First publ. in: Social Studies of Science 21 (1997), 1, pp. 73-111

• ABSTRACT

In this paper we apply the laboratory study approach of the new sociology of scientific practice to a 'thinking science': theoretical physics. To specify the work and accomplishments of theoretical physicists we choose the notion of 'deconstruction'. Deconstruction involves the expansion of a concrete object, such as an equation, into a series of other objects upon which the 'hardness' of a problem can be shifted and distributed. In solving an equation, however, the determinate path of a deconstruction method needs to be supplemented by the exploration of clues and guesses, trials and tricks. We trace a series of devices, and iterations thereof, which physicists mobilize in dealing with hard problems: formal deconstructions, detours and tricks to identify a working deconstruction, variation, 'doing examples', modelling and, finally, thought alliances between subjects.

Deconstruction in a 'Thinking' Science: Theoretical Physicists at Work

Martina Merz and Karin Knorr Cetina

When theoretical physicists do a calculation, when they compute the BRST cohomology of the W-algebra, when they grade by the ghost number or face the *Tb* 77 term or when they discuss the advantage of doing *H(M®M*)* over *H(F®F)*, students of science usually look the other way. Are these sorts of operations still within the scope of our interest? More important perhaps, are they even within our reach? Can we study them ethnographically or observationally? What, if one could study them, might one find out?

This paper is a first attempt to use the laboratory studies approach[1] to begin to explore the epistemic practices of scientists engaged in the above kind of operations: we will look, in particle physics, at the realm of string theory,[2] a subfield of theoretical physics. The work done in this field counts as "theoretical theory";[3] theoretical particle physicists sort themselves into "phenomenologists", whose work is considered rather directly related to experiments, and "formal", "mathematical" or "theoretical" theorists, whose work is not.[4] In studying theoretical theorists, we shift the laboratory studies approach as far away as it can get from its original focus upon experimental settings. In the eyes of most practitioners, theoretical theory is, like mathematics, and perhaps like philosophy, a thinking science: a science in which laboratories might be 'disembedded' (held together by e-mail connections and travelling activities), work is performed at the desk, instruments are reduced to the pencil and the computer, and processing is realized through writing. Desk activities which consist mainly in the exploration of realizations of abstract algebraic structures do not lend themselves easily to direct observation. What is more, they are opaque even to physicists who are not fully involved with the same family of problems. The numerous discussions of the puzzles and paradoxes of physical theories which exist in the literature[5] are of little help in illuminating the darkness. They do not deal with physics theorizing as practical work, but with idealized reconstructed systems of thought.[6]

It has to be admitted that when we first entered the Theory Division at the European Laboratory for Particle Physics (CERN) in Geneva, Switzerland, with the explicit purpose to observe a theoretical thinking science at work, it was with some reservations.[7] It also has to be admitted that the laboratory approach had to be adapted to the obdurateness of the field: the study is based rather less on the observation of physicists' activities than on one analyst's capability to exploit her physics training and interact with participants as a member of their culture. It is also anchored in a close 'reading' of physicists' personal-professional communications (their e-mail correspondence; see note 3), their calculation protocols, and their explanations to us which invariably involved paper and pencil.[8] The close 'reading' was adopted to gain access to the ethnomethods implicated in doing theoretical physics work. Our approach yielded layers of methodical policies, "ansätze", tricks and other devices, which are piled into doing a theoretical computation. The policies, ansätze, tricks and devices were mutually embedded in one another within a sequential interactional system involving disembodied objects, several physicists and competing teams. In this paper, we try to unravel the system by focusing on the methodical side, leaving social interactional issues and contextual questions largely at the margin. This has the advantage that one begins to study the epistemic culture[9] of this field by shining the analytic torch on central (time consuming, absorbing, identity-defining, career relevant, and so on) involvements of practitioners. It has the disadvantage that such involvements are also the most esoteric and difficult for outsiders. As the reader will note, the obdurateness of theoretical physics objects finds its way into the present analysis and into the presentation of results.

Theoretical physicists' methodical practice has much to do with the building and understanding of disembodied objects such as models and equations.[10] In order to understand a disembodied object in the kind of universe studied (and in mathematics), one may have to change or reconfigure it, a process through which one also manipulates one's chances of success with the object. In Maurice Merleau-Ponty's terms, what gets reconfigured is not simply an object independent of a scientist or the subjective perceptions and conceptions of the latter, but the field of 'self-other-things', or the relationship between scientist and object." In this paper, we will specify this reconfigurational process in terms of a 'struggle' — as a physicist's struggle with the "hardness" of an equation — and trace the multiple transformations that such struggles undergo when physicists attempt to do a particular computation. The interactional dynamics of such struggles appears to consist of a curious oscillation between "straightforward" algorithmic practice and "being stuck", followed by non-algorithmic practice. Non-algorithmic practices are resorted to when following a pre-specified logic of procedure fails,[12] as it continually does.

To characterize the general thrust of physicists' policies in the struggle with theoretical objects we choose the notion of 'deconstruction'. Deconstruction is a term much used in postmodern social science and literary criticism to challenge traditional concepts of meaning, of authorial intention, of coherence and interpretation. The recent source of the term 'deconstruction' are three volumes by Jacques Derrida, published in 1967, which are devoted to the question of writing.[13] The meaning of deconstruction in Derrida's texts has been characterized both as specific (by Derrida interpreters such as Johnson and Norris)[14] and obscure (by critics such as Ellis),[15] with the source of the difficulty lying in the fact that Derrida cannot wish deconstruction to have a specific, self-identical meaning.[16] For the present purpose, deconstruction can nevertheless be glossed as a way to draw out a text's performative techniques. Deconstructive readings of texts (exemplified by Derrida's and Johnson's work) pay close attention not only to the logical structure and coherence of an argument, but also to the complementarities, digressions, circumventions, discontinuities and gaps inherent in its writing. They also bring out a text's suppressed meanings, silences and distortions of other claims. Deconstructionists reject the claim that a text can simply be interpreted according to an author's intentions and show instead its material working. Key notions in deconstructive literature are the concepts of 'expansion' and of 'supplementary logic'. For example, in a poem various images and metaphors can function as supplements, meaning additions and substitutes which simultaneously bridge and widen the gap between an original motivation or imperative in the poem and what the rest of the poem suggests.[17]

Writing a literary text, one presumes, differs from doing a computation in theoretical physics. Hence when physicists deconstruct an equation, the results will not be the same as those obtained when a poem is deconstructed. We choose the term deconstruction not to suggest a close similarity or identity between the two disciplines involved, but to open up the analogy from writing for mathematical-physical activity. Terms like 'expansion' and 'supplementation' seem to us to capture the general drift of physicists' unfolding procedures, which do not simply consist in decompositions of equations into pre-existing substructures, and in taking advantage of the less complex features of these substructures. Rather, physicists' procedures involve opening up a space for manipulations through expansions and complication, just as a poet opens up a space for literary manipulation (and literary reading) by making an original issue more complex through substitute images and other literary forms.

Simple forms of deconstruction in mathematics are known from school. For example, consider a quadratic equation like

$$X2 + 4A{-}{-}5 = 0$$

which is to be solved. The equation can be addressed by considering its left hand side as split into two terms $(x^2 + 4*)$ and -5, by then rewriting $(x^2 + Ax)$ as $(x + 2)^2 - 4$, such that the quadratic equation now reads

$$(* + 2)^2 - 4 - 5 = (A\text{-} + 2)^2 - 9 = 0,$$

noting that $9 = 3^2$, and by finally applying $a^2 - b^2 = (a + b)(a - b)$, such that the above equation is transformed into

$$(x+2+3)(x+2-3) = (x+5)(x-1) = 0.$$

The solutions of the quadratic equation are thus $x = 1$ and $x = -5$. The 'id6e maîtresse' (according to a French schoolbook)[18] is the rewriting of $x^2 + 4x$ which seemingly renders the equation more complicated: it expands the equation through supplementing $x^2 + Ax$ by $4 - 4$. As a result, a well-known identity can be applied. The deconstruction of the quadratic equation thus consists not only of a split or decomposition but requires an expansion of parts of the equation, and therefore of the equation itself.

Expansion is also a technical term in mathematics; it refers (for example) to the expansion of a function into an infinite sum of terms. Throughout the paper, the notion of expansion is employed in both ways; when meant as a technical term it appears in double quotation marks (for instance, "mode expansion").

Theoretical physicists practice a particularly intricate, sophisticated and elaborate version of deconstruction. This practice does involve features reminiscent of Derrida's supplementary logic: for example, it includes the expansion of a theoretical object, the substitution and replacement of original patterns by other sequences (through detours, variations and "doing examples"), and the exposure of hidden possibilities and implications which are coded into an equation but not immediately visible to the analyst. These practices are oriented toward closure. But, as in the case of literary texts, this closure that seems always within reach appears never quite attained. There are also disanalogies; for example, physicists draw on a repertoire of prespecified, highly elaborate formal instruments, perform their work collaboratively, and so on. The disanalogies will become obvious in the course of this text.

However, we are not interested in weighing analogies against disanalogies. If anything is to be suggested, then it is perhaps that practices of deconstruction found in mathematics and formal branches of the natural sciences played a role in Derrida's insistence on certain terms and on certain procedures. Barbara Johnson claims that it is 'no accident' that the word 'differential' is central both to Derrida's theory of writing and to mathematical calculus.[19] Derrida insists (as Johnson puts it) that a supplementary logic 'cannot be held in the head but must be worked out in external form',[20] and thus can be compared with the 'writing' necessities of formal disciplines. For example, an algebraic equation with more than one unknown also cannot be solved through thinking, as Johnson indicates. If Johnson is correct, then work such as the one investigated in this text can be seen as something of a genealogical forebear of deconstruction in literature and postmodernism.

It might be interesting to look at other formal/mathematical fields more closely from the angle of their work as writing, a perspective which remains at the fringe of the present paper. Instead, let us emphasize another facet of the work investigated, the phenomenon that seemingly mental objects are not only articulated through writing, but are subject to collaborative manipulation. Social scientists who study non-concrete objects in non-scientific settings have stressed the possibility of analyzing 'thinking' from an activity theory perspective without resorting to mentalism or cognitive categories.[21] Studies of molecular biologists show that at least some 'thinking' in this area appears to be accomplished through talk between scientists,[22] and a similar observation in experimental high-energy physics has prompted one author of this paper to consider 'discourse' rather than scientists as the epistemic subject in this field. In the present context, collaborative manipulation has to be seen in the context of physicists' tendency temporarily to lose their battle with theoretical objects. In these cases, the determinate path of a deconstruction method is first supplemented by non-determinate policies involving clues and guesses, trials and tricks. These policies of overcoming obstacles are aided by the inexact compass of experience and a repertoire of landmark examples which point the way. But obstacles never really disappear during these attempts. Instead, they vanish from sight temporarily, only to turn up again at a later point in unexpected places. In the native vocabulary introduced before, physicists get stuck.

"Getting stuck" is expected in the investigated field, and countered by recruiting other scientists into theoretical projects. Theoretical physicists in the area studied accomplish and publish most work collaboratively, in groups of two to four participants.[23] The collaborations also have another motivation: the need to recalculate results from a different angle, or, even better, by another person. Since empirical 'testing' is not available as a method of self-conviction, other scientists "check" the results. The collaborations physicists enter into with other physicists have the character of pacts that strengthen them in their concrete struggle with obdurate objects. But they also have the character of friendships, which sustain the possibility and recurrence of a pact.

What Theoretical Physicists Do: The Literal Story, and a Triangle of Relationships

What do theoretical physicists do? What kind of problems do they face, what kinds of outcomes does their work have? Most theorists encountered were engaged in the "construction" of models, or they remained within the frame of a particular model or theory to compute certain physical quantities. The outcome of these computations are expressions for physical quantities represented by numbers, plots, equations and the like. The "cohomology computation" which we will pursue in this paper is a problem of the latter kind. It consists of determining the "physical states" of a given model; in the case studied, the models are various quantum gravity theories. "Physical states" are basic operators of the model; they correspond to measurable physical quantities such as momentum, energy or magnetization. Physical states can be used as input in computations of physical factors measurable by experiments, such as scattering amplitudes. However, most models in the field investigated (that is, in "theoretical theory"), among them quantum gravity theories, are still too 'simplistic' to relate to experiments.[24] In these cases, theorists focus on the underlying model itself which they attempt to "understand" better by deconstructing the model. The knowledge of physical states is an important ingredient in this process of understanding:

"If some time we want to compute things in a realistic string model, this will have to be done approximately along the same lines; however, it will be much more complicated. So if we would understand this simple system in all aspects, then maybe we will understand better how to study things in a more complicated system."

The process of construction, computation-deconstruction, and understanding has a circular — but highly non-trivial — character. For the case investigated, Figure 1 exemplifies the circle.

Consider now the problem of "doing a cohomology computation" which we will follow in this paper. What kind of problem is it? How are the physical states determined? One of the theorists involved in the computation described the problem as follows:

"You have an operator, you have a space and you calculate the cohomology for this operator on the space."

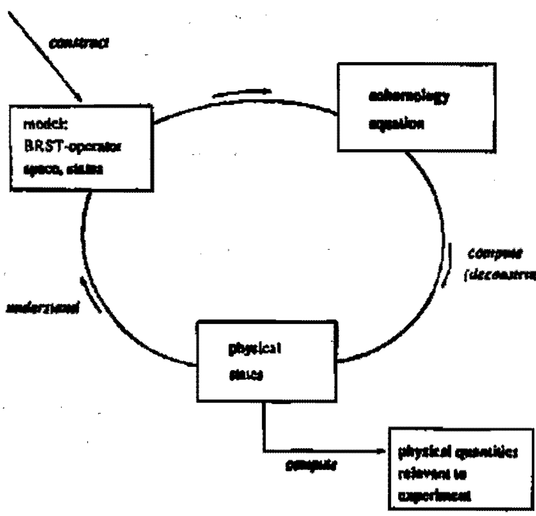
This statement might also have come from a mathematician: "cohomology" is a mathematical term, a very general concept in algebraic topology. Cohomologies can be computed "by a lot of abstract techniques" that were developed in mathematics (among them the "spectral sequence technique" which we will discuss later). What transforms this 'mathematics problem' into a problem of interest to (mathematical) physicists is its elements — the "operator" and the "space" mentioned. The operator (in our context: the BRST-operator) and space (for example a Fock-space) characterize the physics model which is explored.[25] In addition, the result of the computation can be interpreted as the "physical states" of the underlying model.

Consider now what a cohomology computation involves. If we simplify matters greatly,[26] it consists of solving an equation of the kind

$$Ax = 0.$$

A is an "operator" (the "BRST-operator" characterizing various gravity theories),[27] x represents an infinite number of "states" which

FIGURE 1
Construct — Deconstruct — Understand



The cohomology computation on which the paper is based covers the circle shown: the *computation* of the physical states which are needed in order to better *understand* the underlying model.

are the elements of a particular set called "space". The operator acts on each state and transforms it into another state. For a given operator, the problem is to find a particular subset of the space, namely those states which the operator transforms into zero:

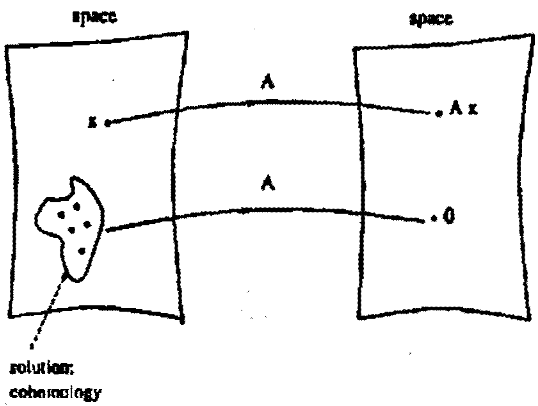
$$x \xrightarrow{A} 0.$$

The total of all states satisfying this requirement is then called the "cohomology" of the operator, and each state in the cohomology is called a "physical state". The requirement that each physical state be transformed by A into zero can be stated in the form of the above equation, $Ax = 0$ (see Figure 2).

The problem consists in solving the equation. The solution may appear obvious, but it is in fact not just $x = 0$. To see the obdurateness of the equation, consider A to be a matrix and x to be a vector, both with an infinite number of elements: A does something much more complicated than simply multiplying x by a factor (it transforms the states), and JC is not just a number but an object with a complex structure.

The point to emphasize is that the original equation hides much more than it shows.[28] In fact, the equation conceals the complexity

FIGURE 2



The operator A transforms an arbitrary state x into Ax. States are called "physical stales" when they are transformed into zero. The complete solution, i.e. the cohomology, is given by the set of all physical states.

and internal environment of all components, their difference in kind, and the fact that the equation, by proposing an equality, legislates a requirement that has to be fulfilled. In looking at the internal environment of the equation, at the BRST-operator, the space and the states, one can begin to see how physicists may need to labour for an "understanding". In the case considered, this understanding consists in finding out which states fulfil the requirement of being transformable by A into zero.

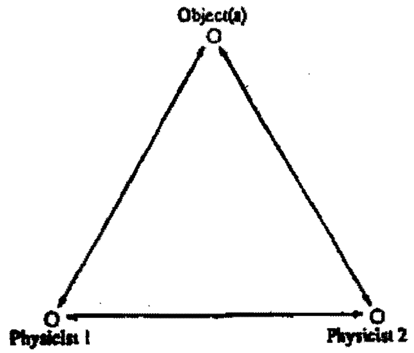
The search for an answer to such problems can also be considered in the light of the relationship between a physicist and other physicists, and between physicists and disembodied objects. What physicists do 'literally' to a computation is embedded in emotional attitudes, reactions, motives and an evaluative vocabulary of distinctions and classifications which point to such relationships, even if only in fuzzy and vague ways. To begin with the objects, what physicists seek is some sort of an alignment with the objects in question which allows them to convince themselves that they "understand". On the object side, enough independence appears to be retained for them occasionally to strike back, to withhold understanding, and perennially to suggest and to enhance further work. An alignment comes about through an attempt to engage the object, for example the above equation, in a computation. Engaging an object is not usually a straightforward process. As indicated before, theoretical physicists continually run up against the hardness and obdurateness of a disembodied object: they need to live up to it, work through it, prove themselves against it, extract their own brilliance and sense of self from it. Thus, while the physicists, who are matches for the hardness, work on new theories, the hardness of theoretical problems seems to work on them.

Consider for a moment the vocabulary in terms of which physicists talk about computations. Working toward a solution does not mean simply doing a calculation, but finding ways out of being "stuck". Physicists "get stuck" many times while doing a computation. But not only do the physicists get stuck: so does the computation. Physicists and computations are stuck together against the resistance of an equation. In the beginning of a computation, the object asserts itself forcefully, and ways must be found for physicists to "gain control". Gaining control rather literally means reconfiguring the object, through the process of deconstruction. The equation is changed by becoming "reduced", "converted", "divided up", "cut", "split" or "decomposed". "Reducing" or "converting" is done by "cutting" or "splitting" the computation into smaller components, which have to be "combined" or "recombined" later to arrive at final results. The concrete form of such deconstructions will become visible in subsequent sections. While physicists try to "reduce" an overbearing equation to manageable size in order to prevent it from getting "totally out of hand", they also expand the equation into new elements which can be rearranged, substitute other equations for it and supplement it by studying exemplary cases in other contexts. Physicists' "cutting" and "dividing" vocabulary conceals the expansionary character of the actual deconstruction. It does not address the heterogeneity created (the equation is not only expanded, it is surrounded by detour sequences, variations, model cases, and so on), and the complexity that recurs on other levels.

Turn now to the scientists engaging with an equation. Alignment first requires reconfiguring the entity with which the alignment is sought. But it also involves, in the field considered, a reconfiguration of scientists, who are edged into collaborating with each other in their struggle with equations. In this most "theoretical" of theoretical thinking sciences, the alignment physicists seek with objects is frequently extended into a triangular relationship: a second theorist (and frequently a third) join forces with the first. The 'pact with objects' requires a pact among subjects (see Figure 3).[29]

In this paper, we characterize the theoretical physicists studied as

FIGURE 3



Schematic rendering of a coalition between collaborating subjects in relation to an object.

a tribe of collaborators sharing objects of understanding, and — if you wish — of desire, but we do not focus upon relationships between subjects. Suffice it to say that these relationships can be analyzed along the lines of their rhythm, their sequencing, their logic and dynamics. Theoretical physicists seemingly learn how to collaborate in early contacts with their thesis supervisor and with fellow students, as they learn how to handle objects. Like these teacher-student relationships, the 'thought alliances' physicists form later also contain an element of consultation. However, thought alliances are also sustained by and embedded in 'friendships' which develop from physicists spending time together in one place. Friendships, too, are marked by the parallelism of alignments — by the conduct of lives mostly spent in physically very different locations (after having stayed at the same institute or department for some time), but continually 'paralleled' through electronic mail. Through electronic networks, the exclusive, singular and intense relationships that physicists purchase in their 'pact with objects' are extended far beyond their desk — in regard to distances breached and people reached. They are also continued through (and enfolded in) the lighter and more distant understandings of the non-consummated friendships of paralleled lives.

The Story of a Collaboration

Consider now the story of one collaboration, witnessed at CERN, and how the collaborators embarked upon the cohomology computation analyzed in this text. An interview conducted in June 1992 with the mathematical physicist N, a research fellow in his second year at CERN, became the first step of our involvement in the current project. The interview was based on a recently published TH-preprint (CERN's Theory Division issues its own preprint series), written by N and two collaborators of several years, A and P. In the preprint, as well as in several earlier publications, the collaborators had documented their expertise in doing cohomology computations. They had computed cohomologies for a variety of theories, using different methods: one was the "spectral sequence technique" which will come come up again later; the other was an approach developed by two mathematicians, which we explain below as a 'detour'.

In late summer 1992, N and his two collaborators had started working on yet another cohomology computation which was to become the focus of the present study. They used a similar set-up and similar techniques as before, but addressed a different theory: W-gravity. Their interest in performing the computation for this particular theory had been triggered by the fact that the "right" BRST-operator for W-gravity had become available; it had been constructed by G, another theorist working at CERN, and his three collaborators. N's collaboration drew additional motivation from the brand new results presented by yet another group of colleagues, to which N was alerted by a preprint available from the "bulletin board", or "hep-th", as theorists call it for short (it is the theory section of the "High Energy Physics E-print Archive").[30] This last group had obtained some explicit results for a related problem by extensive computer calculations. Before the preprint had come out, N explained, *"we have had some ideas but not that clear. We certainly had nothing to check it against.... We were not confident enough, and the techniques which we tried to use didn't seem to work or in any case not without hard work."* The new results put N and his collaborators under considerable time pressure, since the problem at this point seemed attractive to several other groups of theorists from all over the world. For example, the above mentioned CERN-theorist G and his three collaborators were interested in computing physical states for W-gravity. After they had succeeded in constructing the corresponding BRST-operator, they now wanted to *"show that they (the physical states) exist analogously to those in previous work"*. In contrast to the attempts of N and his collaborators who were looking for a complete classification of the physical states, G and his collaborators were interested in constructing some explicit examples (see below). They therefore heavily relied on the computer, using "symbolic manipulation programs" (that is, computer programs manipulating formulae and not just numbers), whereas N, P and A at that time did not employ the computer as a computational tool.

N and his two collaborators were located at different institutes on different continents, and all interaction at the time proceeded through electronic mails.[31] The three physicists frequently exchanged several mails per day. The mails included statements of current progress or difficulties, discussions of what to do next and communications about what priorities to set, or what to give up. G and his collaborators were also employed at different institutes. G and N worked at CERN, and two of G's collaborators worked at the same physics department as one of N's collaborators. Although the two collaborating groups would not discuss their progress with each other on a regular basis, they were still aware of what the others were up to.

The deconstructivist procedures presented in the next sections were extracted mainly from data obtained by following N, A and P in doing the cohomology computation for W-gravity. These data were supplemented by a study of other computations N and his collaborators had worked on before, and by information obtained from G about his work on W-gravity. As already mentioned, we had become involved in the project in June 1992, and followed its progress until N left CERN live months later. At this stage, not all problems were resolved but the line of calculation had become clear, with some technical problems left to solve. While a paper based on the work described in the following was published in 1993,[32] some of these problems have still not been resolved today.

Deconstructi©n and its Epistemic Profit

Let us focus, in this and the next three sections, on the desk, and start by asking what a working deconstruction of a cohomology equation $Ax = 0$ consists of. When computations and the objects they involve become complex, special techniques can be used to reconfigure an equation. For this purpose, physicists use the "spectral sequence technique" (in the following referred to as 'SST'), recently taken over from mathematics, where it developed in the 1940s in the field of "homological algebra". The SST has since become a standard technique; its first step consists in expanding the objects, the second in reshuffling their components, and the third attempts a sequential reordering of them.

Deconstruction through expanding the object: The operator A and the states x are thought of as composite objects, which are built up through the multiplication and addition of more elementary operators or states. The first step in deconstructing these objects consists in "expanding" A and x into their components in such a way as to render the objects' constructedness explicit. For example, A might be:

Here a "mode expansion" has been performed. The modes b_n and c_H

— labelled by their energy levels — are common building blocks of both A and the states. Often, the modes L_n and L'_n are themselves composite, and have to be specified in supplementary equations.

To simplify matters (the following procedure holds irrespective of the energy-labels of a mode), we will from now on simply speak of a term *bcc* instead of, for example, $b_{m+n}c_{-m}c_{-n}$.

Reshuffling components through building in degrees: Deconstmction through expanding A and x is but the first step in dealing with a problematic equation. To 'simplify' the equation, another step is needed, one that attaches "weight factors" to each component. A weight factor is just a number (also called a "degree" or a "grading"). Assume, for example, that one of the terms that make up A consists of something like the product *bcc.* A degree is now attached to *b,* and to *c* (how these degrees are chosen is discussed below), say for instance: degree-1 to *b* and degree 1 to c. Since degrees can be summed up, the assignment of degrees to the basic components results in the attribution of a degree to the whole term *bcc,* which, in the case discussed, is $(-1) + 1 + 1 = 1$. Each term in A, and correspondingly in x, now has a degree attached to it. In a further step, all terms with the same degree are joined together, such that A and x are now made of components of different degrees. The grading 'reshuffles' the order of the original components, 'regroups' them, and gives each component a weight factor. Through what physicists call "building in discrete parameters", a preliminary ordering (or a reordering) is achieved.

Sequential reordering: The lowest allowed degree attributed to a term in A is zero. A can therefore be rearranged as the sum of components of sequentially increasing order:

$$A = AQ + AJ + A_2 + \ldots$$

This sum is chosen to have only a finite number of terms, which can vary from problem to problem and from grading to grading. The index attached to the components labels the degree of each particular term. States x are sequentially reordered in the same way. As a result of the grading procedure one has now deconstructed A and x into an ordered sequence of components.

What is the epistemic profit of the above deconstmction and reordering of objects? How does deconstmction, understood as the expansion and re-evaluation of components, transform the problem? Deconstruction distributes and dissimulates the hardness of the attempted computation. If you think of the hardness as a fixed property encaged in the original equation, then deconstruction sets this hardness loose. It allows the work its resolution requires to be shared by many objects and steps, and it allows the hardness to be moved from one object to another as the calculation proceeds. But not only has the problem been transformed through its decomposition into components: so has the relationship between physicist and object, to the advantage of the physicist.

The obtained rearrangement allows for the sequential input of solutions already achieved, of hardness already conquered. Not only can smaller problems be solved more easily, but each solved component strengthens the physicist for the next step. Instead of the original equation $Ax = 0$, one now has to solve a sequence of equations, which are graded in the sense that the result of the first step — the solution of $AQX = 0$ — is used as input to the second step — the computation of $A_{\prime}x \sim 0$ — and so forth. When objects are deconstructed through the SST, the computation is not only deconstructed into a sequence of calculations, but the intermediate results are gradually narrowed and brought nearer to the final result in the sense that each step results in a reduction of possible solutions for the next step. For example, the first step might leave you with six physical states; the second step rules out two, leaving you with four remaining states whose explicit expressions were slightly changed in the course of the second step; and the third step no longer changes the number of steps. In general, a finite number of steps is sufficient to solve the problem. The final solution — the cohomology that was looked for — consists of the outcome of the last step. The number of required steps depends on the particular problem (that is, on the form of the BRST-operator and the choice of the space of states) and the grading chosen. Some problems can be solved by just one step. Physicists say that the sequence "converges", meaning that successive terms give "finer and finer approximations" of the result they were looking for. The sequence is said to "collapse" when the last required step is reached.

Second Expansion: Heuristic Rules for Shifting the Hardness

With the above deconstiuction of problem and object, each step has become easier to handle than the original. However, accomplishing a working deconstruction is itself problematic: the selected components are only willing to cooperate if they are aligned with each other in such a way that *every* step is manageable. The difficulty can be characterized in terms of a 'trade-off' process through which amounts of hardness are shifted from one step to another. The shifting has to be done in such a way that all steps remain or become solvable. Thus, the first step in a deconstruction can only be simplified at the expense of the following steps. Since an appropriate distribution of hardness has to do with the grading chosen, several heuristic rules are invoked to help achieve a well-aligned series of steps and a well-balanced distribution;

- Choose a grading such that you need as few steps as possible, since the first step is the easiest, and each further step becomes more complicated.
- Choose a grading such that the lowest term (that is the solution of the first step) comes closest to the cohomology you want to calculate.
- If you have several gradings that require the same number of steps, choose the one for which the calculations are simplest.

All these rules sound "fuzzy", as a physicist said, but this fuzziness represents the complexity of the problem: There is no such thing as a "straightforward" path towards the end result. Without proceeding through variations as one "tries out" different degrees, no "right" way — that is to say, no way that "works" — can be found (see also the section on 'variations', below).

Besides heuristics that help in appropriately distributing the hardness of the problem, there are 'constraints' which have to be met and which help to achieve a suitable grading. There are, on the one hand, gradings that are "legal" and those that are not, and gradings that are manageable and those that are not:

- *"Legal" gradings:* The deployment of the mathematical technique requires certain mathematical properties to be fulfilled, like the above-mentioned rule that the term with the lowest degree in A must have degree zero. No negative degrees are allowed.
- *"Manageability":* Other requirements hold for the sake of manageability. For example, one does not know how to deal with a zero-degree term in A that consists of three basic components such as the term *bcc* discussed before. These terms are not

> forbidden in a mathematical sense. If, however, a grading is chosen such that the lowest-order term is of this type, "you are stuck since you don't know how to do the first step".

Through physicists' grading rules, the hardness of a problem can become manageable. Deconstruction, however, does not always work. Certain deconstruction steps may turn out to be impossible, ineffective within a particular problem context, while they had succeeded before in another context. Other strategies may lead to unprecedented difficulties. In the course of constructing the deconstruction of objects, there is no *a priori* way to avoid "getting stuck".

Third Expansion: Detours, Tricks and an "Ansatz" in Response to "Being Stuck"

> "I am stuck on computing the BRST of the W-charge — basically I could not see any way of avoiding the hard work. If you consider looking at such a problem of any relevance, please do so,"
>
> (e-mail 12; P to N on 24 September)

Theorists expect that new obstacles will 'pop up' all along the first road chosen towards a solution as a typical feature of their work; and they do. A further challenge in concluding a pact with objects is to find out how to get around these obstacles, either by clearing the path or by looking for an alternative route. Physicists announce the challenge by declaring themselves "stuck". How are physicists caused to be "stuck" by objects? By being forced back, or by being confronted with an overcomplex situation in which they lose their grip.

"Being stuck" can mean reaching a point where you have to go back, for example to try out degrees through doing a 'variation' (see below). But theorists also become stuck when they lose confidence in the path they pursue, and when they are not sure which alternative to choose or what the alternatives are. Physicists might lose confidence when they do not see how to "avoid the hard work". They might be able to proceed with the computation, but "it's a mess", they have "no control over it except by writing notebooks full of equations". Too many steps are required and too many terms have to be included so that "computations really get totally out of hand". When physicists are stuck, the control promised by a particular avenue is lost and they "run out of ideas" on how to regain it.

"Being stuck" is a description whose application depends on the physicists and on time: on the expected effort required for solving a particular problem, on the effort they are willing to invest, on the personal style of the researcher — some theorists are more persistent and acknowledge that they are "stuck" later than others. It also depends on predicting the future. Alternative paths sometimes look unmanageable, whereas later they turn out to constitute a way out. "Being stuck" builds reflexivity into hard problems: it forces theorists to reconsider their strategy for solving them. Efficiency, effort and time have to be rethought continually during the course of a computation — unless, of course, one deals with "straightforward" problems which, in theoretical physics, are rare.

Although the SST is used as a standard tool in the course of a cohomology computation, there is no standard recipe for its application. If physicists start from a different BRST-operator, a cohomology computation based on the SST may become impossible to handle. In a case observed, the different grading requirements (see above) turned out to be incompatible: either the zeroth-order term A_0 was of the type *bcc*, and thus not "manageable", or the lowest degree term "illegally" assumed a negative number. The computation was "stuck"; the "direct analysis", using the SST as described, did not lead to any result.

Alternative strategies — indirect ways — had to be explored, of which two succeeded independently in resolving the issue. The first was a detour strategy, the second a strategy of exploration by "doing examples" followed by a "trick". A third way of dealing with the complexity of the problem consists of a reverse strategy: instead of refining the SST, it starts out from the problem in the most direct way and then uses an assumption — an "ansatz" — to map out the structure of a possible solution.

The Detour

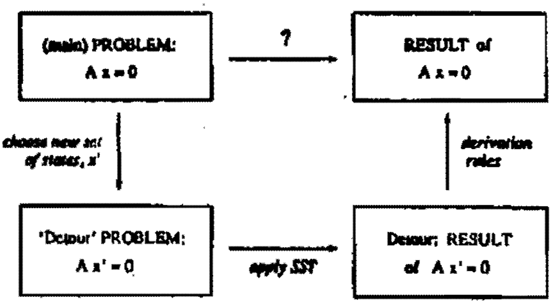
'Go back one or several steps, then try a path to get around the difficulties you could not resolve before', is the motto of this approach. In the case of 'our' cohomology computation, physicists went back to the initial equation as a starting point for a detour. The detour was inspired by a common mathematics approach to solving similar problems: the new path consisted of selecting a different set of states (that is, a different space) among which solutions of the cohomology equation were sought. The computation for the new space again requires applying the SST, while the merit of the detour lies in the possibility of finding a grading that now satisfies all requirements. The actual SST steps can then be solved with very little effort. However, the new physical states in a detour are not those one is looking for, and hence a problem remains: can the cohomology of the initial problem be deduced from the result of the intermediate step? If this is possible, the new computation plus the deduction rules can be substituted for the 'direct' computation of the cohomology in which one got stuck. Detours simplify the activity of finding a grading and of solving the SST steps. Nevertheless, they do not dissolve the hardness of the problem but continue to shift it from one object to another. The hardness of the initial problem is replaced by other potential difficulties, for example by the difficulties of deriving deduction rules that require physicists to solve new subtle mathematical problems. These cannot be considered here (see Figure 4).

Detours are context-dependent: What is considered to be a detour for mathematical physicists might be the "natural way to proceed" for mathematicians, as is the case in the discussed problem.

Tricks and Further Work on Objects

Up to this point overcoming the obdurateness of physics objects meant expanding them into still hard but more congenial pieces, or

FIGURE 4



If the main problem cannot be solved directly, a detour may be accessed. After solving the new problem of the detour, derivation rules are needed to deduce the wanted result from that obtained.

substituting one kind of obdurateness and hardness by another. In concluding their pacts with objects, however, physicists also resort to tricks — and they have developed strategies that improve their chances of finding a trick. In one of the cases observed, the computation got "stuck" because no suitable grading could be found. The physicists then turned to "doing examples" (see below) to come up with ideas for a practicable path through the computation. These ideas are called "tricks" when their application substantially reduces the hardness and obdurateness of a problem.

In the case analyzed, a gloss on the character of the end result — provided by "doing examples" — made physicists recognize that a suitable grading might be found after inserting an intermediate step in the middle of the SST path. The extra step came between 'expanding the object' and 'building in degrees' and consisted in 'transforming' the object's components. The transformation mixed the initial components; it folded them into each other in such a way that each new component incorporated pieces of several initial components. After the transformation, the SST worked: a grading had been found, which allowed the remaining steps to be solved without further difficulties. The computation had been stuck for more than a month. Once the extra step was performed, the computation could be completed within a few days.

Physicists called their intermediate step a "trick" since it reduced the complexity and hardness of the calculation by a large amount. With tricks, the hardness of an equation appears to be neither shifted nor redistributed, but dissolved. Tricks are 'simple' devices in the sense that they are usually one-step procedures: a single step introduced in the computation managed (surprisingly) to break up the computation. If a newly developed trick turns out to be applicable to advance other computations as well, it might become part of the common trick repertoire shared by the field.

The Explicit Construction of Solutions

Instead of applying a deconstractive technique (like the SST) to solve the cohomology equation, a computer program can be used to detect physical states starting directly from the initial ingredients, the BRST-operator and the states. The program can check the states, arranged according to increasing energy levels, and select as solutions those that are "annihilated" by the BRST-operator. In practice, however, the observed problem is of such complexity that only a few physical states can be constructed in this way. The number of terms which have to be considered for each check increases dramatically with higher energy levels. Today's computers are powerful with respect to the amount of information they are able to process; nevertheless, they cannot handle such complexity. As a consequence, solutions can only be "constructed explicitly" — by "hand" or computer — after the complexity of the problem is reduced.

How are the hardness and obdurateness of an object attacked to negotiate a computation in this case? Constructing solutions explicitly relies on a 'guess', called, in this context, an "ansatz". An ansatz is (mainly) of instrumental or technical value. It allows the number of potential solutions to be brought down by an assumption about their structure that can be "plugged" into the cohomology equation. The new input is provisional; its goal is to advance the computation by acting as a stepping stone which enables theorists to get on with the work. The result relies on the assumption but is not itself provisional, since a false assumption as ansatz will lead to a contradiction and rule itself out. The ansatz is thus legitimized in retrospect.

Consider an example: the explicit construction of physical states relied on an assumption which was derived from an analogy of the problem with a similar one studied earlier. In the earlier case, the physical states were arranged in a diagram whose pattern was determined by a particular "symmetry" of the problem. When studying the new problem, the BRST-operator changed, and with it the symmetry, so that an adjusted diagram proposed itself as a guideline for the search for physical states. The structure of the diagram could be translated into an ansatz, which served the purpose of prestructuring the desired solutions of the cohomology equation. An ansatz is like a 'skeleton' formula, it presumes the form of the solution and some of its properties, leaving space for the particularities of the solution's 'body', the concrete values of the parameters. In this way it allows a reduction of the complexity of an object by provisionally eliminating some of its variables. Thus the problem becomes solvable either by hand or by computer, as in the observed case.

Is there deconstruction in an "explicit construction"? An explicit construction does not rely on a deconstructive technique such as the SST, but the computation is deconstructed by other means. These include the "inspired guess" which is translated into the ansatz. In order to "split the problem into the smallest possible parts" to reduce the complexity of an object, an ansatz must be very specific. It also varies with varying parameters which are related to the energy level. An ansatz can therefore be adapted to certain energy ranges by changing some of its internal structure. In this way a system of 500 equations with 500 unknowns can be transformed in one of 500 equations with only 50 unknowns. Such a system either leads to a contradiction (because of "overdetermination"), in which case the ansatz was wrong. Or it is essentially equivalent to a system of only 50 equations with 50 unknowns, which constitutes a considerable reduction of the hardness and obdurateness of the problem.

With an ansatz approach no solutions can be found which do not have the structure the ansatz proposes. Besides, one never knows whether all solutions have been detected, since infinitely many states have to be "checked" in a cohomology equation. The considered ansatz is therefore likely to catch only a subset of the solutions one is after.

"We construct these (physical) states — of course these are not all of them but some, so to say the most obvious ones —just to show that they exist analogously to those in previous work .... Since a general proof, I believe, is extremely complicated in this case . . . it's simpler to just construct them explicitly here, as examples."

Fourth Expansion: Three 'Hidden' Procedures

The pact with objects can be attempted through a first-order deconstruction, and, if this is hard to achieve, through a second round of detours and tricks. This section introduces a third category of devices — those which can be deployed toward both first-order difficulties and second-round problems — and which are hidden in publications. In a third run through theorists' practical work of dealing with computational objects we will focus on three strategies: variation, "doing examples", and model objects.

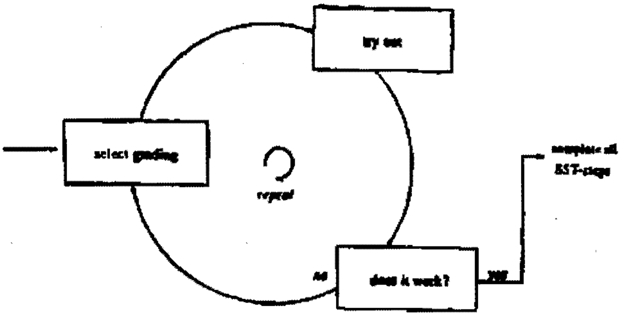
Variation

Techniques like the SST do not specify how some of their requirements are to be met. Variation is used in many contexts in which such a technique has to be deployed to a concrete situation. Take the example of 'how to find a suitable grading'. This opens the door to the deconstmction of the computation by the SST. Variation, then, provides a strategy to find the right key to the door. It means trying out different keys, making a selection among them, and inserting the chosen one into the keyhole to see whether it will turn. Variation also implies that this process of 'selection' and 'trial' is repeated until a satisfactory choice can be made.

Successively searching for alternatives seems "straightforward". In practice, however, the straightforwardness of the 'selection-try out-repetition[1] scheme is broken up by several complications. For example, there might be more than one suitable grading, or there might be none at all. Not all gradings can be explored because not all possible gradings are known, and hence 'completeness' is an unresolved issue. The answer to the question of whether a particular grading works is itself problematic, since checking tends to cost considerable time. The effort a physicist is willing to concentrate on a particular grading depends on how promising it seems, which varies over time: giving up on a grading at one point does not imply that you are done with it once and for all. For all these reasons, working through the cycle 'selection-try out-repetition' is patchwork, the patchwork that results from pursuing a segment in the cycle, giving up on it, choosing a new starting point, perhaps picking up an older segment later — and never knowing whether all alternatives have been found and explored. In an attempt to minimize the number of cycles, physicists do not make first choices for a grading at random, but based on experience. Having handled a similar problem in the past might help in making a "clever choice". But if the first choices are not successful, further selections proceed "in the spirit of trying everything once". Searches become more random, variation turns into 'blind variation', and some gradings discarded in the past are reconsidered and explored in more depth.

Which selections are made is up to each physicist. A 'wrong[1] choice discredits itself without causing any harm, since the attempts are carried out in an immaterial world where all actions are reversible and the playgrounds infinite. However, the time and the effort physicists consider appropriate for dealing with a particular grading are a finite resource. Nevertheless, physicists engage in some "playing around" with gradings which are afterwards discarded as a means to increase their experience and to teach them how to recognize more efficiently which grading solves a problem.

FIGURE 5
Variation Cycle



A grading is selected, tried out, and if it works, all steps of the SST are completed. If the grading is not manageable, the procedure is repeated and another choice for the grading made.

Variation is a 'direct' strategy: the cycle of 'selection-try out-repetition' fits in the computation at the 'level' of the problem (as illustrated in Figure 5) with the 'right'[3] grading as the 'direct' and necessary ingredient for all further steps of the SST. Variation does not require or use decomposition. Nor can it be avoided by the use of a systematic procedure for finding a 'working' grading. But the grading once found sets off the deconstruction of the computation by means of the SST.

Examples

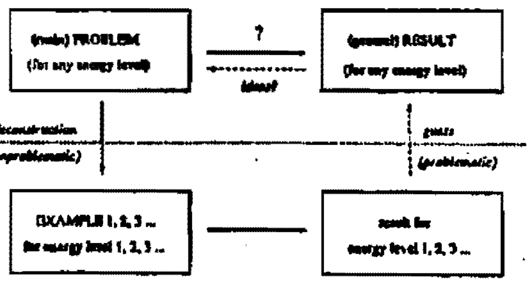
Examples turn up in many different contexts in theoretical physics and serve various epistemological purposes. For instance, we showed above, in discussing the use of ansätz, how examples of physical states could be constructed explicitly in cases where the complete solution is unattainable. Examples are virtually omnipresent in the work of physics computations. In the following we will be concerned only with their potential for advancing a computation by providing a gloss on its outcome. *"At the moment I don't have a good idea how to make progress, so I'm playing around with examples to see whether I can possibly discover a 'hidden structure'".*

In "doing examples" the problem is considerably simplified, at a cost: one obtains only clues and hints instead of the full solution. In contrast to identifying the 'right' key to a problem by variation, a key which potentially advances the computation by a 'real'[1] step, doing examples can only provide 'virtual' progress: a new step might be hinted at but is never already performed. Doing examples transforms the problem into a related simpler one which dwells on a lower level, on a level where the objects are reduced to special cases. Two transitions are necessary: the step to the special case, and after examples have been worked out, the step back to the original computation. The latter step is the most problematic part of this heuristic strategy. For its purpose, physicists compare several examples from which they "guess" a solution of the general case.

The sketch of "doing examples", illustrated in Figure 6, is somewhat similar to the one for taking a detour. With detours, however, the step back to the main level of the computation is a rigorous deduction, so that the detour as a whole can substitute for the part of the computation which could not be solved on the main level. With "doing examples" that step is a guess; it cannot stand on its own and represent the main path.

In general, theoretical physicists grade different steps in a computation as more or less "straightforward", depending on how "hard" or problematic they are — with a straightforward procedure, theorists do not get stuck. However, different steps or parts of the computation can be straightforward or unproblematic in different ways. For example, variation was a straightforward strategy in principle, but not in practice. With "doing examples", we have to

FIGURE 6



The results of several examples are compared in order to arrive at a guess for the general result. Physicists hope that the guess will provide them with ideas on how to solve the main problem.

distinguish "straightforwardness" in a technical sense from the epistemological sense. Doing examples can be straightforward in that the decomposition that provides the objects as input is rather 'trivial', and in that no technical intricacies are involved in calculating the examples. However, whether the performed examples are of any relevance for 'unsticking' physicists with the general computation (by providing a guess for its outcome) is another matter.

To obtain a guess, physicists usually work out more than one example of a particular kind. Repetition, however, differs from the one in a variation cycle. The variation cycle has to be repeated every time a grading ends in a deadlock until one is found with which the cycle can be moved towards a solution. Working through several examples, on the other hand, has the purpose of building up towards a more and more complex pattern, which increasingly resembles a general solution. Usually three or four examples of each type can provide an idea for the "behaviour" of the general solution.

For obtaining a productive guess it may not be enough to do several examples of only one kind. Physicists may also have to vary the type: they may have to repeat the procedure — 'deconstruct-solve-guess' — for different deconstruction grids until they find a productive guess. Although it is not *a priori* clear which grid can be made productive, some of them constitute an 'obvious' first choice. There is, for instance, the 'trivial' deconstruction, which consists of converting a parameter into a number, usually an integer. This deconstruction starts off with rendering the constructedness of the object explicit, and continues by choosing a component to "cut". Cutting means substituting an abstract quantity by a concrete value, different for each example. This deconstruction is not as elaborate as the deconstruction performed by the SST. When dealing with examples, deconstruction may mean nothing more than the unfolding or fanning out of an object along a certain parameter.

Consider an illustration: an object might be specified by a sum of n terms, where n is an arbitrary integer. The first and most obvious example would consist in choosing $n = 1$ and then performing the computation for this single-component-object instead of doing it for the general sum. The second example would start out from the sum of two components as a new object, and so on. Thus the obvious choice for a sequence of examples is to begin with $n = 1$ and to continue by increasing n.

As mentioned above, the "trick" that resolved the problem of finding a suitable grading was discovered by doing examples. In this case, the deconstruction grid used for the examples was slightly more elaborate than in the 'trivial' deconstruction, since the parameter along which the unfolding took place was not explicitly contained in any of the objects. Doing examples required physicists to go back to the original equation $Ax = 0$, and to try out "the most direct way possible" — that is to say, without invoking the SST. To this end, objects had to be expanded in the same way as for the SST, for example by doing a "mode expansion" (see above). Doing the example, then, consisted in writing down the cohomology equation for each energy level separately and solving it, thus searching for physical states of a particular energy at the time. Consider the following component of A:

$$\Sigma_{m,n \in Z}(m-n)b_{m+n}c_{-m}c_{-n} = 3b_{-1}c_{-1}c_2 - 3b_1c_1c_{-2}$$
$$+ 5b_{-1}c_{-2}c_3 - 5b_1c_2c_{-3} + \ldots .$$

Examples for low energy levels are simple to compute. For instance, no term of the above sum contributes to the physical states of energy levels 0 and 1, whereas the terms 3^_,c_jC$_2$ and -3£>,C[C_$_2$have to be considered for finding the physical states of energy level 2. Examples for higher levels become more and more complex, since they comprise a growing number of components. For instance, eight more components of the infinite sum are needed to determine physical states of energy level 3. One could say that in every succeeding example a new and broader layer in a pyramidal structure is explored. In this way, physicists hope to get a glimpse of the behaviour of the general solution for all energy levels.

Model Problems

Examples can be seen as a special case of modelling. When physicists do examples, they profit from going from the main problem to less complex special cases. When they turn from the main object to models, they choose a model object that has been studied before, with which they have previous experience, an object that is well explored and understood. Naturally, the model object must contain some of the typical features of the main object, and it must be more accessible (the problem must be solvable).

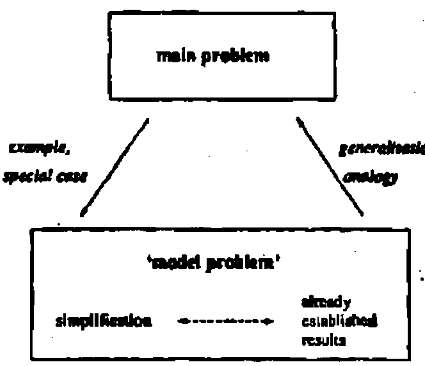
"When you construct a model you are trying to simplify the situation to

something that is computable, ... and which still keeps the flavour of the non-trivial properties of the model you want to study."

Accessibility usually also means some simplification, but an important point is the existing experience. A model object, if you wish, has already been won over; the pact with the object exists, and the question is what one can learn from it with respect to a new object (see Figure 7).

A first answer to this question is that physicists may learn more from modelling than from doing examples. These can provide them with a gloss on the nature of the result they are after. This gloss, however, is but the last step of several in the main computation, and it is not always possible to fill in the missing steps "straightforwardly" in order rigorously to derive from them the glossed result as required. In other words, with the gloss one might have the end result without having all crucial steps leading up to it, a situation not appreciated in theoretical physics. Similarly, when examples are called upon to create the opportunity for physicists to hit upon a "trick" (see above), no trick may materialize. As physicists put it: *"I know the result, but I cannot prove it."* To some extent, examples can even be used to anticipate a working procedure, by posing not only the question 'Where do I want to go?', but 'How could I get there?' — all with a simplified version of the problem. This

FIGURE 7
A Problem is Modelled



The 'model problem' has to embrace known results and/or be simpler to work with than the main problem. The model can then be used as a playground where new ideas are tested.

simplified version, however, is not sufficient to explore the question in depth.

Model objects, on the other hand, are related to the main object by analogy. As a consequence, they can be exploited in more flexible ways along the lines of their analogous relationship — as models for the object, for the problem specification, for difficult technical steps, or for possible results. One can take advantage of a suitable analogy by adapting known results to the new context. In addition, one can use the model as a playground for new attempts to deduce old results — for trying out new calculations in a safe and controlled environment. For example, one might try to find a new proof for a solved problem to test a proposal for how to solve the main problem. This work with the model as a 'laboratory' for trying out new calculations is very important and distinguishes physicists' use of models from pure reasoning by analogy or from doing 'exemplars'.[33]

Consider an illustration: the collaborators N, P and A work on a cohomology computation for a BRST-operator constructed by the colleague G and his three collaborators. P and A have not yet succeeded in obtaining a result by applying the SST directly. It is then that N sends the following e-mail message, stating some *ad hoc* conjectures for how to solve the computation by adopting the discussed detour strategy. The conjectures are formulated in analogy to a similar problem which they had solved before.

> Some very sketchy remarks:
> Suppose we are interested In computing the W-cohomology on the Fock space of two scalar fields (BRST operator of F) to prove the results of U et al. One could proceed, in complete analogy with the Virasoro case, as follows.
> Probably, in some domain of p (...), *Fp* Is Isomorphic to the (dual) Verma module (we derived Shapovalov determinants for *W* several yrs ago, rememberl, so this is something we can check/prove).
> Also assume that some version of the reduction theorem is valid (I am aware of the obvious problems in providing/Interpreting such a reduction theorem but nevertheless confident that for any reasonable BRST-operator something like this should be true),... Then we can compute the rhs by means of a resolution of C in terms of *W* (dual) Verma modules (these we also know!).
> Skipping all the details we can at least verify whether *H* ((the cohomol-°9y)) produces anything like the results of U et al. Work in progress.
>
> (e-mail 21; N to P, A on 30 September)

And ten hours later:

> My preliminary analysis of *H* seems to confirm the results of U et al. Let me give you the results for the *gh* = -3 states (*gh* = 0 operators). There are 9 in total (...) and they occur at energy levels 4, 6,6, 8, 8, 9, 9,11,11. At energy level 4 we have the identity, and at 6 the operators *x* and y of U et al. The ones at level 8 and 9 are anticipated by U et al in the discussion (sect. 4).
> More on this tomorrow.
> (e-mail 26; N to P, A on 30 September)

The model problem starts from a BRST-operator constructed by F, which can be deduced as a special case of the operator of G et al. For this BRST-operator U *et al.* had explicitly constructed physical states using a symbolic manipulation program instead of the SST. The link to the model' problem consists of translating the conjectures to the context of the model. N assumes that his conjectures hold, and tries to build up on them. He tries to deduce rigorously the physical states of the model problem in order first to check or test his ideas on a well-known system. The results are then compared with the outcomes obtained by U et al. (e-mail 26), The correspondence of the results gives confidence in the approach, and so N starts with an attempt to prove the conjectures for the general case. The results of U et al. were known from preprints and publications.

Modelling can be thought of as a device that taps into existing pacts with objects to make the experience available for a new one. In the physicists' fight against the obdurateness and hardness of a calculation, they 'virtually' join forces with larger groups of scientists.

Last Expansion: A Note on the Pact between Subjects

In the last section, the alliance between subjects — physicists collaborating on a computation — has forced itself back into our text. From physicists' e-mail fragments, one gets a sense of how the real-life interaction between objects and subjects is more detailed, more complicated, and more laborious than we could render it so far. In a sense, we would need another expansion: to delve into the concrete interaction between physicists, to see how their 'collaboration' is brought in as a resource that aids in all previous attempts of trying to align theoretical physics objects. Physicists' (e-mail) dialogue is the last — and the most universal — device. It can be brought to bear not only on unpacking the object, but also on finding detours and tricks, or on doing variations, examples and models. It is also a device that recapitulates the dissociating and expansionary character of the previous tools. Theoretical objects and problems, in addition to becoming segmentally deconstructed, expanded, detoured, varied, modelled and attacked through examples and tricks at the desk, are simultaneously divided up, partitioned, varied and 'paralleled' (worked out separately) between physicists. The structures and the rhythm of this partitioning and paralleling cannot be analyzed here. Suffice it to say that like previous methods, the alliances between physicists centre around "getting stuck[1]' — they prepare for it, guard against it, measure the degree of being stuck, and try to overcome it. They may magnify the problems of being stuck — when two or more physicists do not know how to go on. But in centring around the possibility and difficulties of getting stuck, they also 'loosen up' ideas for travels between desks and thought laboratories, the walkable and excitable minds of the thinking sciences:

> "It seems we're stuck. I have no earth-moving new ideas, but instead of keeping silent let me just communicate some loose thoughts in the hope that somebody can do something with them."

Concluding Remarks

In this paper we have traced devices which theoretical physicists use in their attempts to conclude an agreement with obdurate and "hard" objects: formal deconstruction, detours and tricks to identify a working deconstruction, variation, doing examples and modelling to assist with the first and the second type of procedure, and thought alliances between subjects. The last device not only purchases assistance in the struggle with objects, it also secures physicists' guidance and trust — trust in the pact once it seems impending, guidance and surveillance along the way, and assurance with respect to the possibility of going on when they are stuck. The pact between subjects, it seems, not only completes the deal with objects, it also supplements individual physicists' capabilities. In this sense, theoretical physics remains, with its thought alliances, an individuating science.

To illustrate the e-mail interaction on which the paper draws, we present three extracts: (1) an e-mail sequence discussing on what topic to concentrate in the future, dating from the beginning of the observed computation; (2) a sequence of questions, suggestions and comments concerning the choice of a suitable grading; and finally (3) some declarations of "being stuck".

The Beginning of the Computation

N to P, A on 25 September:

"Well, one thing this vacation did for me is to provide me with new energy to start some new project. But what? It seems (several people independently pointed this out to me) that the latest 'hot issue' (...) is the construction of free field representations for quantum groups ..."

A to N, P on 28 September:

"I would like to decide over the next few days if we actually can do something on W-brst, or whether we should move on to bigger (?) and better (??) things.
I saw N's suggestion, although I haven't looked at these papers — no doubt they are all sl(2)? ..."

P to N, A on 28 September:

"I have printed out the paper of U and comp on BRST for W-gravtty——
I haven't read what they have in detail, but an extensive use of Mathematica sounds scary. If we want to do anything in this direction we must do it VERY fast. P"

N to P, A on 29 September:

"We should be able to improve considerably on what has so far been written on W-cohom; let's crack It this week??!! — N"

The Search for a Suitable Grading

P to N, A on 2 October:

"Should we expect cohomology at finite or infinite number of momenta?

There is a term in the BRST which is 7_0 times a third order polynomial in p's, which it seems to me can kill almost everything.
Anyhow, there seems to be a convenient grading that splits this operator into manageable pieces. Define a double degree *ip,q),* where pfy) = 3 = -p(fj) and zero for the rest, while p Is 1 for c and a_{+1} and 3 for 7, and - that for the other fields. Then d_{00} is the old gravity friend, and d_{33} is 7_0 term noted above. Things in between are simple. I am sure I should be able to get out the result this way. Of course, this is not too helpful for the real stuff, ie w-gravity + matter..."

A to P on 5 October:

"Perhaps more hopeful Is to lift 7, lose the *yW* and play with Vir type BRST. Then try to bring back IV bit by bit So I'm thinking about that again. Beyond all this I have the W_Q problem lurking around which I don't really handle yet. Somehow it's got to be important though ... A"

P to A on 5 October:

"My hope is as follows: grade the BRST by number of 7's. Then the 0-order is standard Virasoro, the 1st order is *yW,* and the 2nd is junk. Now *yW* is niipotent on the cohomology of the Virasoro. At this stage one can still try to split into *W+* and *W_*. If things converge fast enough, maybe one can conclude something, but not me."

N to P_s A on 7 October:

"I'm not too optimistic about the W-cohom derived from the Vir-cohom by SS-ing, but since we cannot do better at the moment it's worth pursuing. (I still have no idea how you intend to Impose W_Q = 0 explicitly, or maybe you don't want to consider (full) relative cohom?) — N"

P to N, A on 13 October:

"I have still problems in mapping out a strategy of dealing with the W-algebras. Even if we understand whether these Fock spaces are free or cofree or irreducible, or whatever, there is a fundamental question on how to implement this information. Thus far I have found that the only systematic way is to grade by the number of W-ghosts, but this requires a fairly detailed knowledge of the Virasoro cohomology. So, what are we going to do???? P"

N to P on 14 October:

"I'm not sure whether it solves the fdeg problem, but I was thinking of trying to do something along the following lines: Grade by W-gho9t number so that d_0 becomes Vir-BRST operator. Then use (co)-freeness of

*M®M** to compute this guy. Cohom Is probably still free over remaining W generators, so that we may possibly be able to also compute next step in SS explicitly. Hopefully after this step we've killed everything so that there is no need to face *Tb* 77 term anymore. This might be the advantage of doing *H(M®M*)* over *H(F®F)*\ N"

On "Being Stuck"

P to N on 6 October:

"The problem with the fdeg is precisely where I have been stuck for few weeks now. The presence of this quartic term is also responsible for not being able to define a nilpotent operator for nf etc. ..."

A to P, N on 22 October:

"As for *W*I am completely stuck, and everyone else too I guess...."

N to sociologist on 7 January:

"What else? Ever more obstacles in our cohomology computation (or rather at the point where we have been stuck for a while)...."

N to sociologist on 8 February:

"... Yes, we are still stuck at a certain point... the paper will therefore be a collection of (unreadable) partial results____"

• *NOTES*

This study was conducted at the European Laboratory for Particle Physics, CERN, in Geneva, Switzerland. One of us (MM) has been accepted into the ranks of the CERN Theory Division, admitted to its seminars and supplied with desk and terminal, since September 1991. We want to thank the former head of the Theory Division, John Ellis, and the current head, Gabriele Veneziano, for their gracious 'admission' of sociologists into physics, and Peter Bouwknegt, Jim McCarthy, Krzysztof Pilch and Wolfgang Lerche for their tolerance and for sharing with us the intimacies of their work. We also thank the anonymous referees for valuable comments and suggestions, as well as the Deutsche Forschungsgemeinschaft for financing, and the Institute for Science and Technology Studies in Bielefeld for facilitating, this research.

1. Examples of laboratory studies are: Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction ofScientific Facts* (London & Beverly Hills, CA:

Sage, 1979); Karin Knorr Cedna, *The Manufacture ofKnowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford & New York: Pergamon Press, 1981, 2nd edn, 1984); Knorr Cetina, *Epistemic Cultures* (Cambridge, MA: Harvard University Press, 1997); Michael Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge & Kegan Paul, 1985); Sharon Traweek, *Beamtimes and Lifetimes: The World ofHigh Energy Physics* (Cambridge, MA: Harvard University Press, 1988); Michael Zenzen and Sal Restivo, 'The Mysterious Morphology of Immiscible Liquids: A Study of Scientific Practice', *Social Science Information*, Vol. 21 (1982), 447-73.

Sharon Traweek provided the first ethnographic study of physicists, comparing labs and communities in the United States and Japan. For further important recent studies of physics subfields from a contemporary history perspective see: Peter Galison, *How Experiments End* (Chicago, IL: The University of Chicago Press, 1987); and Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh: Edinburgh University Press, 1984). Physics procedures are investigated from an anthropological point of view in Martin H. Krieger, *Doing Physics: How Physicists Take Hold of the World* (Bloomington, IN: Indiana University Press, 1992).

2. The realm of string theory includes not only string theory itself but also closely related topics like conformal field theory (in fact, string theory is a two-dimensional conformal field theory), and superstring and quantum gravity theories,

3. Throughout the text, double quotes ("...") are used to refer to physicists' written technical terms, and to their spoken expressions and technical language. Single quotes ('...') denote our notions or concepts we want to highlight. A typewriter font is used for physicists' e-mail quotes; interview excerpts are printed in italics.

4. The question in what way these theoretical physicists' work differs from that of mathematicians (other than in terms of a difference of goals and motivation) is an interesting — and unresolved — issue. Suffice it to say that such differences exist, but they are of no concern to this paper.

5. Peter Gibbins, *Particles and Paradoxes* (Cambridge: Cambridge University Press, 1987); Bruce Gregory, *Inventing Reality: Physics as Language* (New York: Wiley, 1988); David Park, *The How and the Why* (Princeton, NJ: Princeton University Press, 1988); Mendel Sachs, *Einstein versus Bohr* (La Salle, IL: Open Court, 1988); Franco Selleri, *Quantum Paradoxes and Physical Reality* (Dordrecht, Boston, MA & London: Kluwer, 1990); Anthony Zee, *Fearful Symmetry: Tfie Search for Beauty in Modern Physics* (New York: Macmillan, 1989).

6. What is of help is work within the new sociology of science on mathematics, particularly David Bloor, *Knowledge and Social Imagery* (London: Routledge & Kegan Paul, 1976); Sal Restivo, *Mathematics in Society and History* (Dordrecht: Kluwer, 1992); Andrew Pickering, *The Mangle of Practice: Time, Agency and Science* (Chicago, IL: The University of Chicago Press, 1995), especially Chapter 4; Eric Livingston, *The Ethnomethodological Foundations of Mathematics* (London: Routledge & Kegan Paul, 1986); Livingston, *Making Sense of Ethnomethodology* (London: Routledge & Kegan Paul, 1987). The first three studies are based on historical materials. In its focus on practical work, Pickering's and Livingston's research is closest to the present study.

7. CERN's TH-Division hosts up to 150 theoretical physicists from all over

Europe and many other countries. The scientists conduct research in a wide spectrum of theoretical particle physics topics, ranging from the construction of fundamental theories and models involving the most modem mathematics, to work closely related to current and future experiments, concerning the analysis and prediction of results.

8. We put 'reading' within quotes because it involved, besides literal reading, discussion, recalculation (trying out or following what was explained through redoing computations), bringing in further information, and so on.

9. K. Knorr Cetina, 'Epistemic Cultures: Forms of Reason in Science', *History of Political Economy,* Vol. 23, No. 1 (1991), 105-22; Knorr Cetina (1997), op. cit. note 1.

10. See also Livingston, op. cit. note 6.

U.K. Knorr Cetina, 'The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science', in A. Pickering (ed.), *Science as Practice and Culture* (Chicago, EL & London: The University of Chicago Press, 1992), 113-38.

12. Compare H.M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge, MA: MIT Press, 1990).

13. Jacques Derrida, *Speech and Phenomena, and Other Essays on Husserl's Theory of Signs* (Evanston, IIL: Northwestern University Press, 1973); Derrida, *Of Grammatology* (Baltimore, MD: Johns Hopkins University Press, 1977); Derrida, *Writing and Difference* (London: Routledge & Kegan Paul, 1978). All three books were first published in French in 1967.

14. Barbara Johnson, *A World of Difference* (Baltimore, MD & London: Johns Hopkins University Press, 1987); Johnson, 'Writing', in Frank Lentricchia and Thomas McLaughlin (eds.), *Critical Terms for Literary Study* (Chicago, IL: The University of Chicago Press, 1990), 39-49; Christopher Norris, *Deconstruction, Theory and Practice* (London & New York: Routledge, 2nd edn, 1991).

15. John M. Ellis, *Against Deconstruction* (Princeton, NJ: Princeton University Press, 1989).

16. It would be impossible for Derrida to argue that meaning is inescapably deferred in writing, and simultaneously to insist that such is not the case in his own texts. See also Norris, op. cit. note 14, 136ff.

17. For an illustration in the case of a poem see Johnson (1990), op. cit. note 14, 44ff. A social scientific text would be seen, within a supplementary logic, simultaneously to bridge and to widen the gap between the described phenomena and what it says about them. The closest to deconstructionism in science sludies is the reflexive programme: see Steve Woolgar and Malcolm Ashmore, 'The Next Step: An Introduction to the Reflexive Project', in Woolgar (ed.), *Knowledge and Reflcxtvity* (London: Sage, 1988), 1-11.

18. Christian Artigues, Yolande Bellecave and Pierre-Henri Terracher, *MATH analyse, Ires S et E* (Paris: Hachette, 1991).

19. Johnson (1990), op. cit. note 14, 45.

20. Ibid.

21. See for example: Sylvia Scribner and Michael Cole, *The Psychology of Literacy* (Cambridge, MA: Harvard University Press, 1981); Jean Lave, *Cognition in Practice: Mind, Mathematics and Culture in Everyday Life* (Cambridge: Cambridge University Press, 1988); Lave and Etienne Wenger, *Situated Learning:*

Legitimate Peripheral Participation (Cambridge: Cambridge University Press, 1991).

22. Klaus Amann and Karin Knorr Cctina, 'Thinking Through Talk: An Ethnographic Study of a Molecular Biology Laboratory", *Knowledge and Society: Studies in the Sociology of Science Past and Present,* Vol. 8 (1989), 3-26.

23. Martina Merz, "'Nobody can force you when you are across the ocean" — Face to Face and E-mail Exchanges between Theoretical Physicists', in Jon Agar and Crosbie W. Smith (eds), *Making Space for Science* (London: Macmillan Press, forthcoming).

24. The models are 'simplistic' in the sense that they are only two-dimensional, instead of incorporating all four dimensions of 'real' space-time. Two-dimensional theories have less degrees of freedom and are easier to solve. Even these models, however, are mathematically non-trivial and of extreme structural complexity.

25. It is difficult to provide an intuitive understanding of what kind of object a BRST-operator is. We will therefore not make the attempt within the space of this paper.

26. To enhance the readability of the paper, we have simplified the physics 'language' as much as possible. For the interested reader a physicist's definition of the "physical states" follows:

> *"In the context of BRST-quantization, physical states are given by the cohomology of the BRST-operator A. The cohomology is defined by Ker A/Im A. So, in order to find the cohomology, you do not only have to solve the equation Ax = 0, but also identify solutions which differ by an 'exact' state, i.e. a state in the Image of A. This is essentially the requirement of gauge invariance. Two solutions which differ by an exact state represent the same physical state."*

27. BRST is the abbreviation of Becchi, Rouet, Stora and Tyutin.

28. We want to emphasize that the notion "computation" is used in different ways in theoretical physics. It can either refer to the act of solving a simple and small calculation, such as '$x^2 - Ax - 5 = 0$'; or it can refer to a much more complex set of procedures whose outcome is a physical quantity, and which may include the formulation and proof of a mathematical theorem. A cohomology computation is a problem of this kind. To avoid misunderstandings, it also needs to be mentioned that the notion of 'computation' does not *per se* refer to numerical calculations performed on a computer.

29. 'Pact with objects' is a notion somewhat analogous to the 'contrat naturel' recently developed by Michel Serres in *Le Contrat Naturel* (Paris: Editions François Bourin, 1990). For discussion of object relations see also Michel Callon, 'Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay', in John Law (ed.), *Power, Action and Belief: a New Sociology of Knowledge?* (London: Routledge & Kegan Paul, 1986), 196-229; and Bruno Latour and Jim Johnson, 'Mixing Humans with Non-Humans: Sociology of a Door-Opener', in Leigh Star (ed.), Special Issue on 'Sociology of Science', *Social Problems,* Vol. 35 (1988), 298-310.

30. Paul H. Ginsparg,' @xxx.lanl.gov — First Steps Toward Electronic Research Communication', *Los Alamos Science,* No. 22 (1994), 156-65.

31. The complete collection of e-mails (all in English) exchanged between N, P and A and related to the W-gravity computation was available to us for analysis.

However, without N acting as interpreter, the e-mails would have remained Double Dutch to us!

32. Peter Bouwknegt, Jim McCarthy and Krzysztof Pilch, 'Semi-infinite Cohomology of W-algebras*, *Letters in Mathematical Physics*, Vol. 29 (1993), 91-102.

33. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: The University of Chicago Press, 1962, 2nd edn, 1970), esp. section 5 of the Postscript, 198ff.

Martina Merz is a researcher affiliated to the Sociology Department at the University of Bielefeld. She is based at CERN, the European Institute for Particle Physics in Geneva, where she conducts a 'laboratory study'. Trained as a physicist, her main research interests now lie in the epistemic practices of theoretical and experimental physicists.

Karin Knorr Cetina is Professor of Sociology at the University of Bielefeld, a former member of the Institute of Advanced Studies at Princeton, and currently President of the Society for Social Studies of Science (4S). Her books include one of the first laboratory studies of science, *The Manufacture of Knowledge* (1981), *Advances in Sociai Theory and Methodology* (ed. with Aaron Cicourel, 1982), *Science Observed* {ed. with Michael Mulkay, 1983) and a comparative study of the epistemic machineries of two scientific fields (high-energy physics and molecular biology), *Epistemic Cultures* (Harvard University Press, forthcoming).

Authors' addresses: (M.M.) Theory Division, CERN, 1211 Geneva 23, Switzerland; and Institute for Science and Technology Studies, University of Bielefeld, PO Box 100131, D-33501 Bielefeld, Germany.
Fax: +41 22 767 3850. E-mail: Martina.Merz@cern.ch
(K.K.C.) Institute for Science and Technology Studies and Faculty of Sociology, University of Bielefeld, PO Box 100131, D-33501 Bielefeld, Germany.
Fax: +49 521 106 5844. E-mail: knorr@post.uni-bielefeld.de