

The practically relevant deductive nomological explanation of literary actions and quantitative or qualitative methods of data acquisition

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It should be made clear from the start that quantitative and qualitative methods of data acquisition in the empirical sciences do not mean fundamental differences between aims of research, as it is occasionally claimed when having in mind the aim of prediction on the one hand and that of understanding on the other. By the way, there is still a third method of empirical data acquisition, that of acquiring comparative data. And to conclude my preliminary remarks a widespread error exists in automatically subsuming statistical data processing under the heading of quantitative data acquisition. Very often it is qualitative data which are statistically processed.

Statistical data processing and inductive statistical explanation have one decisive snag. They cannot provide a deductive nomological explanation of a singular event. On the other hand, because of its application of the mathematical theory of probabilities they give the wrong impression of an approximation to truth. But since the ideology of true knowledge about an empirical object was vanquished by Tarski's notion of truth and Maturana's biology of knowledge, statistics in the empirical sciences has lost a clear objective and often a methodological justification, and accordingly sometimes produces very curious and ephemeral results: To give an example which I ran into in one of the daily newspapers of Berlin - it was *Der Tagesspiegel* of February 2, 02 -, a study of Harvard University tried to prove statistically that a country with a cool climate, a sound economy and the lack of a functioning democracy can hope to achieve Olympic medals with a probability of 96 per cent. I will only quote here the judgement on certain scientific statistics by the German philosopher of science Hans Albert, which he pronounced almost forty years ago: "Vor allem verträgt sich mathematische Exaktheit ganz ausgezeichnet mit methodischer Schlamperei. [Above all mathematical exactness excellently complies with methodical sloppiness]." ¹

Of course, statistics has a limited justification not in the development of practically relevant scientific empirical theories, but in supporting practical decision-making with alternatives of practical actions. This may be called applied mathematics, but it doesn't found an independent empirical science in a strict sense. On the other hand statistics obviously plays a role in certain paradigmatically established empirical sciences such as nuclear physics or genetic biology. But there it steps in on the basis of already existing paradigmatic explanatory theories, where deductive nomological explanations cannot be achieved for the moment, or for principal reasons of scientific observation. ² It cannot, however, help develop explanatory empirical theories in a strict sense and practically relevant deductive nomological explanations.

¹ Albert 1964, 38

² It's interesting, however, that Einstein in a letter to Popper in 1935 raised his doubts about the finality of statistical results even in quantum mechanics: „Man kann sich fragen, ob der statistische Charakter unserer experimentellen Befunde gemäß der heutigen Quantentheorie erst durch die fremden Eingriffe inklusive Messungen veranlasst wird, während die Systeme als solche ... sich an sich deterministisch verhalten. Heisenberg liebäugelt mit einer solchen Auffassung, ohne sie konsequent zu vertreten.“ [“The question may be asked whether from the point of view of today's quantum theory, the statistical character of our experimental findings is merely the result of interfering with a system from without,

As finding, or approaching, empirical truth is no longer tenable as an objective of empirical science, pragmatism seemed to open a way out of the dilemma. As W. B. Gallie put it in his book on *Peirce and Pragmatism* (1952): “Science, the free spirit of inquiry, the will to learn, have their roots in our practical beliefs, and have as their initial motive simply the ‘fixation’ of these beliefs.” But, as Gallie also formulated: “the ends and standards of scientific inquiry cannot be equated with those of practice – whether conceived in terms of immediate economic utility, personal well-being, or social cohesion.”³ And Alfred Schutz expressed this even more distinctly in 1951: “Pragmatic justification (that is the practical ratification of thoughts by actions in the outer world) even becomes a criterion of truth”⁴. But neither pragmatic justification – whatever that is –, nor even practical usefulness or relevance – which is something more precise and restricted, as I’m going to explain – can be considered a criterion of truth. And since Kuhn’s book on scientific revolutions, at the latest,⁵ a steadily growing bulk of homogeneously accumulated and fixated “practical beliefs” seems very unlikely. On the contrary scientific ‘findings’ often come and go like fashions in society.

Practical relevance, in contrast to the untenable fixation of the rather woolly “practical beliefs”, here shall mean contribution to the further development of cooperative capacities of action with the aim of satisfying joint needs (= *German*: “gemeinschaftliche Bedürfnisse”, a notion developed by Karl Marx on the basis of ideas of Hegel⁶) by means of practically relevant deductive nomological laws. These laws can prove their actuality (= *German* *Wirksamkeit, Wirklichkeit*), not their truth – although Marx in his time couldn’t yet make a clear distinction between the two notions⁷ –, only by their successful application with the improvement of a practical capacity.

Let me add some further clarifying remarks on my notion of practice. Practice is no longer the opposite of theory as it was in antiquity, it’s meant to contain non-scientific as well as scientific practice. It consists of cooperative actions as applications of joint capacities/know-how to directly or indirectly satisfy joint needs for better survival. It takes place in our *Wirklichkeit*, the unsurveyable context of cooperative actions – and of *quasi*-actions, a term to be explained below. There is no Anglo-Saxon word for *Wirklichkeit*. “Reality”, as signifying the totality of empirical objects either static, or

which comprises measuring it, while the systems in themselves ... behave in a deterministic fashion. Heisenberg flirts ... with this interpretation, without adopting it consistently.”] And later: „Meiner Meinung nach ist die gegenwärtige prinzipiell statistische Beschreibung nur ein Durchgangsstadium.“ [“A description ... which, like the one now in use, is statistical in principle, can only be a passing phase in my opinion.”] (Popper 1960, 462/458 and 464/459)

³ Gallie 1966 (1952), 89

⁴ Schutz 1970, 6

⁵ Thomas S. Kuhn, 1962: *The Structure of Scientific Revolutions*, Chicago/London

⁶ Marx 1953, 88

⁷ Marx’s 2. thesis on Feuerbach: “Die Frage, ob dem menschlichen Denken gegenständliche Wahrheit zukomme – ist keine Frage der Theorie, sondern eine *praktische* Frage. In der Praxis muß der Mensch die Wahrheit, i. e. Wirklichkeit und Macht, Diesseitigkeit seines Denkens beweisen. Der Streit über die Wirklichkeit oder Nichtwirklichkeit des Denkens – das von der Praxis isoliert ist – ist eine rein *scholastische* Frage. [The question whether human thinking can achieve objective truth – is not a question of theory, but of *practice*. In practice man must demonstrate the truth, i. e. actuality and power, worldliness of his way of thinking. The dispute about the actuality or non-actuality of a way of thinking – which is isolated from practice – is a purely *scholastic* question.]“ (*Marx Engels Werke* 3, 1969, 5)

in motion, is not an equivalent. Practice can have enormous effects in our *Wirklichkeit*, but we cannot objectively survey the latter as a whole.

If we want to scientifically collect empirical data, either quantitative, or qualitative, or comparative data, for the achievement of practically relevant results, from where should we start? Of course, not with data acquisition itself, and not with a theoretical definition of an underlying object of empirical research. The latter is a fundamental mistake, often found with preparadigmatic empirical sciences, such as the empirical science of literature, which was e. g. designed as a science of literary actions. Scholars yearn for something at least to come to terms with. But in this case a slight glance at the empirical science of physics, the mother of all empirical sciences, would have sufficed to teach them how little physicists have succeeded in defining a fundamental object of their empirical research. And all the same they were highly successful in practice and for good reasons presented the model of, and an incentive to, every other empirical science. Does light consist of corpuscles or of waves or of both (= light quanta)? Is the ultimate object of physics particles or energy or “strings”, a strange combination of both? And what are these “strings” really? The answer of an expert with the name of Stephen Hawking is: “string theory has several problems that must be solved before it can be acclaimed as the ultimate unified theory of physics. ...But can there really be such a unified theory? Or are we perhaps just chasing a mirage?”⁸

Empirical scientific work must start with practical problems which hamper practical capacities and with the political decision about which practical capacity should be improved with priority. This was the way it virtually happened in 17th-century optics or mechanics, though physicists in that period still also believed in a search for truth. Take the example of those troublesome chromatic halos produced by lens telescopes. In Newton’s *Opticks* this led to the reinterpretation of light rays as an overlap of various monochromatic rays with different deflective indices when passing into a denser optical medium. But only by avoiding this entry through the construction of concave mirror telescopes the physical explanation got efficient and *wirklich*, i. e. practically relevant. As Popper already said, experimental checks may falsify some deductive nomological explanation and by this can help to further develop an empirical explanatory theory, but they cannot verify the explanation itself.⁹ This can only be proved practically efficient and in so far practically relevant through successful application in the further development of practical capacities.

Reinterpretation of the light rays themselves isn’t, however, a correct designation for what happened in optics, because exactly speaking it was the colourful **deflection of the light rays** that was reinterpreted and this **process** became the object of a possible empirically explanatory theory, which had to be constructed as a *quasi*-action in analogy to an unimpaired practical predecessor of the impaired practical kind of actions (i. e. the **refraction** of light rays by means of mirrors without the troublesome chromatic halo), because only from there we can develop a necessity of the problematic phenomenon to be explained. This necessity isn’t an objective causal relationship, the objective existence of which was already questioned by Hume¹⁰, but a practical expectation of the consequences of our actions necessary for our survival,

⁸ Hawking 1988, 176

⁹ Popper 1960, 251-84

¹⁰ Hume 1955, 82: “Beyond the constant *conjunction* of similar objects, and the consequent inference from one to the other, we have no notion of any necessity or connexion.”

which we confer on empirical processes for the same reason. This transfer, however, involves a specific structure for scientific deductive laws in analogy to every kind of life-sustaining practical actions, a structure characterized by a necessary dependence of a *quasi*-result (= different deflections of one light ray) on a *quasi*-material (= different superimposed monochromatic colour rays) and a *quasi*-instrument (= denser optical medium). Interestingly enough it was Piaget, who thought in the same direction before: “Causality... expresses what the **objects do** [my boldface, E. N.] as they act on one another...” and “causality is known by analogy with the operations of the subject”¹¹. I would progress even a step further than Piaget, who only speaks of the knowledge of causality by analogy to the operations of the individual subject. I consider it a construction by analogy to a cooperative practical action, which is characterized by the application of joint know-how and serves the elimination of joint needs for better survival. (This is what the German word *Notwendigkeit* for English *necessity* originally meant: abolishing a need.)

So the second step after selecting the practical problem to be scientifically explained is the construction of the object of the potential explanatory theory, which is not an object of a descriptive theory. As a third step we need a first specific empirical lawful hypothesis to define the **t-theoretical functions of the theory**, those terms which only belong to the explanatory theory.¹² This lawful hypothesis must contain a specific three-valued necessary logical relation between a *quasi*-result, a *quasi*-material and a *quasi*-instrument. Exactly speaking it helps define the t-theoretical terms of the theory: *quasi*-material and a *quasi*-instrument, whereas the *quasi*-result, the t-non-theoretical term of the theory, is the intuitive reconstruction of the practical problematic phenomenon on the basis of an unimpaired practical predecessor. Deflection of light rays into something like a rainbow is reconstructed as something similar to practically unproblematic refraction, but with differing deflective angles corresponding to the various colour rays. And when in refraction the refractive angle is always the same as the angle with which the light ray meets the refracting surface, individual deflective indices for the various colour rays can be measured by the quotient of the angle of incidence and the angle of the passage through another optical medium. The hypothesis that various monochromatic colour rays are contained in the one light ray which enters the differing optical medium and that each of its monochromatic light rays has a relative deflective index is only empirical, if we can empirically identify the monochromatic rays and the specifically denser optical medium. But both can only be done by further applications of the same hypothesis.

And here at last we enter upon the central theme of methods of description or data acquisition for the various t-theoretical and t-non-theoretical terms of the explanatory theory. These descriptive theories or methods of empirical description generate no truth value and have no scientific relevance independent of an explanatory theory, but they stem from practical methods of data collection either qualitative, or quantitative, or comparative, which in some cases only have to be transmuted, combined or submitted to general conventional norms. With Newton's *Opticks* we can easily see that the methods are derived from non-scientific practice and that they can be quantitative as well as qualitative even within one practically relevant explanatory theory. From practical navigation Newton knew the instrument of a quadrant to quantitatively measure different angles and there was also a conventional scale of 360 degrees. For measuring deflection of light this had only to be developed further into a

¹¹ Piaget 1974, 1f. and 10

¹² For the t-theoretical and the t-non-theoretical functions or terms of a theory see Sneed 1979, 33.

yardstick of indices by arithmetical division. But as different monochromatic colours in Newton's time couldn't be quantitatively measured by wavelengths - Newton didn't even believe in light waves -, he simply relied on a common practical yardstick of what was red or blue or yellow. And regarding the density of an optical medium he didn't try to develop a quantitative scale, which he easily could have done by means of the various quantitative measurements of deflection of one and the same light ray with different optical media, and which we now dispose of as the scale of absolute deflective indices. But he contented himself with a comparative method of description.¹³ All the same his optical hypothesis proved highly efficient, i. e. practically relevant. The only thing he needed was the construction of a necessary dependence of different optical media on the different relative deflective indices of different colour rays. Otherwise he would not have disposed of a t-theoretical term of density of optical media and of the other t-theoretical term of monochromatic colour rays.

We can even go a step further back in history to Archimedes' principle of buoyancy, which can be constructed as a practically relevant deductive nomological explanation of physics without any quantitative data acquisition at all, only with purely qualitative and comparative methods of description. Buoyancy has only to be reinterpreted as a displacement of a part of a fluid by a solid body freely floating in it. If and only if some part of a fluid is displaced by a solid body freely floating in it, the weight of the displaced part of the fluid is equal to the weight of the floating body:

For all displacements x 's holds (Displaceable fluid $x \wedge$ Displacing solid body $x \rightarrow W_{Dfx} = W_{DSBx}$).

For comparing the two weights we only need a balancing beam scale, but not a quantitative yardstick of measurement for the individual weights.

Before we finally arrive at data acquisition for an empirically explanatory theory of literature, we have to clarify whether the deductive nomological explanation for prediction in physics must be considered the only kind of deductive nomological explanations in all empirical sciences that try to avoid substantialism or 'model Platonism' ["*Modell-Platonismus*"], as Hans Albert put it¹⁴, and no longer accept an outdated ideology of empirical truth, or whether another kind of practically relevant deductive nomological explanations could be visualized, which wouldn't effect practically relevant prediction for better production, but - to phrase it with all precaution - **practically relevant understanding**, of course, not in a non-empirical hermeneutic sense, but for better practical orientation or better practical criticism. Practically relevant explanation for understanding must also be distinguished from Hempel's scientific answering "*reason-seeking* or *epistemic*" why-questions, which in itself needn't have any practical relevance.¹⁵

A genetic reconstruction of human practice may show us that, from a certain stage of development, division of labour did not only mean specification and improvement of productive capacities, but qualitatively new branches of practical actions. A second, qualitatively different branch of practical actions, is **teaching practical capacities** (of

¹³ Newton 1952, 222

¹⁴ Albert, loc. cit., 27 ff.

¹⁵ Hempel 1965, 335

masters, teachers, coaches), and a third is **critically steering practical cooperative actions** (of managers, politicians, journalists, critics etc.) . And both kinds of practical actions have their corresponding *quasi*-actions with their specific logical necessities, which we presuppose in practice, even if we don't already dispose of adequate explanatory laws. Otherwise we would not even dare to anticipate the outcome of actions of our cooperators, or to communicate the deontic meaning of something for the learner of a specific practical capacity: "For all who want to climb Mount Everest certain little clouds may be a warning to postpone their climb." Which kind of *quasi*-actions may be chosen for the construction of an empirical explanatory object, depends on the 'political' decision of the scientists which specific practical capacity they would preferably try to help improve. If an empirical science should do supporting scientific research for improving capacities of teaching or of steering cooperative actions, it wouldn't be really a matter of better prediction, but of giving better reasons for specific practical activities or for relevant criticism of specific cooperative actions.

The second and third kinds of *quasi*-actions, though being likewise characterized by a necessary dependence of a *quasi*-result, - in contrast to **process** as the elementary kind of *quasi*-actions - come under the heading of **originations of deontic meaning**, and **originations of cooperative actions**, respectively. I cannot go into details here about the specific structures of these kinds of *quasi*-actions. I will only add that their structures aren't strictly universal as those of processes, but also related to special existing individuals¹⁶.

The improvement of a critical steering of cooperative literary actions of recipient participants of literary life will be my 'political' choice for a practically relevant empirical science of literature, to improve a specific practical literary capacity, in my opinion the capacity of successfully influencing the installation of specific valuation criteria among a reading public according to the individual needs and interests of an individual reader. The motive of the whole literary cooperation is the socialization of members of a disintegrating society by means of new values, exactly speaking new valuation criteria. A practical problem to be solved here is not to avoid different valuations between recipients of the same literary work, but to prevent inconclusive reasoning among the recipients in favour of a certain valuation of something in or at it, to formulate it more drastically: **the irrationality of the old interpretive and manipulative literary criticism**. The recipient obviously feels a desire to share valuations about a piece of literature – mostly only parts or aspects of it - and he often fails to convince his fellow recipient, not because they offer different valuing assessments, but because they often have different 'views' of what the fellow recipient refers to and what his valuing expression really means. So the hypothesis must focus on valuation criteria, developed by the readers from the authorial contribution to the literary cooperation, and on respective valuing expressions among the readers in their mutual cooperative communication.

If we accept the origination of a valuing cooperative action of a reading participant as the partial potential model M_{pp} – as the reconstructionist Stegmüller calls it¹⁷ – of an explanatory empirical theory, the first object of a descriptive theory or method of description for the t-non-theoretical term will be **valuation** in the common language

¹⁶ I tried a first sketch of them as early as 1989 in Nierlich 1989, 387-96.

¹⁷ See Stegmüller 1985, 108 ff.

of the role-taking reading participant and his fellow reader with reference to something in or about the authorial cooperative action.

In terms of a strict empirical description we are already confronted here with two difficulties. Of course, valuation can be described qualitatively, but what is the yardstick for a specific valuation? It was Gunter Gebauer, who in the continuation of the later philosophy of Wittgenstein made a fundamental linguistic distinction between ostensively, or descriptively, teachable words and the so-called “kriterienbedingten Wörter [= criteria-conditioned words]”¹⁸. And while Newton’s colour rays could easily be described by ostensive reference to a list of conventionally accepted samples, valuing expressions belong to the group of ‘criteria-conditioned words’. According to Gebauer criteria can be defined:

as individually adopted traits of situations, which serve as standards for rules of applications of linguistic expressions that convey information, but are neither ostensively nor descriptively teachable. They are, by convention, their conditions of application. The “criteria” are, on the one hand, individually adopted “cutouts of situations”, but with their help the correctness of the application of the “criteria-conditioned words” can be intersubjectively checked ... by the language community.¹⁹

And for ‘criteria-conditioned words’ holds that the adequate use of them only presupposes a subset of its criteria as Gebauer says: ‘As a rule the subset consists of exchangeable elements: some of them can be replaced by others. That’s why the criteria aren’t, normally, *necessary* conditions for the application of the respective linguistic expressions.’²⁰ And it can happen that some criteria belong to another colliding valuation as well. This requires data acquisition about the valid criteria among the communicating readers, when a special valuation of a reading participant takes place.

But that’s not all. How can we describe the object of his valuation? It is a trait of the authorial contribution - but which? -, a ‘cutout of a valuing situation’ of the individual recipient and a possible later criterion for other valuations among the reading public. The author, when taking his role in the literary cooperation, must accept the same motive of installing new valuation criteria among a literary public as the recipients do and the aim of his cooperative action is offering a conglomerate as varied as possible of future valuation criteria selectable by his recipients.²¹ But his offer is not fictive, and his conglomerate of possible valuation criteria must be empirically observable.

The description of a cutout for valuation is easy to provide with theatre performances, where the various recipients are present together and the progression of the play makes it easy to identify the special object of valuation. And critical steering of

¹⁸ Gebauer 1971, 31 ff.

¹⁹ loc. cit., 34: „Die ‘‘Kriterien’’ lassen sich definieren als individuell rezipierte Züge von Situationen, die als Standards von Anwendungs-Regeln der sprachlichen Ausdrücke dienen, die Informationen vermitteln, aber weder ostensiv noch deskriptiv lehrbar sind. Sie sind kraft Konvention die Anwendungs-Bedingungen dieser Ausdrücke, ohne dass eine logische Beziehung zwischen ihnen bestünde. Die ‘‘Kriterien’’ sind zwar individuell rezipierte ‘‘Situations-Ausschnitte’’, aber mit ihrer Hilfe lässt sich die Korrektheit der Anwendung der ‘‘kriterien-bedingten Wörter’’ intersubjektiv kontrollieren ... durch die Sprachgemeinschaft.“

²⁰ loc.cit., 33: „In der Regel besteht die Teilmenge aus austauschbaren Elementen: einzelne ‘‘Kriterien’’ können durch andere ersetzt werden. Daher sind die ‘‘Kriterien’’ normalerweise keine *notwendigen* Bedingungen für die Anwendung der entsprechenden sprachlichen Ausdrücke.“

²¹ Think of Goethe’s theatre principal [Theaterdirektor] in *Faust I*: “Wer vieles bringt, wird manchem etwas bringen. [He who offers many sorts of things will offer something to many a person.]“

theatre-goers among the audience was obviously the unimpaired practical predecessor of literary valuations of recipients among a reading public. The reading recipient does not construct an ideal work of art to perform a similar process in his mind - that was obviously the idea literary scholars seemed to support from Ingarden's phenomenological *Literary Work of Art*²² to Derrida's deconstructionism - , but he scans the cluster of mediated offers for valuation from the author, till one of his preferred valuing expressions, positive or negative, fits for her or him. So he offers the valued 'cutout' to another recipient as a further joint valuation criterion. This may not be accepted by the latter for his special reasons, but the communicative cooperation can only work, if both recipients have found out about joint criteria before, which serve as a yardstick for the possibly new joint criterion, which may serve as such not only for further literary works, but in social life in general.

After a partial potential model of an explanatory theory has been constructed on the basis of the structure of a fundamental kind of *quasi*-actions and of an unimpaired practical predecessor of the impaired kind of practical actions, the invention of a special empirical law or deductive nomological hypothesis is the most creative moment for the construction of an explanatory theory, just as Archimedes experienced it in his bath tub or Newton under the apple tree in his mother's garden. But even here we can go back, as Sneed already proposed, to practical notions: "The theoretical function and the constraints on their values can be recognized as being 'something like' pre-theoretical concepts that are relevant to the phenomena in question."²³

I must owe, the formulation of a first lawful explanatory hypothesis for the completion of an empirical explanatory theory of literature still has to be found. A framework for its invention offers itself with an obvious structure for all critical steerings of cooperative actions, and for all originations of cooperative actions as their corresponding *quasi*-actions. It's first a certain presupposition of the valuing action of a role-taking literary recipient, and second an intention of his within the literary cooperation. This rather complex and intricate and not yet completed construction will, however, point to the increasingly important problem of how to avoid manipulation of valuation criteria among a literary public within an increasingly multiethnic, multicultural and multisocial language community.

For the moment, however, I hope that three things, at least, have become clear: (1.) that quantitative data acquisition is, to say the least, of minor importance in an empirical science of literature, (2.) that statistical data processing leads astray for the development of an explanatory theory of a still preparadigmatic empirical science of literature, and (3.) that researchers in literary studies must urgently be warned not to begin with any fundamental definition of a comprehensive empirical object of scientific description, either qualitative, or comparative, or quantitative.

²² Roman Ingarden, 1931: *Das literarische Kunstwerk*, Halle

²³ Sneed 1971, 298

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