

Max Weber's Concept of Rationality Some Relevant Concerns

The formal task of this enquiry will be the examining of Max Weber's concept of rationality insofar as it is assumed to be of central importance to the formulation of his sociological descriptions of world religions. This type of enquiry is, of course, consistent with a long tradition in sociology which examines such knowledge as has thus far been put forth in the field, and the procedures by which it was obtained (the sociology of knowledge and the area of 'methodology'). There are various warrants for such activities, such as to point out that, as natives of this society engaged in a particular social activity, science, sociologists' activities are part of the phenomena that they wish to study, and thus any knowledge about what sociologists do is knowledge about ordered activities in social settings and thus about the proper subject matter of the discipline. There is however a particularly dubious and prevalent warrant which I want to initially mention to avoid any such assumption being made about this enquiry. A positivistic sounding brand of it might go something like this: The 'successful' sciences like physics, in the growth of their enterprise, not only learned about physics but also learned how to learn. Thus, if one could discover the procedures employed to yield physicists their greatest successes, we might benefit from the knowledge they gained about knowledge in these processes and adopt some of their techniques for our enquiries. This approach is by no means limited to positivistic assumptions about the nature of the social world, but can be generalised to provide a valid warrant for any

endeavour to discover the exact techniques used to achieve a given scientific success, since such a success has simultaneously demonstrated the ability of those techniques to achieve wanted results in some general way. Thus, one attempts to learn Weber's method in order to apply it to gain analyses of similar scope and insight.

Such an approach might be of little consequence if used merely as a methodological rule of thumb, but it has apparently been strengthened in some quarters to serve as the *criterion* by which enquiry is conducted—to act so as to instruct investigators how they *should* proceed, insofar as they wish their efforts to be treated with respect. Partial results of this include what Dr John Horton has characterised as 'more and more sophisticated ways to find out less and less.' Needless to say, I don't intend this paper to be an attempt to make available another such 'respectable method.' Indeed, such an activity as I characterised sounds quite insane, but just in case the reader hasn't noticed this quality about it I will try to briefly bring it into relief before I proceed.

In the case of areas like physics there is little for me to do, since social scientists didn't in fact actually ascertain how this science's knowledge was obtained but instead employed *a posteriori* formulations of how it was obtained by logicians, philosophers and other theorists. With a familiarity with some of these formulations and two years' experience with basic theoretical and experimental physics I can confidently state that these

descriptions are generally false in very real senses, contrived and meaningless for anyone who would do effective physics, past or present. Isaac Newton's original formulation of the calculus, for example, was inconsistent, vague, and unsystematic enough to read, today, like mythology. Years of reformulation were needed in order for it to be possible to reinterpret (or reify) what, after all, he was 'really' getting at, so as to make his achievement describable as accomplished by adhering to notions of consistency, methodicalness, systematisation or clarity of thought. Also, in modern physics I'm only aware of one area (a field of quantum mechanics) where anyone has bothered to list a set of axioms so that the empirical statements in the field could be regarded as part of a *bona fide* mathematical system. In general one goes about doing effective physics and indeed mathematics by working with a motley assortment of particulars, special cases, problems, etc. In no way can one employ the schemata of philosophers of science, summarisers, abstracters, and so forth to do practical work effectively, since among their other defects such characterisations are too general and abstract to lead an investigator to the kinds of intuitive insights he needs to accomplish his task, or indeed to even permit practical calculations. If physicists and mathematicians can't use such things to do what they do, it is surely madness to assume social scientists can perform intendedly similar activities with them; yet it turns out that, indeed, they can—in the sense that much of the social scientist's activity consists of the kinds of things one can accomplish by applying these general formulae to his subject matter. In the general case let it suffice for me to point out that when someone studies a given subject matter, he only studies that subject

matter and does not study the study of that subject matter and consequently doesn't explicitly learn anything about studying the subject matter. If one wishes to develop ways to judge probable features of ways of doing things, such as their chances for being effective, then he should first initiate a discipline which investigates these ways of doing things as its *phenomena* instead of employing the procedure we have just been discussing.

Having laid a typical warrant for this enquiry to rest for the moment, let us proceed to consider what is undoubtedly the prevailing formula for accomplishing my task as it was stated. It might go something like this:

Obviously the examination of Max Weber's concept of rationality ought to begin with a precise determination of exactly what he meant by rational. Since he was relatively explicit on this point in his methodological essays, we ought to be able to arrive at a relatively clear definition of his concept as he intended it to be understood. Having clearly defined what he meant by rational, we can then proceed to deal with the concept's use in his descriptions of world religion, now equipped with what we need to know in order to test key issues like the correctness of his rational characterisations in specific cases, the methodological adequacy of the concept, and so forth.

One reason I say this approach is a prevailing one is that if one asks, at least, a graduate student in sociology whether it was correct, adequate, whatever, for Weber to characterise a given system of actions as rational he characteristically commences his reply by reminding you that 'this was the ideal type...' It shall presently be argued that this method of enquiry is

all wrong in principle, but for the present let it be noted that even if it were correct in principle, the programme I set forth can't be carried out as proposed without immediate modification. The modification is necessitated by certain practical historical difficulties prohibiting the prospect of treating what Weber intended by rational, as what he said he intended by it in his methodological essays. In certain cases such a treatment would be just plain wrong and in others at best questionable.

The practical difficulties centre around the context in which the only explicit exposition by Weber of his notion of rational is to be found. This explication is embedded in part one of his *Wirtschaft und Gesellschaft (Economy and Society)*. This work was intended as his contribution to a collaborative work on sociology and sociological economics, *Grundrisse der Sozialökonomik (Foundations of Sociological Economics)* which would amount to a virtual encyclopaedia of the social sciences (it comprised nine parts in fourteen volumes). Weber commenced working on his contributions to this series in 1913 and continued to add to it until the time of his death in 1920. His wife, Marianne, edited and published the work such as it was in 1922. As to the condition of the manuscript at the time of Weber's death I quote Ephraim Fischhoff (who?):

At the time of Weber's death, *Wirtschaft und Gesellschaft* was in an incomplete, unpolished and presumably preliminary state, except for the initial methodological section which he had completed and revised for publication. Some portions of the systematic sociology projected by Weber were never written, and others were broken off in the midst of the presentation. Throughout the text there are numerous

promissory references to subsequent discussions of points to which Weber never returned.

The massive biography by his widow, Marianne, a writer on social welfare problems, especially the woman question, provides a distinguished intellectual chronicle of her eminent husband. Yet his account, a primary source, together with Weber's own letters of information about his life and achievement, sheds no light on Weber's purposes and intentions in this work, or its principle of organisation. The text of the early editions is obviously corrupt in numerous places, and Weber's original footnotes have apparently been lost. Moreover, there were no subject headings or outlines; and since Weber left no clear directions as to the arrangement of the work, the material was arranged according to the decisions of the widow and her associates in the publishing venture. It has long been obvious that the organisation of the text is imperfect, but it was not clear whether this was due to the unfinished state of the manuscript or to other causes. For instance, it was realised that there are obvious discrepancies between the theoretical formulations of Chapter I of the German text and the treatment in subsequent chapters of concrete areas of social life, such as politics, law and religion.¹

Much of the material might have gone into properly organised notes, which the treatise lacks almost completely, in contrast to the richly-documented essays on the sociology of religion (now assembled in the *Gesammelte Aufsätze zur Religionssoziologie*). Indeed, *Wirtschaft und Gesellschaft* contains no citations or identifications whatsoever—either for quotations from religious

¹ Ephraim Fischhoff, *The Sociology of Religion*. Boston, MA: Beacon Press, 1963, page 282.

scriptures or for references to the writings of social scientists.²

The style in which the work was written evoked generally unanimous confessions from translators that their tasks necessarily involved more or less drastic alteration of the literal sense of the original text in order to make passages intelligible to the English reader at all. Max Rheinstein comments:

Little of this oratorical brilliancy can be found in Weber's writing, and least of all in his *Economy and Society*, where his style is heavy, involved and cumbersome. One of the reasons for this unpleasantness of his written style lies in his passion for accuracy. Every sentence had to be just right: quite particularly, he would not tolerate overgeneralisation. So every statement is narrowed by a qualifying statement, which in turn is qualified again and again, and the main proposition is combined with its qualifiers and sub-qualifiers in just one sentence, which often enough is of such monstrous length and involvement that even a German reader does not find it easy to unwind the thread and hunt for the predicated. The uncommon aspect of Weber's style is aggravated by his use of words newly and artificially coined by him. Most of these terms of art are precise and poignant; but they can not be understood without constantly keeping in mind the definitions by which he explains his linguistic creations or his highly technical use of words which also occur in the common language; and many of these definitions are involved enough and based upon terms which in turn can not be understood, or are likely to be misunderstood, without Weber's elaborate explanation. None of Weber's newly coined terms can, of course, be found in any German-English dictionary, and many of those terms which can be found are not used in their common

meaning. To make things worse, Weber died just before he could apply his finishing touch to his manuscript, of which considerable parts were left in the stage of a draft, jotted down to give expression to a course of thought, but without regard to beauty of style or even readability, and meant to be worked over and rephrased before publication.

The translators' work was thus beset with extraordinary difficulties. Many a sentence of Weber's had to be studied over and over again to unravel its structure and to discover its meaning. Completely literal translation is, of course, never possible in the case of any text. In Weber's case even that measure of literalness which is possible in most instances of proof could not be considered. His sentences had to be divided into new ones of reasonable length and structure, and English equivalents had to be found for his terms of art. As contrary to German, new English words can not ordinary be formed by simply joining together existing ones into new composites, circumscribing explanations had to be formed and formulated. Finally, the English text had to be readable with at least some measure of ease, although it could never be hoped to make it read like a piece of literature.³

Furthermore, the various published editions of the work have evoked a sea of controversy over charges of misrepresentation, incorrect texts, omissions, additions, and so forth, bringing forth historical investigations, resulting in a large literature on just this textual problem that we are exploring. I can't go into the details here but this following quote may serve to give the flavour of the issues:

To a far greater degree than Weber's famous Protestant essay, which

² Ephraim Fischhoff, *The Sociology of Religion*. Boston, MA: Beacon Press, 1963, page 285.

³ Max Rheinstein, *Max Weber on Law in Economy and Society*. Cambridge, MA: Harvard University Press, 1954, page xv – xvi.

experienced peculiar vicissitudes after it left the author's hand, in that its intention has often been misconstrued, his major work *Wirtschaft und Gesellschaft* has been bedevilled by various mischances, even in regard to the very state of the text. Here is a work of great importance and difficulty, left unfinished by the author's sudden death, the initial section of which was prepared for publication years after the main portion of the text, thereby creating certain theoretical problems caused by the juxtaposition of divergent perspectives. Moreover, the text, which was published posthumously by dint of the widow's great drive and dedication, underwent displacements that garbled the intention of the author, as Johannes F. Winckelmann, of whom we shall speak later, has now demonstrated on internal and other evidence. Even in Germany the imperfect state of the text and its difficulty have prevented *Wirtschaft und Gesellschaft* from exerting the influence a work of its scope might have been expected to exert—as witness the assertion of Carl Schmitt regarding the total lack of influence among legal scholars of that portion of the grand treatise dealing with the sociology of law. Finally, the fate of Weber's final system of sociology, in regard to English translation, has been less than fortunate because of piecemeal translation. For this great work has been rent into segments, and even now, after the appearance of the present volume, some sections of the huge treatise will remain untranslated.⁴

The section we are interested in is purported by Weber to be essentially a revision of a methodological essay which appeared in *Logos* in 1913.⁵ The descriptive chapters of *Economy and Society* were composed for the most part between 1911 and 1913 either before or concomitantly with the original essay in *Logos*. These include

his major treatise which we know by the translated title, *The Sociology of Religion*. Weber's original *Protestant Ethic* first appeared as an essay in 1905. Thus both of these major works were written *before* the definitions of rational at our disposal were published and before such definitions were formulated, even in their original form, in the case of the *Protestant Ethic*. Weber began his vast studies of religion and the economic ethics of the world religions in 1911, which were later to be collected and posthumously published in the three volumes of *Gesammelte Aufsätze sur Religionssoziologie*. What we know as 'The Religious Rejections of the World,' 'The Social Psychology of the World Religions,' *Ancient Judaism* (published as a series from 1917 to 1919), and large portions of *The Religion of China* and *The Religion of India*, first appeared as articles in the journal *Archiv für Sozialwissenschaft und Sozialforschung* roughly between 1916 and 1917. I needed to say 'large portions' for the last two entries because of the 'piecemeal translations' spoken of in one of the previous quotes. These just mentioned publications were for the most part all translated from the three volume collection of Weber's work I made reference to, and this collection 'collected' material from here, there and everywhere so that it is difficult to know the exact place in the chronology of Weber's life of much of this material. However there is evidence to the effect that he reworked the categories of social action which resulted in the revision of his methodological essay that we have available after 1918 and up until 1920. Moreover, it has been asserted that he didn't live to rework his religious descriptive material in the light of the

⁴ Ephraim Fischhoff, *op. cit.*, page *x* – *xii*.

⁵ Talcott Parsons, *The Theory of Social and Economic Organisation*. New York, NY: Free Press, 1964, page 87.

revised categories⁶ which would help to explain widely observed obvious discrepancies between the theoretical formulations of Chapter I of *Economy and Society* and the treatment in subsequent chapters of concrete areas of social life such as religion, discrepancies which persist despite all scholarly efforts, new improved editions, etc., designed to reconcile them.⁷

In summary then, the interpretation we have by Weber of rational in translations was taken from materials left seriously incomplete; materials whose organisation and intended purpose were not indicated by its author due to his demise, materials written in a manner necessitating the taking of drastic liberties with the text in order to render them intelligible to an English reader. Furthermore, much of the important work on religion was clearly accomplished before he came to formulate this interpretation; other material either was probably also written before or has a relation to the theoretical essay which is not definitely ascertainable by examining available sources. This latter fact has caused some to postulate that Weber's work with world religions actually *led* him to see the need to formulate a general sociological typology instead of the typology dictating the manner in which he described world religions.⁸ Clearly I am here pushing to make a case for the fallacious character of an assumed relation between the definitions of rational and their use in descriptions of religions; while doing so the deck has undoubtedly been stacked by omitting a discussion of the possible relations they could have, evidence for these, etc. However, I think it fair to claim that the facts

presented indicate that any investigation to verify such relations would be an historical mess and couldn't in any event validate something as simple as 'he intended by rational what he explicitly said he did except when he committed methodological "errors".'

Getting to some of the more formal difficulties with my defining procedure, it is clear that there must be an immediate qualification to the previously made suggestion that such a procedure is 'all wrong in principle.' Obviously the procedure can be accomplished as an academic activity in its own right as decades of term papers testify to. However it will be argued 'all wrong,' in that, this procedure could not serve as a successful means to certain ends (using the jargon of rationality) which nevertheless it frequently addresses itself to accomplishing. Thus, to roughly indicate tasks to which my initial arguments will not apply let me digress to catalogue a few orientations from which the study of Weber's rationality might be approached.

One such group of orientations would involve tasks for whose accomplishment one need only pay explicit attention to the *internal* features of the texts he read, tasks like judging the literary worth of Weber's works or wishing to be educated as to the general nature of sociological accounts. In the case of Dr Kuper's suggestion to regard various passages as generating hypotheses for an investigator, one need not be concerned with any questions of whether his descriptions were adequate for that society, or indeed whether there even existed a society which served as the subject matter for his descriptions. It is sufficient that his work suggests promising or interesting

⁶ Ephraim Fischhoff, *op. cit.*, page 285.

⁷ *ibid.*, page 285.

⁸ Max Rheinstein, *op. cit.*, page xx.

problems to test. Naturally its status as a purported sociological description of an actual society leads you to consult it rather than a science fiction novel but when and while you consult it in order to accomplish what you are trying to do you needn't concern yourself with the relations it has to any such society.

Then again, one might have in mind answering just such questions as, 'are his accounts adequate in some sense?' or 'are they correct in some way?' Such matters would typically involve the relation or connection the works considered had to a given society or to a set of documentary materials. Thus if someone informs me that Ronald Reagan won the election, if I wished to assess the correctness of this information I need merely compare his statement to certain independently available information to do so, not having to look into how or why he came to say what he did. I believe attempts to 'learn' his theory or method belong in this classification also, where by 'learn' I mean to indicate tasks where an individual seeks to acquire the skill of producing accounts similar to that of a given theorist in various situations. Students regularly are admonished to do just this for purposes of examination; not to just memorise passages from a given author but to be able to apply his techniques to the solution of problems, essay questions, and so forth, on exams. In such a situation one must be able to achieve a certain relation similar to the one accomplished by the author between one's accounts and that which the account is about—to view a phenomenon as Marx would have, etc., so he needs to be attentive to the relations of the author's accounts to that accounted for. However, he needn't be explicitly familiar with the process by which the author and now himself are able to produce such

accounts—he need only *somehow* come to be able to do it. As Alfred Schütz remarks, 'One needn't know how a telephone works in order to use it effectively.'

This leads us to my final broad classification, namely tasks which require the discovery of the process which the written texts stand as the product of; here's the solution what was the problem? The last remark already suggests that the 'how' in 'how did he come to do it?' can lead one in many directions. There is Schütz's distinction of 'in order to' and 'because' motives: Weber wrote accounts in the manner he did in order to generate hypotheses, defend Eastern religions, etc.; he wrote accounts in the manner he did because of his education and contact with German idealism. Then there is the historical approach: can I discover the procedure by which Weber and presumably anyone in possession of it could start with documentary materials as input and put out sociological descriptions of societies never observed? This is precisely the search for Weber's 'method' that we discussed earlier (note I did not reject this aim but only a particular warrant for undertaking it). Notice that for this entire variety of goals for studying Weber's rationality, the discovery of what he meant by rationality in arriving at a definition would be at best of dubious assistance and in the case of the last task absolutely useless and irrelevant. Not it might be objected at this point that a question like 'how did he do it?' could certainly be answered by discovering what he meant by his various notions of rational (assuming one could in fact find this out). Could I not, by knowing what he meant, be able to say how he came to call something rational or why he referred to some particular processes as rationalisation? Well, one

could indeed collaborate at great length on such questions but I claim that such activity would at best result in reproducing Weber's typical interpretations of his statements after they were produced which would not stand as a solution to the problem I had posed. To clarify this consider Alfred Schütz's way of treating meaning:

Meaning, as has been shown elsewhere, is not a quality inherent in certain experiences emerging within our stream of consciousness but the result of an interpretation of a past experience looked at from the present NOW with a reflective attitude. As long as I live in my acts, directed toward the objects of these acts, the acts do not have any meaning. They become meaningful if I grasp them as well-circumscribed experience of the past and, therefore, in retrospection. Only experience which can be recollected beyond their actuality and which can be questioned about their constitution are, therefore, subjectively meaningful.⁹

I believe he made an error of omission here in not proceeding to indicate that he wasn't speaking about just experiences but only about actuated ones, for interestingly enough in the case of rational action experiences *to be had* obtain meaning precisely in the way an actor interprets such experiences in order to plan his actions orientated to a given goal. Indeed, the interpretation in retrospect of the experiences actually had, depends in part on such prior meaning of these experiences, insofar as it permits recognition of success, failure, surprise, etc., in terms of what was expected *a priori*. Thus, there is a distinction to be made between two kinds of activity both involving the 'meaning' of things in the sense

discussed. In one activity you already have the 'meaning' of a yet to be produced series of experiences and your skill consists of manipulating your world so as to actually produce the experiences that will have meant what they were to mean before they happened. For example, most natives can arrange things so they will have insulted another native if they so choose. A definition of rationality will undoubtedly help to achieve this sort of skill with Weber, in that a knowledge of what he means by the term you can learn to 'apply' this part of this theory so that you could account for phenomena as rational or irrational where the particular meaning of such accounts will have been described by the definition of rational. In the other activity your skill consists in the production of the meaning of some events after you have actually experienced them—in the activity of interpreting them. Here again a definition of rational can not fail to aid one in such a task with Weber's descriptions. Given what he 'meant' by rational you can start with an account where he calls something rational and proceed to interpret it—to explain what he meant by his account, why he called it what he did, etc. But in both of these cases we don't have anything at all relevant to the solution of our problem: How (in my sense) did he produce his accounts? In the first case, as Schütz pointed out, one needn't know how the telephone works in order to use it and wouldn't need to know how Weber or the person himself manages to produce their accounts in order to *somehow* learn to produce them. In the second case someone's interpretation of an account which can only be given after the account is formulated is just not necessarily a description of the process by which the account was produced, and considering that such an interpretation has as a necessary

⁹ Maurice Natanson, *Alfred Schütz: Collected Papers*. The Hague: Martinus Nijhoff, 1962, page 210.

condition to its production that it be always *a posteriori*, there is reason to suspect that it could never coincide with such a production description. It is, after all, one thing to know the names of all the notes on your instrument and where they are located so that you can describe how any tune was played after it was played; it is quite another to learn to improvise on it, to learn to actually play. Or even more illustrative—we have all probably had the experience of replying to a friend's comment in some automatic way only for the two of you to then hear that you had just insulted him; however you came to say what you did, it wasn't by planning to hurt his feelings and choosing appropriate words to do so but it was only after you spoke that you could interpret your remark as an 'unintended insult' and attempt to apologise. Although, this seems like a simple point, the ambiguity of 'how' has constantly allowed explanations of meaning to stand as solutions to production problems like the one we have considered, just notice the abundance of explanations whose ability to tell you the 'psychological mechanisms' that caused you to do this or that depend for their cogency on the perpetuation of just this confusion.

There is a further argument to be made though it is that, granting the non-necessary character of an interpretation of a performed action and a description of how the action was accomplished coinciding, why insist that these two are distinct in our case? Could not my knowledge of how rational was defined enable me in some sense to say how Weber produced a given account—to describe why or how he chose the word 'rational' for such and such a system of actions? My answer to this is to admit that such a thing is of course possible but to state the conviction that

a definition doesn't provide the resources for such an achievement. The only way I could hope to convince the reader of this is to try to make him suspicious of what he knows when he knows a definition, a task I will now embark upon.

So what does one have when he has a definition? One popular notion is that definitions of a concept supply one with the particular features concrete objects must have if they are to be denoted by the word defined. We get a clear exposition of this from Frege:

A definition of a concept (of a possible predicate) must be complete; it must unambiguously determine, as regards any object, whether or not it falls under the concept (whether or not the predicate is truly assertible of it). Thus there must not be any object as regards which the definition leaves in doubt whether it falls under the concept: though for us men, with our defective knowledge, the question may not always be decidable. We may express this metaphorically as follows: the concept must have a sharp boundary. If we represent concepts in extension by areas on a plane, this is admittedly a picture that may be used only with caution, but here it can do us good service. To a concept without sharp boundary there would correspond an area that had not a sharp boundary-line all round, but in places just vaguely faded away into the background. This would not really be an area at all; and likewise a concept that is not sharply defined is wrongly termed a concept. e.g., would the sentence, 'any square root of 9 is odd' have a comprehensible sense at all if 'square root of 9' were not a concept with a sharp boundary? Has the question 'Are we still Christians?' really got a sense, if it indeterminate whom the predicate 'Christian' can truly be asserted of, and who must be refused it?¹⁰

¹⁰ Peter Geach and Max Black, *Translations from the Philosophical Writings of Gottlob*

In Frege's terms when we encounter a denoting phrase like 'the author of Waverley' or 'the rational action' there can be distinguished the phrase's reference, that which it stands as a sign of in the context where it is found, and its sense, the mode of presentation of the phrase or the information indicated as to how we find the reference of the phrase or the thought expressed by the phrase. Thus 'the author of Waverley' and 'Sir Walter Scott' would have the same reference but different senses. So would 'the square root of four' and 'the sum of one and one': both refer to the number two, but each indicates a different way of locating what is referred to. Frege extends his notions of sense and reference to declarative sentences, which express their sense (a thought) and denote The True or The False, with this terminology we can make two broad distinctions. 'Rational' will always occur in a context in Weber's descriptions. One issue concerns its reference, in that we would want to know the conditions under which he regards it correct to call a particular thing rational. With such knowledge at our disposal we could, as has been indicated, say why Weber chose to speak of 'The extensive rationalism of the priesthood'¹¹—not 'why' in the sense of why he chose this word instead of others which would involve production questions already mentioned, but 'why' in the sense that we know that the phenomena involved had the needed features for Weber to term it rational—we would regard this phrase as an example of the consistent manner in which he tagged phenomena with the term 'rational.' Another matter concerns being able to understand the *intended* sense of statements in which

rational occurs—the way rational is related to other words and meshes with them to express intended thoughts to anonymous audiences. This latter matter of 'sense' is just a heading for a whole series of complicated semantic problems but it is nevertheless a distinct area from that of reference, a fact insisted upon by Frege and modern linguists:

It is essential to distinguish between the meanings of morphemes and utterances, on the one hand, and on the other, the antecedents and consequences of specific acts of speech. The meanings of morphemes and of combinations of morphemes are, as has been said, associative ties between those morphemes and morpheme-combinations and things and situations, or types of things and situations, in the world around us. These semantic ties are more or less the same for all the speakers of a language. The antecedents and consequences of a specific act of speech can be quite different for a speaker and for his hearers.¹²

The question is now what can we learn about these and other matters by consulting Weber's definitions. The answer(s) will turn out to be more confusing than enlightening due to the special features of defining activities. Now the reason I brought up 'sense' was to call attention to the kind of thing that seems to be often offered by way of definition—some sort of description of the word which suffices to enable one to then 'understand' utterances in which the word is found and to use it himself. Thus, if I 'define' positron as a sub-atomic particle of anti-matter with a mass equal to that of an electron and a positive charge, I don't give you the criterion you need to tell whether something is a positron

Frege. Oxford: Basil Blackwell, 1960, page 159.

¹¹ Ephraim Fischhoff, *op. cit.*, page 24.

¹² Charles Hockett, *A Course in Modern Linguistics*. New York, NY: Macmillan, 1958, pages 139 – 140.

or not, because among other things you would need the terms further clarified, information about measurement, etc., to be able to actually spot positrons. However, what I do give you is what you need to understand, at least partially, future statements in which the term is embedded and to use the term yourself. Again if I define horse for you—relate it to other words in some way—you can immediately use it effectively all over the place without even having to see an actual horse. The fact that natives can assign a sensible meaning to a symbol and proceed to describe and talk about that meaning where it is irrelevant for these activities whether there is any concrete thing at all that the symbol can be said to be connected to, stand for, or indicate is, of course, crucial to Weber's enterprise as he must describe and analyse societies that are simply not around to observe. Now how might that restrict the kinds of things he can do? Strangely enough, the question seems to be a practical one, in that, in principle, you can be verbally told all you need to know to comprehend almost any type of text, after a fashion, with only prior direct experience with things tied to the words of your basic vocabulary. Philosophers, noting this, have often taken it into account in various metaphysical theories, speaking of the combination of simple ideas to form complex ones, etc. To indicate more clearly what I am talking about, consider an example: If it is desired to be able to read music scores one might begin by associating a sound of a given pitch with each symbol for the notes in a diatonic scale. Then you would become familiar with the rules of length, time signatures, and so forth. How a combination of notes on a score sheet could be read as a tune. It would make no difference that you have had no prior direct experience with the exact tune that a combination of notes,

regarded as one symbol, now stands for, for you to understand the symbols as the symbols for that tune. However, since you learned the diatonic, not the chromatic, scale, you would not have associated a sound with the symbol for a note like E flat, you could formally understand what was written as the notes of a tune, etc., and by knowledge of the meaning of the symbol for E flat could read it as the sound between D and E, but you could not in this case read the whole tune from the notes; there would be a recognition activity that could not be performed with these symbols due to the lack of direct experiences with a portion of the subject matter tied to the symbols. Now the matter of practicality enters thusly: In principle you can read the tune from any combination of notes each of which you know, but in fact this becomes a practical impossibility for scores of sufficient complexity. That is, if you try to read a sonata note by note you won't be able to do it. There will be so many notes that by the time you are on the second page the sound of the notes on the first page will have been forgotten so that you don't know how the first and second parts sound together—necessitating going back, it will take enormous amounts of time to figure out small parts of the piece and so forth. In order to be able to practically read such things you will have to learn a second order basic vocabulary by which I mean that certain *combinations* of notes will have to be automatically associated with phrases of sound, so that you can treat them as one symbol for a phrase you have direct experience with. Thus, whether a tune can be read from a score by someone with a basic vocabulary of associations of single sounds with single note symbols depends on the type of score which in turn depends on the type of music the score stands as the transcription of.

This example brings out only some of the more gross aspects of the type of issue I am discussing but it will hopefully indicate the general feel of the problem: What aspects of a society are such that they can be described in a manner understandable to a readership whose direct experience with the situations and things tied to symbols in their language includes no experience with that society? How does this limit what features of this society can be described? How does it govern the forms that descriptions of the society must take, in order to express what is intended?

Our musical example already gives us a way to talk about Frege's 'sense' of words. For one can introduce a new symbol by relating it to other symbols and not explicitly giving its relation to what it stands for. Thus, I can tell you that such and such a symbol stands for a given combination of notes and leave it at that, letting you fill in what tune those notes and thus this symbol refers to. Things clearly aren't going to be as banal as that though, as we will see as we get into the sense of a definition as 'what you have to know to understand a word and use it.'

To bring out my suspicions on this matter, I will use a very misleading but simultaneously very useful analogy. In a game of chess (my favourite source of analogies) what might 'defining' a given piece consist of? Well, first one would describe how it legally moved, then he would give its value with respect to the other pieces (indicated in chess by a number), finally some typical things it did would be illustrated—how it captures, checkmates in combination with another piece, how it pins or forks, its typical role in controlling the centre, etc. Now notice the situation in an actual game. The piece's value is

typically what was given, but its value at any particular time can be more than this or next to nothing depending on the particular game situation at hand. Furthermore, the definition of the piece will always furnish a description of the piece's role in very general terms—it is on such-and-such a square and can move here or there. It will also describe the reason for a move in terms of its legitimacy—he could move there because his piece was on square *X* prior to the enemy vacating square *Y* and thus he could legally go there due to how the piece moves, etc. However, such descriptions can only provide the skeleton of the significance of that move. In the definition a few typical uses were given in bare bones—how it pins for instance. Now the piece is indeed pinning but that means here that it is preventing a checkmate by stopping a move. Also, the piece can 'do' many more things than the definition illustrated; for what it does consist of the relations it can have to various pieces (the relevant number of those depending on the position) in specific game situations. These relations, all are based on the definitions of how things move and are large indeed in number and complexity. Thus, what is needed to see why a given move was made at a certain point and what the piece is doing in the game upon the completion of that move is a complete analysis of that game situation. Knowing definitions of all the pieces and the game rules will enable one to perform such an *analysis*, the form of which will depend on his previous random (because he probably hasn't played a representative set of past games) experience. Thus, the definitions allow one to produce a meaning for a move but, they don't allow producing the intended meaning of the person making the move. Now this is very important. Defining the pieces can

rightly be seen by most as something that everyone does pretty much the same. This is so, in part, because the personal habits and judgements of definers are not included in the defining activity. Therefore, even if the person making the moves you are watching were the one who defined the pieces to you, you couldn't have thereby learned how he typically chooses his moves, how he tends to evaluate game situations, so you are in no necessarily preferred position to see what his intended meaning for a given note is, in terms of his plans, evaluations, and so forth.

In what way do we have similar situations in verbal definitions? First we must dispel the notion brought forth in the first quote from Frege and in the music analogy. This is that defining a new symbol consists, at least ideally, of equating it to a bunch of equivalent ones already known, so that the thing defined becomes nothing but a shorthand for the known things. We find examples of this in certain technical languages designed for specific purposes. For example, in the language of the first order predicate calculus consisting of proposition letters p_1, p_2, \dots, p_n , — (they stand for declarative sentences), parentheses (.), the negation sign \neg (read 'it is not the case that'), and the implies connective \rightarrow ($p_1 \rightarrow p_2$ reads 'if p_1 then p_2 '), we can add a new symbol to this language, the symbol \vee (read 'or'), and say that $p_1 \vee p_2$ (p_1 or p_2) means nothing more than $\neg p_1 \rightarrow p_2$ (if it is not the case that p_1 then p_2). The two expressions $p_1 \vee p_2$ and $\neg p_1 \rightarrow p_2$ are equivalent in the sense that in the semantics or model theory of this language one expression is true if and only if the other is true and in the syntax or proof theory of the language one expression can be proved if and only if the other can. Thus, they

are equivalent *only for these purposes* (truth and provability). Notice even here we can only make the weak claim of equivalence and not assert the expressions are equal or the same. For even in 'hard' disciplines like logic notions like defining a symbol as something that stands for the same thing as another, so that the two expressions are equally useable, causes difficulties with respect to doing what the discipline wants to do. We know that $2 + 2$ stands for the same thing as 4. So anything about $2 + 2$ must be true about 4, right? Wrong. Consider the true statement, Jim wants to know if $2 + 2 = 4$. It is true about $2 + 2$ that Jim wants to know if it equals 4. It is false about 4, that Jim wants to know if it equals 4—he already knows that. Again, linguists testify to numerous other ways in which things that are 'the same' or 'equivalent' for the sake of definition just aren't so with respect to other activities carried out in natural languages:

Later, and for the analyst, there is another method: he can be told—in a language or part of a language he already knows—what a newly-observed form means. This second way is often very unsatisfactory. One can ask a Russian who knows some English what the Russian word / drúk / means, and the answer will be 'friend.' This is roughly true, but the precise social circumstances under which a Russian calls another person /drúk/ are by no means the same as those under which we call someone a friend. The meaning of /drúk/, or of friend, for a speaker of the language involved, is the result of all his past experiences with that word.¹³

Bilingual dictionaries and easy word-by-word translations are inevitably misleading; the shortcut of asking what a form means must ultimately be supplemented by active participation in

¹³ *ibid.*, page 141.

the life of the community that speaks the language. This, of course, is one of the major reasons why semantic analysis is so difficult.¹⁴

I might egotistically mention here that the main thrust of the semantic analysis spoken of in the above quote, thus far marred with difficulties and lack of success, has been precisely to attempt what I am arguing against: the deduction of what a speaker means by a combination of words on an actual occasion of their use from a set of definitions of each of these words in a lexicon.

At any rate, it is safe to say that a verbally defined term, as used in a natural language, is almost never a notational substitute for the words used to define it. The defined word may perform tasks that these words can not alternatively perform, be applied in a manner that the words are not and the same holds the other way around.

However, what if we choose, for example, Frege's notion of the proper definition of a concept as an 'ideal type' with which to describe actual verbal definitions? This brings up an important side to pursue which certainly deserves a page or two of comment. My previous suggestion seems to take the line that Weber himself seemed to indicate, for his use of the rational typology as an ideal:

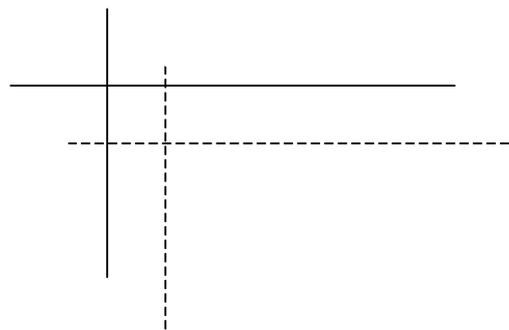
For the purposes of a typological scientific analysis, it is convenient to treat all irrational, affectually determined elements of behaviour as factors of derivation from a conceptually pure type of rational action.¹⁵

Indeed it was asserted in the seminar and apparently by Weber also that we

needn't be concerned with whether, when he calls an action rational, he was correct to do so or whether that action was really or actually rational:

Meaning may be of two kinds. The term may refer first to the actual existing meaning in the given concrete case of a particular actor, or to the average or approximate meaning attributable to a given plurality of actors; or secondly, to the theoretically conceived pure type of subjective meaning attributed to the hypothetical actor or actors in a given type of action. In no case does it refer to an objectively correct meaning or one which is true in some metaphysical sense.¹⁶

The claim would be that his typology is merely a choice of the language he will use to describe whatsoever is seen so that if he calls an action irrational and the native actor who performed it calls it rational, they do not disagree as to what actually happened, but only have different reference points from which to formulate the description of what happened. Just like if you describe the position of a point with, first, a set of Cartesian co-ordinates, and then these co-ordinates moved one unit to the side and two units down, you get two different descriptions of where the point is but there is no disagreement by virtue of this as to where it actually is:



¹⁴ *ibid.*, page 141.

¹⁵ Talcott Parsons, *op. cit.*, page 92.

¹⁶ *ibid.*, page 89.

In Weber's case, he asserted that his purposes were behind the choice of the rational descriptive apparatus. Part of the lure of this typology was undoubtedly, as Talcott Parsons points out, his ability, as a scientist with a stock of verified knowledge, to regularly pick out particular actions and results and categories this as a means, that as an intended end ———. Without this skill, of course, the typology would be inapplicable and thus useless. But notice something extraordinary about this skill—it defines reality! That is, any system of actions of single action results in *something*: namely, the direct antecedents of these actions, whatever they may be. Thus, with only the notions of means and ends available one could represent all actions as rational by simply categorising the actions witnessed as the means toward accomplishing whatever these actions resulted in, as the ends. Various unconscious theories of intentions rely on just such a possibility—witness the idea of the Freudian slip while talking and how you in some sense must have meant what you accidentally said. But, no—this would never do. Weber's skill in applying the means-ends categorising then must consist of his ability to *select* certain actions as the ones representing the means, and determine by his own knowledge what ends their means are typically, and thus for his purposes, being sought—ends not necessarily identical with the observed results of the means and thus not necessarily there to be detected at all. Thus, he is involved in treating observable actions as not there for his purposes insofar as he selects actions that are meaningful and are 'means;' he is involved in treating what is observably there when he selects the ends that the means are directed to. In short, his activity doesn't consist of

using his typology to determine whatever is there, from his point of view; his activity demands that he consult whatever is there *in order to discover* what is there from his point of view. The examples of this constraint on activities are almost too numerous to mention, it can be seen happening in almost all activities with notation systems designed to reproduce a setting that is notated. The author's investigation of dance notation this summer revealed how in order to notate a dance to be seen one had to get straight before he even saw the dancer do the dance which movements were merely interpretation, irrelevancies (like blinking one's eyes), details, and so forth. Then we should change the analogy of the reference point a bit to conform to these considerations. Weber isn't choosing a reference point so much as he is choosing a reference *system*—Cartesian co-ordinates, as opposed to cylindrical co-ordinates, or polar co-ordinates. For we can see how two people who are using notions of means and ends in similar ways can come up with alternate descriptions to the same action by viewing the action from different reference *points*—one has a knowledge that *A* has been proved a means to *B*, the other thinks *A* and *B* are unrelated, one sees the action directly, the other only sees a film of it, etc. Now the descriptions are different but the features of the descriptions are identical since both are using the same system. But just as a description based on the choice of a two-axis Cartesian co-ordinate system necessarily posits information about the location of a point—namely, its position with respect to the third dimension, so the choice of a rational typology limits the amount of phenomena that can be described, insofar as it simply isn't there with respect to that system. Thus, if Weber can't arrange actions into

ones deviating from or conforming to means to ends, with choices and so forth, he can't describe it with his typology, a point he realised. If we were to treat verbal definitions as deviating from or conforming to the ideal of Frege's proper definition, various social phenomena occurring in defining would escape our notice as linguists demonstrate to us. It might be objected, though, that this is news from nowhere in that Weber admitted that it wasn't his business to describe concrete actions in their full detail and individuality, but that it was his task to describe only those features relevant to his scientific interests. But this is just the point in that subjectively meaningful action he admits is relevant to his interests and it is precisely such action that his system makes unavailable to him. It is with this in mind that I advocate anyone who would study Weber's use of rationality to look to his studies to see what he might mean by his definitions and not the reverse.

I must now regretfully ask the reader to turn back to page 161 and recall the chess game description, so he will remember what I want to be doing as I finish off the discussion of definitions. The only result obtained from the previous digression relevant to this task is that, like defining a move, verbal definitions could reasonably be regarded as giving one in some way what he has to know to understand and use a word.

Now let's proceed to the idea of a legal move. Members have to be told where, that is in what verbal contexts, the defined word can be located so as to make sense. Much of this information is not explicitly given in the definition as there is no need; it is built into the description of what the word means. For when you explain that a word

names *X*, or that a word indicates the quality *Y*, you can be doing nothing else but indicating that one is used like a noun and the other like an adjective. You are telling how the words are related to things and situations? Well, perhaps, but notice that the way that 'chair' names something and the way that 'behaviourism' does are sufficiently different to make any claim of the similarity in the way they are both related to what they stand for, not too convincing. The same can be said for the way 'red' indicates a quality and the way 'dualistic' does.

We also have the situation with verbal defining of giving a few typical things a word does (means), never a full picture of what it does in an actual situation of use, but always a demonstration in bare bones, like the chess example where one is shown how a piece pins but not a full meaning of pinning in an actual game:

In a like sense, 'go' may be said to have a similar kind of meaning, since it refers (among other things) to a motion of an object. But it is difficult, even fruitless, to attempt to specify exactly what motions are indicated. Compare 'He goes home. John goes with Mary.' And 'The watch goes.' Indeed, it may be used of a quite immobile subject as in 'The road goes to Weston.' These variations of reference to the outside world can in part be accounted for by the assumption that a speaker of English has learned to structure content in such a way as to bring these diverse elements of experience together into a single category. The meaning of 'go' rests in the interrelationship between the morpheme /gow/ and the point within the content system where these things are brought together.¹⁷

¹⁷ H.A. Gleason, Jr., *An Introduction to Descriptive Linguistics*. New York, NY: Holt, Rinehart and Winston, 1955, page 54.

Further the defining activity is by nature finite and only certain meanings of the word are given; selected with respect to their typicality:

A final lay assumption about ‘words,’ which does not actually hold either for the layman’s words or for ours, in that they should always have some sort of meaning of their own, predictable in terms of their structure if they are larger than morphemes, and reasonably constant from one occurrence to another.¹⁸

The text from which this quote was extracted then goes on to discuss expressions like French fries, white paper, northern halibut—where knowing the meaning of French and fries will not yield one the meaning of French fries. But one may protest that these matters are taken into account in dictionaries all the time when defining. Several nonconnected senses are always given for a word based just on examining its meaning in just such situations as has been indicated by the previous examples. Thus, since we have ‘The room is cool’ and ‘He is a cool guy,’ we get an informal meaning for ‘cool.’ However, the *totality* of meanings of ‘cool’ consists of all the metaphorical, literal, invented, comical ways I choose to find to use it in—as long as it is put in the position of an adjective so you can formally see its function in a sentence—the cool pawn, Cool cigarette commercial, and cool personality are all added meanings of ‘cool.’ One couldn’t list all such because the number of different ways I can find to relate the word to all the other words in the language, like the number of specific roles a piece can have in particular games are too numerous to make a list practical. Just as in the game, verbal definitions usually indicate only typical meanings,

as has been noted. Weber didn’t analyse the exact sense of ‘rational’ in every context he used it in in his books to arrive at his definition; he probably didn’t even analyse one, but simply reflected on what he typically wished it to mean. For there are few who would disagree that he missed several of his own uses in his definitions.

Verbal definitions usually provide one with a skeleton analysis of a word’s use in a given context in terms of whether a proper characterisation was made or not (did it mean that or not?), consistent or correct use of the term, etc. As in the game analogy, the personal habits of the definer as to how he typically chooses the word he does instead of other possible ones and how he interprets the particular meaning of a word in specific contexts, are not given in the defining activity. Thus, a description of the intended meaning of the word as used in a given situation by the definer is based on the definition of the word, and so has been noted in a previous quote, on the entire past history that the person doing the describing has had with the word. So that, just as in chess, depending on the habits of the definer and the nature of the particular situation, his intentions in using the word might be so clear as to be public knowledge or they may be unavailable to most other witnesses, all of which will nevertheless have some interpretation as to these intentions.

The clear specification of the relation a defined term has to things and situations, its reference to use Frege’s term, is analogous to giving the piece’s value in certain respects. In both cases this information is not usually needed except as a rule of thumb, since the reference of the word, like the value of the piece, changes with the context and if a speaker knows the rules of the language game, he can usually at any

¹⁸ Charles Hockett, *op. cit.*, page 171.

given time deduce this from the specific context:

Up to three words in this sentence may be unfamiliar (depending on the reader's background, of course). But the sentence structure is absolutely clear to any native speaker of English. The structural clues which might be yielded by the unfamiliar words are not needed; there are enough others. Faced with a sentence of this sort, one might go to a dictionary for the meanings of the words. This is seldom done. Instead, the meanings are discovered from the context.¹⁹

One of the most serious weaknesses in American language teaching has been just here. Students are told in school that they must be absolutely sure of the meaning of every word. They learn rather well how to use a dictionary, but very poorly how to guess. In effect, they are being prohibited from learning to read German as the Germans do. Dictionaries are useful, of course, but they must be used properly. That means first guess, and then look it up. But the guess must be based on the sentence structure.²⁰

Thus we can see that when Weber consulted his work to see how he might define rational to an unknown audience in an article, there is reason to believe that he did not attempt, for specific texts, to perform what John Wisdom refers to as an analysis²¹ to discover what he meant by what he meant, but instead attempted a description of such texts—a mere explication of what he meant which would involve him in all the specialised defining practices we have discussed. Therefore, it is hoped, I have presented enough relevant considerations to warrant postulating

that what he ended up with in his theoretical essay would not aid us either in discovering the methods he used to produce his descriptions involving rational action or in helping us decide what his specific intended meaning would have been for a passage which contained the term rational or some variation of it.

Now after all the discussions of the dangers involved in trying to define rational by consulting Weber's methodological essay, what are we going to do? Of course, we are going to define rational by consulting his methodological essay. The reason for it, is that the process opens up a set of further issues.

First another reason why we need to be concerned with if an actor 'really' sees his action as rational. Weber intends to set up a model of action for, among other reasons, describing the typical motives of an actor performing a given action—for providing a subjective interpretation of an action. Also, he intends that his model have the advantage of 'clarity' and 'unambiguity.' If that is our task then if we don't investigate the manner in which natives subjectively see their actions as rational then we must at least *specify* how it happens for the model we want to set up, which includes an ideal actor's ideal intentions. Another condition Weber wants satisfied is that an actual native must be able to perform like the puppet we set up so that our notion of rational won't do violence to our intuition. Can such a task be accomplished? I will claim not—that Weber certainly doesn't do it, that it presently can't be done without equivocating and that the manner the rational model is employed is just as obscure, sociologically, as the phenomena it purports to make understandable. Let's start with

¹⁹ H.A. Gleason, Jr., *op. cit.*, page 171.

²⁰ *ibid.*, page 178.

²¹ John Wisdom, *Interpretation and Analysis*. Carter Lane, DC: Broadway House, 1971, page 1.

Weber's definition of the most clear of his rational types:

Action is rationally orientated to a system of discrete individual ends (*zweckrational*) when the end, the means, and the secondary results are all rationally taken into account and weighed. This involves rational consideration of alternative means to the end, of the relations of the end to other prospective results of employment of any given means, and finally of the relative importance of different possible ends.²²

With this in mind, what might our puppet look like minimally? Do we refer just to a type of action, independent of ascribed intentions of the puppet so he can act rationally when he doesn't know he is doing so? The definition suggests rejecting this possibility which immediately suggests another kind of rationality Weber uses. When a group co-ordinates its actions so as to effectively achieve some goal, the actions of the individuals are generally not governed by conscious intentions to achieve that goal but by habitual conduct, internalised norms, etc.—although each member might have knowledge that he is a part of an effort to use *X* to achieve *Y* (or he might not). Thus, the group's rational behaviour might be just a type of *activity* getting done independently of rational interventions of its members—as a matter of fact, for the group to act rationally the individuals might have to act irrationally.

What might our task be to specify theoretically how our puppet would act on a given real occasion if he were rational, *before* that occasion and independent of it? We would minimally have to specify for him some individual goals and some means, provide procedures for him to

eventually decide to undertake, say, *X* to accomplish *Y*, and postulate that the puppet deliberately chose this course of action based on the knowledge that *X* is a means to *Y* in some way. Could we get up such a calculation? Well, let's intuitively investigate what would happen. The time starts and someone has some initial goals, but these are determined by his whole individual biography, not just the situation he finds himself in. so how should we specify some relevant goals for him in a formal way knowing only the immediate environment where the action is to take place? Then again we don't usually base doing *X* to get *Y* on *knowledge* that *X* gets *Y* (scientific). Each person draws from his *random experience*, he reads it in a book, someone told him it, once it happened that *X* got *Y*, and so forth. This procedure is a making do one based on uncertainty. So we couldn't specify that *X* yields *Y* for a native (ideal one) by scientific study that proves *X* produces *Y*. How could this random experience be approximated so we have a model of what a member thinks causes what—as long as he thinks *X* gets *Y* he is rational in doing *X* to achieve *Y*. then there is the problem of the time process. One doesn't perform a course of action like he flicks a switch. One can only imagine an accomplished course of action before you begin it. Thus, it would be irrational to initially start course of action *A* to achieve *B* and when the actual process unfolded it became clear that *B* was no longer an end or you couldn't *accomplish* *A*, or you saw that *A* wasn't going to evoke *B*, to dogmatically stick to your original planned action. It seems rational to change means, ends, etc., as you get more and more information as the process goes on. Thus, if we gave a procedure where the puppet just chooses and attempts to carry out a

²² Talcott Parsons, *op. cit.*, page 117.

course of action no matter what happens later, this might be irrational. How could this continuous feedback aspect of rational action be approximated in our puppet? Then again when our timer started any unaccomplished course of action to get a certain end, would stand as a sought goal in its own right—how can one specify theoretically before an activity starts which sets of intended future acts are means and which ends? It is only after one accomplishes a course of action that he can see what it did and find it was a means to such and such a goal. As a matter of fact, in the *Protestant Ethic*, Weber already has accomplished acts which he is describing and due to the lack of clarity of how the model theoretically behaves—as already suggested—we get the equivocality that we don't know if the accomplished acts described as a means description indicates approximately what the action already done can be interpreted to amount to—lacking a clear model we can go both ways. In summary, then, I suspect that the descriptive apparatus we discussed here is essentially a technique whose successful application demands that it remain vague and that it be employed after the fact. Any attempt to treat it like a model in physics will probably result in the loss of its descriptive power and an unsatisfactory such model in any event.