical tests, should one want them to survive.

It appears then that DURKHEIM, who certainly
did not have more precise measuring techniques at his disposal, has constructed his argument such that the inherent difficulty in testing non-monotonic functions is allowed to remain unnoticed. The book gives the impression that his theory contains not two but four functions. By treating egoistic suicide separate from altruistic suicide in another chapter, and by disposing of fatalistic suicide in a footnote, each parabola is exactly bisected (see the dotted lines in figures 3 and 4), thus giving the impression that DURKHEIM's theory contains four independently co-existent monotonic functions which do not require more precise measuring techniques for their empirical testing.

Finally: a critical-rationalist, confronted by such a situation, need not claim that such a theory is always "sustained" at will and leave it at that. Rather, he will advocate that:

1) it is in the interest of the growth of knowledge that measuring techniques of greater precision be developed which will remove the theory's practical immunity vis-à-vis reality;

2) failing such developments, that one should regard the question as to the truth of theories expressing non-monotonic relationships, as premature.

7 Conclusion: the Different Ways of Abandoning Content as a Regulative Idea

It has already been pointed out that immunization-practices which operate by virtue of abandoning content as a regulative idea, do not necessarily involve statements lacking in content. The escape into tautologies is only one of several examples. The list of immunization-practices is not completed by the addition of the practice of using auxiliary hypotheses of indeterminate content. It is certainly possible that an author puts forward only statements that do have content and still obstructs criticism. Such is the case when opportunities to increase the content of theoretical alternatives are systematically neglected, or when out of two possible alternatives only one is adopted without any attempt at arguing the choice.

But even then the list of immunization-practices gives a one-sided impression, because criticism may be obstructed not only through the statements themselves, but also through the attitudes attached to statements. One can obscure a difference in content between two statements and pass them off as conveying the same information. One can likewise obscure a change in content by surreptitiously altering definitions when statements are confronted with unexpected empirical findings.

SCHMID published an article in 1972 dealing with ad hoc practices in empirical social research. He looks at the situation where one has come to a contradiction between a prediction and a research-finding; a contradiction moreover that cannot be ascribed to the observational theory. SCHMID then distinguishes three ad hoc practices which can be utilised to resolve the contradiction. One such practice he rejects because it involves the use of hypotheses lacking in content. The other two practices, however, he does not reject since they employ only informative and falsifiable hypotheses. Let us briefly examine these last two practices.

An explanatory theory consists of several "levels" of hypotheses differing in content, connected by auxiliary hypotheses. One practice now is to attempt to theoretically resolve the contradiction by replacing one of the auxiliary hypotheses and leaving the highest-level hypothesis untouched. SCHMID fails to find fault with this practice on condition that the new auxiliary hypotheses be also informative. However, we would stress firstly that as a result of such a replacement, a new theory is put forward with all the consequences thereof. Theories differing in content must not be passed off as being identical, and hence the new theory needs to be independently tested. We would furthermore stress that while the proposed theoretical resolution is progressive, a more progressive resolution yet is possible. For one can opt for a strategy of theoretical pluralism by considering a replacement of the highest-level hypothesis as well as that of an auxiliary hypothesis. Faced with the question "must the contradiction be resolved by altering the highest-level hypothesis or one (or more) of the auxiliary ones?", the answer that maximizes the possibility of theoretical progress is "by concurrent attempts to do both"!
The other practice that SCHMID condones is that of attaching to a given hypothesis a new one which can account for deviations by specifying some additional factor. SCHMID argues the progressiveness of this step in terms of the possible development of the new hypothesis; it may grow into an independent hypothesis, which has more explanatory power than the old one. But again we would stress that the content of the old hypothesis is altered by the attachment of a novel specification. Conversely, we fail to see why the extended hypothesis should not immediately be put forward as an independently formulated hypothesis. In the latter case, the hypothesis is more easily falsifiable and a clearer alternative to which other hypotheses may be compared.

It is, strictly speaking, correct to say that these practices as described by SCHMID do not render criticism impossible. But the usage of hypotheses lacking in content is not the only way to obstruct criticism, and it would appear that in the last two cases distinguished by SCHMID the available opportunities for increasing the scope of criticism are not fully utilised. It needs emphasis that a researcher can evoke certain attitudes towards statements by presenting them in a certain manner. One can act as if a difference in content between two statements does not exist, and one can act as if theoretical modifications are of little or no significance. But one then fails to proceed as one should if content as a regulative idea is accepted. For example, one must acknowledge that a theory with a different content must be subjected to a different empirical tests. Further, one must pose the question whether the new theory is true and ask how that theory may be subjected to empirical test. The growth of knowledge is obstructed not only by working with hypotheses lacking in content, as is most frequently emphasized, but also by the refusal to make such acknowledgements and to pose these crucial questions. There are several ways of abandoning content as a regulative idea, and it is hoped that this article has pointed toward some of the less frequently mentioned ones.

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